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Section I.—*Introductory.*

B

of such rays. That there are *dark* lines in the solar spectrum reveals to us the fact that the surface of the atmosphere is cooler than the luminous region beneath. But we may go further than this. Most gases are colourless; in other words, they do not scatter rays incident on them. Neither do they reflect light from their surface. It follows, then, from the laws which regulate the exchange of heat, that where such a gas is of sufficient thickness to be opaque in reference to any particular ray, it will send forth the most intense ray of that particular refrangibility which it is possible for a body of the temperature of the gas to emit. Hence that there are lines in the solar spectrum of very different intensities is an evidence to us that the surfaces of the atmospheres from which they have their source are at different temperatures. It thus appears, upon a rough view, that the upper layers of the atmospheres of sodium, magnesium, and hydrogen are cooler than those of iron and calcium, and that these again are cooler than the upper layers of the atmospheres of nickel, cobalt, copper, and zinc. In this, then, we have evidence both that the atmospheres of the several gases extend to different heights, and that the temperature increases from the surface of the solar atmosphere downwards. Again, such facts as that some of the iron lines are less dark than others in their neighbourhood, that some of the copper lines are not noticeable, prove that even before descending sufficiently far to have passed through a stratum of these gases thick enough to be opaque to these rays, we have already arrived at a sensibly higher temperature. This temperature, in the case of some of the lines of cobalt, copper, and zinc, appears to approach, if it does not pass beyond, the temperature of the luminous clouds.

3. Let us now direct our attention to the darker nucleus which lies within the photosphere. It is known that a body, when surrounded on all sides by an opaque envelope of others at its own temperature, will reflect some of the incident light, if its surface be in any degree polished, and if the body be not wholly transparent or wholly black. It will scatter others of the incident rays if either its surface or its substance be such as would not be wholly invisible if exposed to light brighter than that corresponding to its temperature. And, lastly, it will emit rays in virtue of its own temperature of such kinds and in such quantities that, along with those transmitted, reflected, and scattered, they will make up a total which is definite for each temperature. This is one of the established laws of the exchange of heat. It follows from this that a body thick enough to be opaque which emits much more feebly than others at a given temperature must reflect incident rays better, or scatter them more copiously. It cannot in an eminent degree do both. Unless the dark body of the sun is cooler than the photosphere it is therefore, at least in those places which are exposed to us as spots, either such that it scatters incident light abundantly, or it has a highly reflecting surface.

4. Before pursuing further our inquiry into the nature of the central body of the sun, it will be convenient to enter upon the discussion of the

phenomena of the luminous clouds ; and it will avoid confusion in this part of our investigation to adopt provisionally the definite hypothesis that the central body of the sun is an opaque ocean with a highly reflecting surface—such a surface as an untarnished white molten alloy would present. We shall run no risk of error in doing this if we afterwards carefully reexamine such parts of our inquiry into the phenomena of the clouds as would be affected by substituting for this hypothesis any other that is admissible.

5. [We shall also assume that the photosphere is not itself the origin of the heat which it disperses abroad, but draws it from the adjoining regions. No doubt if chemical action could be the source of solar heat, the photosphere might be its seat, and resemble the luminous part of a candle-flame. In this case the opaque regions within might be cooler than the photosphere, provided the photosphere were so translucent as to allow a sufficient radiation through it from the parts within to the open sky. But the photosphere to be thus translucent should of necessity be at a far higher temperature than an equally bright body with a perfectly radiating surface. And almost to this intense heat it would raise a great extent of the outer atmosphere, which, being eminently transparent, is but imperfectly fitted to moderate the heat it receives from contact with the photosphere. Hence we should expect to see conspicuous bright lines in the solar spectrum, which, however, we do not find. Moreover, the amount of the sun's radiation appears to be decisive against our attributing it to chemical action.—September 1868.]

6. Let us consider what would happen if the photosphere were away, and nothing but an atmosphere of fixed gases in contact with an intensely heated molten sphere. To simplify our conceptions, let us conceive the molten mass to have a core which is maintained at a constant temperature, to have a surface reflecting perfectly, and to be enveloped by an extensive atmosphere of one fixed gas, which, for further simplicity, we shall suppose gives a spectrum of invariable lines. The atmosphere is supposed to be extensive enough to render the change of temperature throughout it so gradual that there are no currents of convection. The under surface of this atmosphere would be raised by direct contact to the same temperature as the polished surface of the dark body within. The temperature would very slowly decrease in passing outwards from the core, first through the molten ocean, and then through the atmosphere, until that upper layer of the atmosphere was reached which alone can emit heat into space. Through the thickness of this outer stratum the temperature would rapidly fall, the whole escape of heat from the system taking place exclusively from it, in the form of undulations of the ether of those particular wave-lengths which the gas constituting the atmosphere can excite. Such is a picture of what would ultimately become the permanent state of such a system as we have imagined.

7. Let us now suppose the surface of the ocean to lose part of its reflecting-power, and to become such an imperfect mirror as is possible with the bodies we know to exist. The surface of the ocean will at once begin

moderately to radiate heat, most of which will escape into space. It will thus become a surface of minimum temperature, cooler than the depths of the ocean within, and also for a time than the adjoining parts of the atmosphere without—just as the surface of the ground becomes a surface of minimum temperature while dew is falling. This surface of minimum temperature will draw heat from the warmer bodies on both sides of it, and will thus tend to cool both the atmosphere above and the ocean beneath.

8. Let us next conceive a particle with a highly emitting surface, and of the same temperature as the surrounding medium, situated in the atmosphere a short distance above the ocean. Such a body, owing to its lavish radiation, would quickly fall in temperature far below the bodies around it. If, however, these latter can supply it with heat so fast as to prevent its reduced temperature sinking below the temperature of brilliant incandescence, it will continue a magnificent spectacle amid the comparative darkness around. Let us now suppose that we have in the neighbourhood of this particle a vapour such that it is gaseous at the ardent temperature of the surrounding medium, but that it is precipitated either as a smoke or mist by the coolness of the radiating particle; it will, the instant it is so precipitated, begin itself to radiate copiously, and so will tend to maintain the reduced temperature which is the condition of its continuing to blaze forth. The process is the inverse of what takes place in setting a flame alight, and is strictly analogous to it*. If the vapour be of great depth, the upper parts, when precipitated in luminous cloud, will protect the rest of the vapour from that free radiation towards the sky which is the necessary condition of the phenomenon. If the blaze had been first communicated to a part below the outer layer, it would at first form a cloud in that situation—the radiation from the upper side of this cloud would be least obstructed—the blaze would therefore tend to spread outwards, and would no doubt do so much more swiftly than the cloud could subside. The blaze would therefore soon fly to the upper† surface of the vapour, where alone it could establish itself permanently. Such, then, appear to be the luminous clouds.

9. Since the great escape of heat takes place from the photosphere, it must be cooler than the contiguous parts of the regions within or of the atmosphere without. And the lowest temperature to which either of these could possibly fall is evidently the reduced temperature of the clouds, a mi-

* It seems not improbable that as there are substances which will take fire spontaneously and, when they have done so, will maintain a temperature of ignition—a far higher temperature than they had before—so perhaps there may be vapours in the solar atmosphere capable of spontaneously forming a molecule of liquid or of solid at their own high temperature, which would have but a momentary existence were it not that its instantly beginning to radiate both renders its new state fixed and sets the whole neighbourhood ablaze.

† It will be shown further on, that a *trace* of the vapour which forms the clouds may, and probably does, extend far beyond them. But the clouds are at the boundary of the region in which there are *large* quantities of the vapour.

nimum which would only be possible under the condition that the luminous stratum was an absolute screen stopping every ray of heat coming from beyond it, and also reducing the entire of the intermingled atmosphere of fixed gases quite to its own low temperature. The former of these conditions, especially, seems improbable when we bear in mind that the film in which cloud can form must be so transparent as to admit of the abundant radiation towards the sky which we have found to be essential. We shall be able to treat this subject further on with more precision; but, in the meantime, we are clearly entitled provisionally to regard the film of clouds as colder than the regions on either side of it.

10. Let us now consider more attentively the thermal conditions of the photosphere and of the subjacent regions. In doing this it is necessary to distinguish that part of the condensed vapour from which there is so abundant a radiation outwards as would enable vapour in that region to pass into cloud, from such other parts of the condensed vapours as are too much screened from the sky to allow any more cloud to form. It will accordingly be convenient henceforth to restrict the word *cloud* to the former, and to use such words as *mist* or *rain* when we have occasion to speak of condensed vapour in a lower situation. Now, in the first place, if from any cause a part of the vapour fitted to produce luminous clouds rose above the general level and became detached, it would form a cloud, which by its own weight, and by the coolness it would impart to the fixed gases interspersed through it, would gradually settle down till it became merged in the general luminous stratum. This behaviour would be hastened by a sudden change of the density of the solar atmosphere, which, as we shall find hereafter, takes place at the boundary of the photosphere.

11. The clouds, though of a thickness small when compared with the enormous extent of the atmosphere of the sun, may nevertheless be of considerable depth; but they can in no place be of such a density and thickness as to be opaque, since no part of the stratum can come into existence from which there is not a sufficiently free radiation towards parts already cooled down or towards the open sky. This, then, will put a limit to the density of the clouds. If, from any cause, heat is supplied unequally to different parts of the stratum, the density of the clouds must be correspondingly unequal, inasmuch as, in the more heated regions, even the lowest part of the stratum, which is the worst-situated, must be sufficiently exposed to the sky to enable it, under these adverse circumstances, to maintain the low temperature which is essential to the formation of cloud. The clouds will accordingly be rarest where most heated. As, then, the clouds are translucent in all parts, and in some parts more so than in others, it becomes of importance to study the intensity of the heat and light which reach us from beyond them.

12. The clouds must either brood like a fog over the surface of the subjacent ocean, or they are separated from it by an interval. I will deal with the latter hypothesis first. Assuming, then, that there is such an in-

terval, we may suppose that the part of the atmosphere which occupies it is either nearly saturated with the vapour from which clouds are formed, or but sparingly supplied with it. If it be very moist, the clouds as they descend, either through convection or by subsidence, will pass into the form of mist, which will collect into a rain that will fall towards the ocean beneath. If, on the other hand, the interval between the clouds and ocean be far from being charged with vapour, the cloud as it descends will dissolve away among the hot and dry gases below, while the ascending currents, as they rise into the situation from which they can freely radiate, restore the same quantity of a thin gauze-like cloud. The possible alternatives, then, are, 1st, that the interval between the clouds and ocean is transparent, or, 2nd, that it is rendered in a considerable degree opaque by mist and rain, or, 3rd, that the clouds reach to the ocean.

13. Let us examine these hypotheses, beginning with that of a clear atmosphere under the clouds. If there be such a transparent space, it is easy to see that the intensity of the rays which strike the under surface of the clouds is greater than that part of the solar radiation which emanates directly from the clouds; for a portion of the rays which flow downwards directly from the clouds will be reflected by the body of the sun beneath; another portion will be scattered at the same surface. These two portions will fall short of the entire quantity of rays in the first instance radiated downwards from the clouds. But as the body of the sun is hotter than the clouds, what is here wanting will, according to a known law regarding the exchange of heat, be more than made up by what the body of the sun will itself emit*. Thus we have already a quantity of heat radiated upwards against the clouds greater than that emitted by them downwards. But further, the clouds must scatter a part of the rays that strike their under surface. Some of the rays so scattered will be afterwards reflected or scattered by the body of the sun, and will augment still further the heat striking the under surface of the clouds over that which they radiated downwards. This excess will be great if the clouds be of a material that scatters light copiously; for in this case we shall not only have a large supply of rays that had been so scattered added to the stock, but also, if the clouds scatter incident rays abundantly, they will, in obedience to the laws of the exchange of heat, be at the same time such as will emit more feebly; and accordingly the brightness which shines upon them from the background will be relatively more conspicuous. But, however this may be, whether the excess be more or less, it

* In fact, if α be the proportion of incident rays reflected by the molten ocean, and β be the proportion scattered by it, and if A be the quantity of heat which would be emitted per square metre by the surface of a perfect radiator as hot as the molten ocean, then will the quantity emitted by the molten ocean be $(1 - \alpha - \beta) A$.

But B , the quantity sent down by the clouds which is incident on a square metre of the ocean, is less than A , since the clouds are at a lower temperature; and of this, $(\alpha + \beta) B$ is returned upwards. Adding, we find the whole quantity sent upwards to be $B + (1 - \alpha - \beta) (A - B)$, which is greater than B .

at all events exists, if the space under the clouds be clear; and the clouds are in the position of a luminous and partially transparent body, with a still brighter body beyond. If the average condition of the nearer body, the screen of clouds, be such that it is in a considerable degree opaque; then will a small spot in it which is thinner and consequently more transparent than the neighbouring parts appear brighter than they; whereas, if the average condition of the nearer body allow rays to pass pretty freely through it, then will a thin spot appear but little brighter than the parts around, and the circumstances might even be such as to render it darker than them. We find both these appearances on the sun's disk. In the middle of the sun's disk we find it to be most luminous; and here the clouds would intercept least of the greater brightness beyond. In the marginal parts of the disk the spectator looks obliquely through the stratum of clouds, which is therefore more opaque to his view, so that, in approaching the edge of the disk, the less intense light emitted by the clouds would be progressively less and less fortified by the splendour within. The brightness of the disk would be accordingly shaded off towards the edge. At the same time thinner parts of the film, if not too extensive, would be seen conspicuously as faculae near the edge of the disk, while towards the centre their brightness would be merged in the general illumination around.

14. But, further, as the shell of clouds is at a much lower temperature than the adjoining layer of the atmosphere beneath it, while it is subjected to but little less pressure, it is evident that there must be a violent motion of convection between the two, the chilled portions descending, while the hot vapours from below boil upwards. Cloud will form in the rising vapours, but it will be less dense than that of the parts more effectually cooled. The appearance will be very much like what we see when looking from above upon water in the act of boiling, the smooth tops of the columns of ascending water being represented on the sun by the brighter patches due to the thinness of that cloud which can maintain its existence in the hotter vapours, while the turmoil which is seen in the water between these columns corresponds to the darker interstitial spaces which give to the sun's surface a minutely granulated appearance (rice-grains, willow-leaves, &c.), and in which the cloud at times becomes so opaque that those flakes which by prolonged emission become the most dusky seem to show like black or, at least, very dark pores. This honeycombed structure of the stratum of clouds will modify the effect of obliquity in rendering the marginal parts of the sun's disk less bright than the centre. It will cause the effect to be perceived further from the border than it otherwise would be.

15. So far, then, the hypothesis of a clear space between the clouds and the ocean seems to square with the phenomena; but upon a further scrutiny we are forced to resign it as untenable. For, as has been explained, the light which has suffered but one reflection, or been but once scattered, by the body of the sun, falls short of that which emanates directly from the clouds, and the greater brightness of the background is

due to the additions made (1) by the rays emitted by the ocean in virtue of its higher temperature, and (2) by the light which has suffered more than one reflection or been more than once scattered at the surface of the ocean. Now the umbræ of spots exhibit to us the body of the sun so dark when compared with the luminous clouds, that the great brightness of the faculæ cannot be due to the light *emitted* by the ocean. It must therefore be due to the second cause, which, as we know, can only produce any considerable effect if the clouds are of such a nature that they scatter light abundantly. But, again, we know from the proximity in which the umbræ of spots have been seen to the edge of the disk, that the interval between the clouds and the ocean is trifling as compared with the superficial dimensions of many faculæ. Hence, if the illumination of the background be due to the second cause, to light reflected or scattered from the body of the sun, the parts under extensive thin portions of the clouds would be sensibly less illuminated, and would give rise to an appearance more like that of penumbræ than of faculæ. The hypothesis would therefore fail to account for *large* faculæ. Its rejection is also demanded by the appearance of the spectrum. For if the clouds had the property of scattering light in the degree which would account for the granulated aspect of the photosphere, they would in the same proportion emit light feebly; and the whole light reaching us, whether from or through them, would fall very perceptibly short of the maximum corresponding to their temperature. And as, on the other hand, a gas is a perfect emitter of the rays of which its spectrum consists, there could not fail to be conspicuous bright lines from those gases which extend only to the hotter strata of the solar atmosphere. Now it is certain that no such lines are *conspicuous*.

16. The same objections lie with still more force against the hypothesis that the clouds are in contact with a polished ocean. We may therefore summarily dismiss this hypothesis.

17. Let us then turn to the alternative of an interval with mist and rain. The mist beneath the clouds, as it is found in a hotter region, would emit more light, though the mist were no more dense than the cloud. But the mist is probably much more dense; and it is natural to suppose that it is dense enough to be opaque, in which case, if it be formed of a material which is a good radiator, it will emit light of almost the maximum intensity which can be emitted by a body of its temperature. Indeed this effect would be produced if the mist and rain were in a quantity much less than that which would be opaque, in consequence of the assistance rendered by the body of the sun beneath, and that without any hypothesis as to the state of the latter, except only that it is opaque and at as high a temperature as the mist. Now as it is likely that the average quantity of mist and rain is much more than this, its density may undergo very considerable fluctuations without its ceasing to pour forth its full torrent of light and heat. Such, then, appears to be the brighter background which shines through the clouds. As in the last case, the currents of convection which

prevail generally over the sun produce the gradation of light fading towards the edge of the disk, and the *finely* granulated structure of his surface, with its little bright patches, its dusky intervals, and its dark pores. Where circumstances render the cloud thinner over any considerable extent while the rain continues, we have a facula which is visible (if it be not lost in the equal brightness of all around) when it is near the centre of the disk. Where such thin parts occur in numerous small patches, they produce that ordinary mottled appearance of the sun's surface which is visible in telescopes of moderate power. If the rain stop, we have penumbra. If the cloud also vanish, we have the umbra of a spot.

18. We have thus arrived at an hypothesis which in a very satisfactory manner agrees with several of the phenomena. Before, however, we trust ourselves to this or any other particular hypothesis, we must retrace our steps and go over the whole ground with care, retaining at each step all the alternatives which up to that point are possible, and reducing the number by eliminating from the list every hypothesis which we find to be inconsistent with any known fact.

19. Now, in the first place, the gradation of brightness from the margin to the centre of the sun's disk has usually been attributed to the action of an absorbing atmosphere telling with most effect upon the edges of the disk. But of course faculæ cannot be referred to any action of our earth; and it is incredible, therefore, that they exist only near the edge of the disk. Hence the cause of the gradation of light, whatever it is, must be such as will leave the faculæ of unimpaired lustre as they move from the centre to the edge of the disk, while it renders other parts more dusky. We may therefore discard the hypothesis that an absorbing atmosphere is the cause, since it would not act in this way. It is therefore due in some way to the nature of the photosphere itself.

The telescope informs us that the photosphere consists of two parts which may be distinguished:—a brighter part, seen in the centre of the disk, in the faculæ, in smaller bright patches, and in its purest form in the brighter specks of those parts of the surface which are granulated; and a dusky part, seen towards the margin of the disk and in the interstices between the bright specks of the granulation. Now, incandescent bodies radiate equally in all directions, and therefore, if the light of the sun emanated from a mere mathematical surface, the disk would not be brightest at the centre. Hence the photosphere is a stratum, not a surface. Again, the brighter parts cannot be at the top of this stratum, since in that case the margin of the sun's disk would be the brightest. Hence the bright and dusky parts are either intermingled, or the dusky parts form the outer layer; and if they are intermingled, the brighter parts must be the more transparent, to render this hypothesis consistent with the gradation of light we find on the disk.

Again, the observations show the whole granulated surface of the sun to be in a state of incessant change, although not by any means so impetuous

as the earlier observers supposed; hence at least the outer layer of the photosphere is mobile. It is accordingly either a gas, a liquid, or a cloud. The nature of its spectrum forbids our admitting it to be a layer of gas* of moderate depth; and if the layer of gas were so profound as to be opaque, it would radiate the maximum amount of light belonging to its temperature at great depths, and so obliterate the mottled appearance which exists.

Again, an *opaque* liquid would be luminous only at its surface, which we have found to be inadmissible. Nor is an ocean of *transparent* liquid sufficient. Little light gets through 20 metres of sea-water; and probably a few hundred metres of the most transparent liquid would be practically opaque. This trifling depth therefore would render the incandescent ocean luminous to almost the full extent which is possible for a body of its temperature. Such an ocean, therefore, if tranquil at the surface, would reduce the whole sun's disk to an uninterrupted gradation† of brightness. If, to account for the granulation, we suppose the ocean to be every here and there fretted with storms, the foam, being endowed with the property of scattering light abundantly, would no doubt be a bad emitter, and would therefore form dusky spots; but these spots would be most conspicuous at the centre of the disk. We must therefore reject the hypothesis of a transparent ocean. The hypothesis of a cloud, then, is the only one which remains.

20. Of clouds, there are two well-marked varieties—clouds precipitated from a state of vapour, like the clouds in our atmosphere, and clouds of fixed solids or fixed liquids, such as smoke, a cloud of dust, the mud in turbid water, oil in an emulsion. The sun attracts with so much more force than the earth that everything on his surface presses down with a force twenty-eight times as great as that with which it would press downwards on the earth's surface. From this, and from the amazing extent of his outer atmosphere, it is natural to suppose that the pressure in its lower strata must be enormous. This must occasion the lower strata to be very dense, unless the effect of the pressure be counteracted by the terrific heat. On the other hand, the average density of the whole sun being only about one-fourth of that of the earth, the solids and liquids on his surface are probably much less dense than with us. If it should happen that the lower strata of the atmosphere were more dense than some of the solid or liquid substances on the sun, these latter would rise until they reached that part of the atmosphere which is of the same density as themselves, and would float there; and if in a state like dust, they would doubtless be maintained in violent agitation by currents of convection, those on the outside being most cooled by radiation and sinking, to be replaced by others from the

* [i. e. a layer of gas whose spectrum is interrupted. But if the luminous matter of candle-flames be gaseous, such a gas is not excluded by this consideration. A gas of this kind, however, would be in a considerable degree opaque, and behave on the sun like the cloud of dust which is disposed of in § 20.—September 1868.]

† The surface, if sufficiently undisturbed, would act as a mirror near the margin of the disk; and accordingly the light emitted by it would in proportion fall off.

hot regions beneath. The dust in the ascending currents would be the warmest, and therefore the brightest, and if the currents of convection were on a sufficiently extensive scale, we might expect as a result such a granulated appearance as the sun presents. But it would be one which would be incompatible with the gradation of brightness which extends from the centre to the margin of the sun's disk. The stratum in which these convection-currents exist could affect the light coming from beyond merely as a partial screen, since there would be no marked* difference in point of transparency between the ascending and the descending currents, so that the peculiar action which the honey-combed structure of the stratum would otherwise produce is not developed. There would accordingly be scarcely any diminution of brightness till quite close to the edge of the disk; and there it should fall off very rapidly. As these are not at all the appearances which present themselves, we must give up the hypothesis of a cloud of fixed solid or liquid matter. The hypothesis of clouds precipitated from vapour is therefore the only one not excluded; and we have found that it appears consistent with all the phenomena that have been yet discussed.

Section II.—*Collateral Inquiries.*

21. The only class of bodies about the molecular constitution of which we have any satisfactory† information is gases. These appear to consist

* [The increase of transparency of the heated portions would be due to the separation of the particles of dust caused by the expansion of the intermingled air. Now at these high temperatures an addition to the temperature produces an immense alteration in the quantity of heat and light radiated (see § 68). Hence the elevation of temperature cannot be great; and accordingly the volume of the air, which varies as the temperature measured from the absolute zero, is but little increased. Such a change would determine great currents of convection, but would not materially separate interspersed particles of dust.—September 1868.]

† The dynamical theory of the molecular constitution of gases, which, if I mistake not, may be ranked in point both of importance and probability along with the wave theory of light, does not appear to have yet met with that general attention and acceptance which it seems to deserve. It may not be out of place, therefore, to add to the numberless proofs which have been drawn from its interpreting the phenomena of gases, by many writers, but especially by Clausius, the following negative proof, which demonstrates that no statical theory, whether on the hypothesis of a continuous substance or of distinct particles, is possible.

A gas is susceptible of enormous dilatation and compression without an abrupt change in the laws upon which its pressure depends; hence, if it consist of particles at rest, the force which acts in any direction on any one, must be the result of forces emanating from many others, no one contributing more than a share which may be regarded as infinitesimal. Hence it is easy to see that if the density be changed, the pressure will vary as the square of the density; for the force in any direction on any one particle will increase as the number of the particles on that or the opposite side (according as the elementary forces are attractive or repulsive) near enough to act on it, *i. e.* will increase as the density; and the number of particles subjected to this augmented force which are found within each element of volume will also have increased in the same proportion. Hence the pressure per square millimetre across any surface within the gas will increase as the square of the density: and as this is a law which does not exist in

of molecules moving about actively and irregularly in all directions, the path of any one being for the most part rectilinear, or, in other words, most of its motion being executed at a sufficient distance from the neighbouring molecules to be beyond the reach of sensible influence from them. Every now and then, however, each molecule comes sufficiently near some other molecule to have its course bent, on which occasions it darts off in a new direction. Moreover many facts in physics and chemistry lead irresistibly to the conclusion that the molecules are resolvable into simpler elements; and the probability distinctly is that each in most gases is a highly complex system. When a body so constituted is enclosed, the molecules by flinging themselves against the walls of the containing vessel produce the pressure of the gas. If the enclosure be at the same temperature as the gas, they do so without gain or loss of *vis viva*. But if the wall be at a higher temperature, the activity of those molecules which strike it is increased, and *vice versa*. The altered activity is shared with the rest of the gas by conduction and convection—or more slowly by conduction only, if the circumstances do not admit of convection; and so the temperature of the whole becomes changed.

22. When we compare different gases, we find that their molecules differ both in mass and in the motions that prevail *within** them. That the internal motions differ is abundantly testified by the amazing variety in the grouping of the spectral lines to which the various gases give rise†. Again, the number of molecules per cubic millimetre is known to be the same in all perfect gases, when taken at the same temperature and pressure. Hence the masses of the molecules are in most simple gases proportional to what chemists have called their atomic weights; and in those instances in which this is not the case they stand in the same simple relation to these atomic weights as the densities of the gases nearly do. Thus the masses of the gaseous molecules of hydrogen, nitrogen, oxygen, chlorine, selenium, bromine, iodine, and tellurium bear to one another the ratios of the numbers 1, 14, 16, 35·5, 79·5, 80, 127, 129—which are the atomic weights of these substances, and nearly in the ratios of their vapour-densities. But to represent the mass of molecules of phosphorus on the same scale, we must double the number used as its atomic weight, and take 62 instead of

gases, it follows that no gas consists of distinct particles at rest. The same proof applies, by the principles of the differential calculus, to the hypothesis of a continuous and homogeneous substance. For this proof given more at large, see Proceedings of the Royal Irish Academy, vol. vii. (1858), p. 37.

* The molecular motions of a gas consist of two very distinct parts—the motions of the molecules among one another, and the motions in the interior of each molecule.

† [An inquiry into the numerical relations between the motions of gases and waves of light forms a collateral inquiry introduced here into the MS. of the present paper as sent to the Royal Society. It has, however, been separated and published independently in the Philosophical Magazine for August 1868, in order to shorten what is here printed as far as possible by confining the collateral inquiries to those which are indispensable.—September 1868.]

31, since its atomic vapour-volume is half that of the foregoing gases. Similarly in arsenic we must take 150 instead of 75 ; on the other hand, in cadmium and mercury we must halve the atomic weights, and take 56 and 100 instead of 112 and 200. In the case of sulphur, each molecule of its vapour has a mass represented on the same scale by the atomic weight of sulphur, viz. 32, if the vapour be observed at temperatures above 1000° Centigrade ; but at some lower temperature it seems to contract to one-third of its former volume, since at 500° Centigrade, and under, it is found to be thus shrunk. The mass of each molecule has become three times what it had been before, and is therefore represented at low temperatures by 96.

23. Let us now consider what it is that puts a limit to the atmosphere. Let us first suppose that it consists of but one gas, and let us conceive a layer of this gas between two horizontal surfaces of indefinite extent, so close that the interval between them is small compared with the mean distance to which molecules dart between their collisions, but yet thick enough to have, at any moment, several molecules within it. Molecules are constantly flying in all directions across this thin stratum. Some of them come within the sphere of one another's attraction while within the layer, and therefore pass out of it with altered direction and speed. Let us call these the molecules emitted by the layer. If the same density and pressure prevail above and below the layer, the molecules which strike down into it will, on account of gravity, arrive with somewhat more speed on the average than those which rise into it. Hence those molecules which suffer collision within the stratum will not scatter equally in all directions, but will have a preponderating downward motion, so that of the molecules emitted by the stratum more will pass downwards than upwards. This state of things is unstable, and will not arrive at an equilibrium until either the density or the temperature is greater on the underside of the layer. If the density be greater, more molecules will fly into the stratum from beneath than from above ; and if the temperature be greater the molecules will strike up into it, both more frequently and with greater speed. In the earth's atmosphere it is by a combination of both these that the equilibrium is maintained : both the temperature and the density decrease from the surface of the earth upwards.

24. We have hitherto taken into account only those molecules which, after a collision, have arrived at the stratum from the side on which the collision took place. But beside these there will be a certain number of molecules which, having passed through the stratum from beneath, fall back into it without having met with other molecules, either by reason of the nearly horizontal direction of their motion, or because of its low speed. The number of molecules that will thus fall back into the stratum will be a very inconsiderable proportion of the whole number passing through the stratum, so long as the temperature and density are at all like what they are at the surface of the earth. In the lower strata of the atmosphere, therefore, the law by which the temperature and density de-

crease will not be appreciably affected by molecules thus falling back. But in those regions where the atmosphere is both very cold and very attenuated, where accordingly the distance between the molecules is great and the speed with which they move feeble, the number of cases in which ascending molecules become descending without having encountered others will begin to be sensible. From this point upwards the density of the atmosphere will decrease by a much more rapid law, which will within a short space bring the atmosphere to an end.

Not, however, before the density has sunk immeasurably below what can be reached in our laboratories. If there be a unit-eighteen* of molecules in every cubic millimetre of the air about us, there will remain about a unit-fifteen in every cubic millimetre of the best vacuums of our air-pumps. The molecules are still closely packed, within about an eighth-metre of one another; *i. e.* there are about 60 of them in a row as long as a wave of orange light. This accounts for our atmosphere's spreading to the height at which meteors betray its presence, which is far beyond the height at which we can detect it by any ordinary means.

25. If an atmosphere consist of a mixture of gases (for example, of uncombined nitrogen and hydrogen), the boundary of each gas will be at a different height. Where the nitrogen is no longer able to maintain itself, the molecules of hydrogen, with a velocity $\sqrt{14}$ (or nearly 4) times as great, can still spread far beyond it. It is also to be observed that the nitrogen will reach a greater height in consequence of the presence of the hydrogen than it could alone, since an ascending molecule of nitrogen has more chance of escaping the fate of falling back without having encountered another molecule if there be molecules of hydrogen to be met with as well as molecules of nitrogen. In this way a substance of which there is but little in the atmosphere may ascend nearly to the full height to which it would rise if present in abundant quantity.

Thus the vapour of sodium, which, as we shall find, is present in the sun's atmosphere as a mere trace, seems nevertheless to reach nearly the full height assigned to it by the mass of its molecules, through the assistance afforded to it by the abundant atmosphere of hydrogen, which extends much further. In the same way the vapour of water is probably borne to the limits of the earth's atmosphere, although but a minor constituent; and the trace of carbonic acid which terrestrial air also contains, is probably supported to a height nearly as great as it would reach if present in much greater quantity. Where, then, as in the sun's atmosphere, the lightest constituent is abundant, all the other gases which enter into its composition, will range to heights which stand in the order of the masses of their molecules, whether they be present in large or in small quantities. And where, as in the earth's atmosphere, there is but a trace of the lightest

* See *Phil. Mag.* 1868, vol. xxxvi. p. 141. A unit-eighteen is a convenient name for the number expressed by 1 with eighteen 0s after it—that is, for a unit multiplied by 10^{18} . Similarly an eighth-metre is to be understood as a metre divided by 10^8 .

constituent, viz. the vapour of water, it will form an exception to the rule, inasmuch as it will be unable to maintain its footing more than a little beyond the limits of the lightest of the abundant constituents, which in the case of the earth's atmosphere is nitrogen.

26. It becomes of importance, then, to arrange the constituents of the solar atmosphere in the order of the masses of the molecules, as this will be the order in which the surfaces of their successive atmospheres will succeed one another. A provisional attempt is made in the following table to arrange on this principle the better-known of the elementary substances, including all the bodies whose spectra have yet been compared with the spectrum of the sun, or with those of other celestial bodies. The position in the list of those substances whose names are printed in ordinary type has been ascertained by direct observations on the vapour-densities, and may be depended on; but all the rest, which are printed in italics, are placed on the provisional supposition that the masses of their molecules when in the state of vapour are proportional to their generally received atomic weights. It is probable that in some of these instances the mass is proportional to some simple multiple or submultiple of the atomic weight, and that the position in the list ought to be altered accordingly. We shall find grounds for concluding that this is the case with Barium, and that it ought to be placed in the list probably between zinc and selenium, perhaps between calcium and titanium, or between sulphur and chlorine.

TABLE I. Table of Elementary Substances arranged in the order of their Vapour-densities where these are known, and in the order of the Atomic weights where the Vapour-densities are not known.

Elements.	Observed vapour-density, that of air being the unit.	Observed vapour-density, that of hydrogen being the unit.	Presumed masses of the gaseous molecules, that of hydrogen being the unit.	Whether present in the sun's atmosphere or not.
Hydrogen	0.692	1	1 which is H	present.
<i>Lithium</i>	7 " L	not.
<i>Glucinum</i>	9.3 " G	
<i>Boron</i>	10.9 " B	
<i>Carbon</i>	12 " C	
Nitrogen.....	0.9713	14.04	14 " N	not.
Oxygen	1.1056	15.98	16 " O	not.
<i>Fluorine</i>	19 " F	
<i>Sodium</i>	23 " Na	present.
<i>Magnesium</i>	24.3 " Mg	present.
<i>Aluminum</i>	27.5 " Al	doubtful.
<i>Silicon</i>	28 " Si	doubtful.
Sulphur above 1000° C	2.23	32.23	32 " S	
Chlorine.....	2.47	35.69	35.5 " Cl	
<i>Potassium</i>	39.1 " K	doubtful.
Calcium	40 " Ca	present.
<i>Titanium</i>	50 " Ti	

TABLE I. (continued).

Elements.	Observed vapour-density, that of air being the unit.	Observed vapour-density, that of hydrogen being the unit.	Presumed masses of the gaseous molecules, that of hydrogen being the unit.	Whether present in the sun's atmosphere or not.
Vanadium *	51.2 which is V	
Chromium	52.5 " Cr	present.
Manganese.....	55 " Mn	present.
Iron	56 " Fe	present.
Cadmium	3.94	50.94	56 " $\frac{1}{2}$ Cd	not.
Nickel	59 " Ni	present.
Cobalt.....	59 " Co	present.
Phosphorus	4.50	65.03	62 " 2P	
Copper	63.5 " Cu	present.
Yttrium	64.36 " Y	
Zinc	65 " Zn	present.
Selenium	5.68	80.46	79.5 " Se	
Bromine.....	5.54	80.06	80 " Br	
Rubidium	85.4 " Rb	not.
Strontium	87.5 " Sr	doubtful.
Zirconium	89.5 " Zr	
Cerium	92 " Ce	not.
Lanthanum	92 " La	not.
Sulphur under 500° C ...	6.617	95.62	96 " 3.S	
Didymium	96 " Di	not.
Molybdenum	96 " Mo	
Niobium	97 " Nb	
Mercury.....	6.976	100.81	100 " $\frac{1}{2}$ Hg	not.
Rhodium	104.2 " Ro	
Ruthenium.....	104.2 " Ru	not.
Palladium	106.5 " Pd	not.
Silver	108 " Ag	not.
Tin	118 " Sn	not.
Thorium	119 " Th	
Uranium	120 " U	
Antimony	122 " Sb	not.
Iodine	8.716	125.95	127 " I	
Tellurium	8.913	128.80	129 " Te	
Cesium	133 " Cs	
Barium	137 " Ba	present.
Tantalum	137.6 " Ta	
Arsenic	10.6	153.18	150 " 2.As	
Tungsten	184 " W	
Gold	196.6 " Au	not.
Iridium	197.2 " Ir	not.
Platinum	197.2 " Pt	not.
Osmium	199 " Os	
Thallium	204 " Tl	
Lead	207 " Pb	not.
Bismuth	210 " Bi	

* [The position of vanadium has been altered from that assigned to it in the MS. of this memoir, in accordance with Roscoe's recent investigations regarding this substance. If the vapour-density of vanadium be ever determined, it is presumable that its molecular mass will prove to be 2V, i. e. 102.4, in analogy to those of phosphorus and arsenic, in which case its position in the Table will need to be altered again.—September 1868.]

Section III.—*Of the Outer Atmosphere of the Sun.*

27. Such, then, is the order in which we should expect to find that those of the elements which exist in the sun's atmosphere succeed one another,—the atmosphere of hydrogen far overlapping all the rest; then, at a profound depth, sodium and magnesium, reaching nearly to the same height, since the masses of their molecules are nearly equal; next, at a great distance further down, calcium; then, in a group reaching nearly to the same height, chromium, manganese, iron, nickel, and cobalt; then, within a moderate distance of these, copper and zinc; and lastly, after a vast interval, barium. These are all the elements as yet known to exist in the sun's atmosphere. Let us now compare with the observations this anticipation founded on the molecular constitution of the elements, bearing in mind that the order is likely to be in some few cases incorrect, owing to our having occasionally erred in assigning the foregoing masses to the vapour-molecules. To make this comparison most effectually, Table II., opposite to p. 32, of the intensities of the solar lines observed by Kirchhoff will be of use. In this Table the lines of each known constituent of the solar atmosphere are placed in the order in which they occur in the parts of the spectrum mapped by Kirchhoff, which extend between wave-lengths 43 and 77 eighth-metres, that is from the indigo about G to the extreme crimson beyond A*. Each spectral line is represented by a number,

* The reader should have by him Kirchhoff's maps of the solar spectrum in illustration of this paper. They have been published in a separate form by Messrs. M'Millan and Co. It will make a reference to these exquisite maps much easier, not only for the purposes of this memoir, but also for many other purposes, to mark with pencil-dots upon Kirchhoff's arbitrary scale each of the following positions of an absolute scale, founded upon Ångström's determinations of the wave-lengths of 70 lines (see Poggen-dorff's 'Annalen,' 1864, vol. iii., or Phil. Mag. 1865, vol. i.).

Positions upon Kirchhoff's scale of the principal points of a scale which expresses the lengths of the light-waves in air.

(N.B. Those positions which have a note of interrogation after them are doubtful, as they are too distant from rays measured by Ångström to admit of a safe interpolation.)

Wave-lengths in eighth-metres, i. e. metres di- vided by 10 ⁸ .		Wave-lengths in eighth-metres i. e. metres di- vided by 10 ⁸ .	
	Kirchhoff's arbitrary scale.		Kirchhoff's arbitrary scale.
43	corresponds to 2873.1	44.30	2651.5
43.10	" 2855.0	45	2553.2?
43.20	" 2837.0	46	2422.0?
43.30	" 2819.0	47	2292.5?
43.40	" 2801.1	48	2164.0?
43.50	" 2783.4	48.50	2099.8
43.60	" 2766.0	48.60	2086.7
43.70	" 2748.8	48.70	2073.7
43.80	" 2732.0	48.80	2060.8
43.90	" 2715.7	48.90	2047.9
44	" 2699.6	49	2035.2
44.10	" 2683.7	49.10	2022.6
44.20	" 2667.6	49.20	2010.2

1, 2, 3, 4, 5 or 6, which also indicates its strength in the solar spectrum, 6 meaning the darkest and 1 the faintest recorded by Kirchhoff.

28. The study of this Table is particularly instructive. It will be convenient to begin by studying the iron lines, since they are numerous, extend over a great range of the spectrum, and above all because there appear to be no bright lines in the iron spectrum to which dark lines in the solar spectrum do not correspond. This was invariably the case with those observed by Kirchhoff, who has mapped upwards of 70 of them

TABLE (continued).

Wave-lengths in eighth-metres, i. e. metres di- vided by 10^8 .	Kirchhoff's arbitrary scale.	Wave-lengths in eighth-metres, i. e. metres di- vided by 10^8 .	Kirchhoff's arbitrary scale.
49.30 corresponds to	1998.0	55 corresponds to	1309.0
49.40	1986.1	55.70	1248.4
49.50	1974.3	55.80	1240.2
49.60	1962.5	55.90	1232.0
49.70	1950.8	56	1223.8
50	1913.0?	56.10	1215.6
51	1762.0?	56.20	1207.4
51.60	1673.0	56.30	1199.3
51.70	1658.3	57	1144.0?
51.80	1644.3	58	1070.0?
51.90	1630.8	58.90	1009.7
52	1617.5	59	1003.0
52.10	1604.3	60	948.0?
52.20	1591.2	61	897.2
52.30	1578.3	61.10	891.9
52.40	1565.5	61.20	886.6
52.50	1552.8	61.30	881.3
52.60	1540.2	61.40	876.1
52.70	1527.9	61.50	870.9
52.80	1516.1	61.60	865.7
52.90	1504.8	61.70	860.6
53	1494.1	61.80	855.5
53.10	1483.8	61.90	850.5
53.20	1473.7	62	845.5
53.30	1464.0	63	800.0?
53.40	1454.4	64	758.5?
53.50	1445.6	65	719.1?
53.60	1436.1	66	682.3
53.70	1426.6	67	648.3?
53.80	1417.2	68	615.9?
53.90	1407.7	69	584.8
54	1398.3	70	555.8?
54.10	1389.0	71	528.4?
54.20	1379.7	72	502.4?
54.30	1370.5	73	477.7?
54.40	1361.2	74	453.8?
54.50	1352.1	75	430.3?
54.60	1343.3	76	406.9
54.70	1334.7	77	383.6?

The following Table contains the original determinations expressed in metrical measures on the supposition that a Paris inch = 27.07 millimetres. The sign + is added where the omitted decimals lay between .0016' and .005, and — where they lay between .005

between G. and C. Kirchhoff used a Ruhmkorff's coil to produce the iron lines; but Ångström has lately compared the solar spectrum with and 0083': + is accordingly to be read plus one-third of a Xth-metre, and —, minus one-third of a Xth-metre. This goes to about the same amount of approximation as the numbers given by Ångström.

Wave-lengths of 68 rays of the solar spectrum in VIIIth-metres, reduced from Ångström's determinations.

Designation of ray.	Wave-lengths in eighth-metres.	Intervals between rays in tenth-metres.	Corresponding positions on Kirchhoff's arbitrary scale.	Darkness of ray, G being the darkest, and breadth of ray, G being very broad, according to Kirchhoff.	Remarks.
H _γ	39.36	36—	Ca.
H _β	40.72—	36—	Ca.
	40.07+	40+	Unknown; strong.
	48—	18+	Fe; strong.
	68	9	Fe; strong.
	75	29—	Fe; strong.
h	41.04—	43+	H; very strong. Lately ascertained to be a fourth Hydrogen line.
	47	82+	Double.
g	42.29+	24	Ca; double line.
	53+	9+	Fe.
	63—	12	Fe.
	75—	36—	Fe.
G	43.10+	18—	2854.4	G	Fe; winged.
	28	15	2831.9	G	Fe; winged; broad.
	43	43+	2796.2	G	H; winged; very broad.
	86+	22	2721.2	G	Fe; winged, very broad.
	44.06+	10	2696.4	G f	Fe; winged.
f	18+	447	2670.0	G e	Fe.
F	48.65+	10+	2080.0	G g	H; winged.
	76—	19+	2066.6	5 a; 5 c	Fe; double.
	95	27+	2041.7	6 b; 6 c	Fe; double.
	49.22+		2007.2	6 c	Fe.
	24+	2	2005.2	6 d	Fe; winged on one side.
c	61	37—	1961.0	4	Fe; with wings of intensity 6.
b _a	51.72—	211+	1665.6	6 e	Fe; Mg; winged on one side.
	73+	2—	1653.7	6 b	Fe; Ni; winged on one side.
b ₂	77	4—	1648.8	6 f	Mg; winged.
b ₁	88	11	1634.1	6 g	Mg; winged.
	96+	8+	1622.8	5 b, 5 c	Fe; double like E.
	52.37	46—	1569.6	5 c	Fe.
	70	28	1527.7	5 c	Fe; Co.
E ₂	73+	3+	1523.7	6 c	Fe
E ₁	74+	1	1522.7	6 c	Fe, Ca. } Interval between E ₂ and E ₁ = 1.07 Xth-metres.
	87+	13	1508.6?	5 b	Fe.
	53.20	33—	1473.0	5 b	Fe.
	28—	8—	1466.8	5 c	Fe.
	32—	4	1463.0	5 c, 5 e	Fe; double line, closer than E.
	44	12+	1451.8	5 b, 5 c	Fe; double line like E.

that given between iron electrodes from a battery of 50 cells, which gives a far greater number* of iron lines, and with this apparatus he has been able to observe the enormous number of 460 coincidences.

TABLE (continued).

Designation of ray.	Wave-lengths in eighth-metres.	Intervals between rays in tenth-metres.	Corresponding positions on Kirchhoff's arbitrary scale.	Darkness of ray, 6 being the darkest, and breadth of ray, 6 being very broad, according to Kirchhoff.	Remarks.
D ₂ D ₁	53.69+	25+	1428.2	5 b	Fe.
	.71+	2	1425.4	5 b	Fe.
	.74+	3	1423.0	5 b	Fe.
	.76-	1+	1421.5	6 c	Fe.
	54.08+	33-	1390.9	5 d	Fe.
	.10-	1+	1389.4	6 c	Fe.
	.28+	19-	1372.6	5 b	Fe.
	.34-	5+	1367.0	6 d	Fe.
	.40+	16-	1352.7	5 b	Fe.
	.51	1+	1351.1	5 b	Fe.
	54.60-	9	1343.5	6 c	Fe.
	55.77-	117	1242.6	6 c	Fe.
	.91	14+	1231.3	5 d	Fe.
	.99	8	1224.7	5 d	Ca.
	56.03-	4-	1221.6	5 d	Ca.
	.07	4+	1217.8	5 d	Fe; Ca.
	.20	13	1207.3	5 g	Fe.
	58.94+	274+	1006.8	6 b	Na } Interval between D ₂ and D ₁
	59.00+	6	1002.8	6 b	Na } = 6.03Xth-metres.
	61.05-	204+	894.9	2 c	Ca.
	.24-	19	884.9	4 b	Ca. Co.
	.39-	15	877.0	4 c	Fe.
	.43+	5-	874.3	4 b	Ba.
	.63+	20	863.9	5 b	Ca.
	.71	8-	860.2	3 d	Ca.
	.92-	21-	849.7	3 c	Fe.
α	62.59	67+	A strong line caused by the earth's
C	65.68	300	694.1	6 c	H, winged. {atmosphere.
B	68.75	307	592.7	6 c	Winged on one side.
A	76.12	737	404.1	6	Winged.

In this list two of Ångström's rays have been omitted—those to which he assigns the wave-lengths 1903.4 and 1936.4 VIIIth-inches, which correspond to 51.53—, and 52.42 VIIIth-metres; since there are no conspicuous lines in the solar spectrum corresponding to them, and since, in the case of the latter at least, there is plainly some misprint. If we might conjecture that they ought to have been entered as 1900.4 and 1932.4 eighth-inches, they would correspond to 51.44+ and 52.31 eighth-metres, and belong to two strong iron rays.

* This appears at variance with the usual law that spectral lines increase in brightness with the temperature, inasmuch as the temperature of a Ruhmkorff's spark is probably very much higher than that from the battery of many cells. We are still too

29. The first thing that strikes the eye in the part of the table appropriated to the iron lines is a continuous gradation of intensity from the indigo to the red. The most refrangible iron lines mapped by Kirchhoff are those in the indigo, all of which he found of the deepest black, which he represents by the number 6. Then follow the lines in the blue, in which there appears to be a struggle between this intense blackness, and the darkest shade short of blackness recorded by Kirchhoff, and to which he assigns the number 5. In this part of the spectrum lines of the intensity 6 are still predominant. In the next region, the bluish-green, this struggle is continued, but now with a predominance of lines of the intensity 5. About the middle of the green we for the first time meet with an unexceptionable line of intensity 4, corresponding to the wave-length 53·87 VIIIth-metres. The last line of intensity 6 presents itself at wave-length 55·77, after which, in the yellow, orange, and red, the intensity of iron lines has for the most part sunk to 4 or 3.

30. Now the iron lines seen in the solar spectrum originate in the upper part of the iron atmosphere, each ray coming from a stratum of such a thickness that it is opaque for that particular ray. This thickness differs from ray to ray, being greater for those rays which are caused by atomic motions of feeble intensity. Such rays therefore will in part originate from a greater depth in the solar atmosphere, and therefore from a region of greater heat. They will therefore be brighter, or in other words less con-

little informed on these subjects to speculate with any confidence on the cause, and perhaps the following conjecture is the best that can yet be made.

The effect may perhaps be due to the brief duration of the sparks. The enormous temperature caused by each spark lasts for a very short time, and is not renewed until after the lapse of an interval long in comparison. The electricity, when it passes, probably produces its direct effect in accelerating and controlling the directions of the motions of translation of the molecules of the gas; and only indirectly, through the resulting violence of the molecular collisions, excites those more subtle atomic motions which give out the light. Those of the atomic motions therefore which are most influenced by each collision will be the first to reveal themselves, and the rest not until after very many collisions shall have taken place, so that before they have had time to culminate, the duration of the spark may be over: whereas when they have time fully to unfold themselves, as they can in a continuous current, they may attain in some cases a higher intensity, and consequently emit a greater brightness.

In support of this explanation, we have the fact that the lines seen with Ruhmkorff's coil have been observed to correspond to the most conspicuous lines in the solar spectrum. Now those atomic motions which are most developed by a few collisions will usually be those of which the periodic time is most subject to perturbation (see *Phil. Mag.* 1868, vol. xxxvi. p. 132). They will therefore in such cases give rise to *dilated* lines in the solar spectrum, and if the circumstances be such as to cause much of the breadth of the line to appear quite black, as for example in many of the iron lines, it will in consequence of its breadth appear much more intense. On the other hand it should be remembered as against our conjecture, that if the Ruhmkorff's sparks last as long as the measures Wheatstone made of the duration of the spark of a Leyden Jar, viz. four Vth-seconds, the number of collisions which take place during the continuance of a spark must be so great as to take away much from the probability of the explanation.

spicuous as dark rays. These same rays, since they are due to feeble atomic motions, will, in the iron spectrum produced by artificial means, appear the faintest. Now in all regions of the iron spectrum artificially produced, rays present themselves of every possible degree of intensity; whereas of those observed by Kirchhoff in the solar spectrum, the fluctuation of intensity in any one region of the spectrum seldom exceeds one degree of his numerical scale, and but once exceeds two degrees. This is conclusive evidence that iron is so very abundant in the solar atmosphere as to be opaque for the feeblest of these rays before a depth is reached which is very much hotter than the outer surface of the iron atmosphere. It also shows that the gradation of brightness in the iron lines from the more to the less refrangible parts of the spectrum is not due to the less refrangible lines coming from profound depths, and being on this account brighter. But the cause is sufficiently obvious. If a body of such a kind that it emits the maximum light corresponding to its temperature, be gradually heated, it will first begin to glow with scarlet, orange, yellow, and green rays; and according as its temperature rises its spectrum will expand in both directions towards the extreme red, and still more towards the violet. If, then, a body heated in a furnace be compared with one at a much higher temperature, the spectrum of the former will everywhere be fainter than that of the latter, but not equally so. It may have a considerable brightness in the red and orange rays, and show sensible light in the green, and at the same time appear in the comparison absolutely black at higher refrangibilities. And the same general appearance* would doubtless be found if the maximum spectrum of any one temperature were compared with the maximum spectrum of a higher temperature†. Now the upper layer of the iron atmosphere, from which comes all the light that reaches us in the iron lines of the sun's spectrum, is at a vastly lower temperature than the photosphere, but not so cool as to be of insensible brightness through the whole range of the spectrum. It begins to glow sensibly in the green, even in comparison with the intense light of the sun, and renders the iron lines of the green short of absolute blackness. And this effect goes on increasing until it reaches its climax in the orange and red.

31. As molecules of calcium vapour are of a mass less than that of iron molecules, in the ratio of 40 to 56, calcium vapour must reach a far cooler

* See § 52.

† It is natural to suppose that this steady increase of intensity with the temperature which pervades the whole range of the visible spectrum, should extend beyond it; and we are assured of it by the phenomena of calorescence. Dr. Tyndall succeeded in heating a body so as to be visible by the concentration upon it of rays beyond the red. This would have been impossible,—it would have been at variance with the principles of the exchange of heat, if the rays which were brought together were of an intensity that could be emitted by a non-luminous source. Hence the source from which they came (which was in fact a far hotter body whose luminous rays had been intercepted) was able to send forth invisible rays more intense than any non-luminous body could emit.

region of the solar atmosphere than iron. Nevertheless none* of the calcium lines observed by Kirchhoff appear to be as intense as many of the iron lines. This is no doubt due to calcium vapour being a much smaller constituent of the sun's atmosphere than iron, just as oxygen is less abundant in our atmosphere than nitrogen, and carbonic acid much less abundant than either. Judging from the indigo and green calcium lines, which are all less intense than the iron lines in their neighbourhood, it would appear that some light reaches us from a hotter region than any light that reaches us from iron lines, and accordingly that calcium gas is so rare, and in consequence the stratum which can intercept and therefore is employed in emitting these rays is so thick that, though its upper surface soars far above the upper surface of the iron atmosphere, its under surface stretches further down than the under surface of the corresponding, and comparatively shallow, active stratum of iron gas. This appears to be the case too with most of the rest of the calcium lines observed by Kirchhoff; but the lines 55.99 and 56.03 in the yellowish-green, and the lines 61.63, 64.32, and 64.55 in the red, all of which are of intensity 5, are probably exceptions, and owe their strength to calcium gas being much more opaque in reference to them, so that they are emitted by a stratum shallow enough to reach but little beyond the extreme verge of the iron atmosphere. These are some of the lines that give the calcium light, when seen undispersed, its beautiful purple colour. Calcium is no doubt very opaque also in reference to the other lines of the same class, such as the lines H_1 , H_2 , and g , beyond the limit of Kirchhoff's maps. In taking a general review of the calcium spectrum, these lines should be left out of consideration as not being comparable with the rest; and if this be done, the remaining lines will exhibit the same gradation of intensity from the red to the blue which we found in the iron lines.

32. But in the immense extent of atmosphere which spreads upward from the surface of the calcium, in the vast elevation to the boundary of the atmospheres of magnesium and sodium, and in the far greater heights to which hydrogen alone can soar, the temperature has fallen too low to produce light visible in comparison with solar light in any part of the spectrum. And accordingly all the lines referable to magnesium, sodium, or hydrogen, in whatever part of the spectrum they may lie, are intensely black. But before proceeding to examine these lines in detail, it will be convenient to inquire into the state of the regions further down.

33. The sun's atmosphere is heated beneath by contact with the scorching body of the sun, and it would throughout its whole extent attain this

* The lines 48.83 and 52.74 of intensity 6, the latter of which is the less refrangible of the lines constituting the close double line E, are left out of account; as they are also iron lines, and no doubt owe their intensity to this circumstance. The line 56.07 of intensity 5, which is also a line common to the two spectra, is probably a stronger line on this account than it would be either as a calcium or as an iron line.

enormous temperature were it not for the escape of heat from it, which is perpetually going on. The first and principal escape of heat takes place from the photosphere, but it is also going on in the form of spectral lines, whether visible or beyond the range of refrangibility that the eye can see, from the upper layer of each gas that is successively left behind in ascending through the atmosphere. The last escape of heat is from the hydrogen lines. The stream of heat which passes per second through any spherical shell concentric with the sun into those parts of the atmosphere that lie outside it, is equal to what escapes per second from the latter into space. This stream therefore remains constant wherever an interval exists between the outer boundary of one gas and the bottom of that upper layer of the next which is thick enough to be opaque for the faintest of its spectral lines; but throughout the depth of each such upper stratum the stream of heat is on the decrease.

34. We shall better understand what takes place by considering the agency by which the heat is carried outwards through the solar atmosphere. It is partly by conduction, but principally by what may be called internal radiation, to which are probably to be added in some situations convection and irregular motions such as would result from storms. By conduction I mean that conduction which is effected by the rectilinear motions of the molecules. It is the only conduction to which experimentalists have found it necessary to attend, since the quantities of transparent gas upon which they operate are not such as to be, in the cool state in which they have examined them, perceptibly opaque to any of the incident rays. But when the gas is incandescent and present in enormous quantity, the chief transference of heat through it will be in consequence of what I have called internal radiation, which comes into play whenever the spectral rays emitted by one part of the gas are absorbed by the surrounding parts before they can reach the outer boundary and escape. If the gas be highly opaque for any particular ray, which is in general the case of those rays that appear very bright in spectroscopic experiments, it will travel but a short distance before it is effectually absorbed; but the rays which are faint in spectroscopic experiments will wander further, and will contribute the most to the rapid carriage of the heat to great distances. It should also be borne in mind that if an extensive gas have a uniform temperature throughout, the rays which at profound depths are dashing about, are all of the maximum brightness corresponding to that temperature; but that if the temperature of the gas be shaded off in one direction, as it is in the solar atmosphere, the rays of internal radiation which are directed outwards at any particular spot are brighter than the maximum brightness corresponding to the temperature of that situation, since they come from warmer regions; and that those rays will be the brightest which in our experiments would be faint, since they come from the most remote, and, therefore, from the hottest of the parts from which any of the rays arrive. ∴

35. It will not now appear strange that the region immediately outside

the photosphere should attain an enormous temperature. It is in contact with the luminous clouds, and would on this account alone be brought to as high a temperature as theirs; but, beside this, rays of every refrangibility are emitted from the hotter region beneath the clouds of an intensity corresponding to the far more consuming heat which there prevails. And if out of this terrific heat all the rays be selected which correspond to all the spectral lines of every gas in the solar atmosphere, they will constitute a body of heat, a small part of which is no doubt spent upon the gauze-like luminous clouds, or absorbed by the intermingled atmosphere, but the bulk of which is poured into the atmosphere overhead. On the other hand the only heat which escapes *outwards* from this upper atmosphere is the quantity, small in comparison, which is emitted by these same spectral rays at the reduced temperatures which correspond to the dusky lines visible in the solar spectrum, or to similar lines lying beyond the limits we can see*. All the rest of the heat received by the superincumbent atmosphere is returned by it downwards, and is the measure of the fervid temperature which its lowest stratum attains. Thus the atmosphere above the luminous clouds will begin by waxing in temperature, and continues to grow hotter through that interval to which the heat emitted from beneath can in any abundance directly penetrate. At the limit of this space there will be a surface of maximum temperature, after which the heat will very gradually fade off by reason of the conduction, convection, and internal radiation which feed the escape outwards from the upper layers of the successive atmospheres.

36. It is of importance to observe that if the boundary of any one of the gases that constitute the sun's atmosphere fall within the stratum which is hotter than the luminous clouds, or very close above it, that gas can only exist in a state of such utter attenuation within the stratum that we can scarce expect to detect any lines in the spectrum corresponding to it. The stratum in question rests upon the luminous clouds beneath, and its upper limit is to be defined as that situation in which the temperature has again fallen to the same point at which it stands in the shell of clouds. At all intermediate stations the temperature is higher, or, in other words, the motions of the molecules of the gases are more active. At the upper and under boundaries of the stratum they are equal; but the pressure, and consequently the density, is somewhat less at the upper station, or, in other words, the molecules of the gases constituting the atmosphere are there a little more separated. Now any gas which comes to an end within the stratum must be unable to maintain itself at the upper surface

* We should remember that much of the sun's heat lies in this direction; for the wave-lengths of almost all visible vibrations lie between 4 and 8 seventh-metres, and the invisible rays beyond the extreme lavender probably do not include waves much less in length than 2 seventh-metres, while the obscure heat-rays at the other end of the spectrum have been observed to extend, though with decreasing intensity, until the waves are 18 or 20 seventh-metres long, and probably reach much further.

of the layer, while in the stratum of luminous clouds it is able to hold its ground with equal molecular motions, solely because the molecules are there somewhat nearer together. It must therefore at the lower station be in a state of almost inconceivable rarefaction; and, from the laws of diffusion, its density at any higher point can nowhere go beyond this. It appears, therefore, almost in vain to expect to see bright lines in the solar spectrum. If, however, any such exist*, they will probably be most readily detected in light taken from near the margin of the sun's disk, where the brightness of the region behind the luminous clouds is cut off, and where the thickness of the stratum of attenuated gas which forms the bright lines is increased by the oblique position of the spectator.

37. This rarefaction (which would be carried to an extreme in the case of a gas, if any such exist, which extends into, but not beyond, the stratum that is hotter than the luminous clouds) will also affect in a very considerable degree those gases which do not spread far beyond it. Accordingly the fainter lines in the solar spectrum either arise from such low-lying gases in a state of great tenuity, in which case those lines only can be visible in reference to which these gases are most opaque, which will therefore be the brightest of their artificial spectra; or they arise from constituents of the solar atmosphere which spread into the colder regions above, in which case they can only be those lines in reference to which these gases are highly transparent—such as are lines 50·48 and 53·52 of the Calcium spectrum, and the lines 49·21 and 51·81 of the Nickel spectrum. It may perhaps be found that faint lines of this latter class will be seen about equally distinctly in spectra formed of light taken from the centre of the sun's disk, and in spectra formed of light taken from near its margin. When the light is taken from the centre these lines have the advantage of a brighter background to set them off; when it is taken from the margin they have in their favour the greater depth of Calcium or of Nickel atmosphere which is looked through. But in the case of those faint lines of the other class which originate in the lower strata of the sun's

* I have several times thought I saw such a line, of wave-length 58·88, between the more refrangible of the lines D and the next line recorded in Kirchhoff's map to the left, almost in contact with this latter line. The appearance, however, may have arisen from the adjoining part of the spectrum having been subdued by lines not marked on Kirchhoff's map, and which a spectroscope of two equilateral flint-glass prisms could not sufficiently make out. I sometimes received the impression that there were such dim lines, but could not satisfy myself that they accounted for the bright line. Possibly there is also a bright line somewhere between the lines 1025·5 and 1027·7 of Kirchhoff's scale, and another in the right hand of the two parts into which the space between the lines D is divided by the Nickel line. Although it is on the whole improbable that the appearances are really due to bright lines, it would perhaps be worth repeating the observations under more favourable circumstances, of which the most important would be to admit only light from the margin of the sun's disk. If the suspected bright line between the lines D should prove real, it is perhaps occasioned by zinc. [For a continuation of this note, see the postscript, p. 57.]

atmosphere, the effect of obliquity will be very much greater ; so that we may expect to find these rays most conspicuous in spectra of light from very near the edge of the disk. This appears to account for observations* lately made by Ångström.

38. Let us now consider the information given to us by the lines of the spectrum which are due to hydrogen, sodium, and magnesium. In the first place the sodium lines are narrow and sharply defined. In both respects they differ from the lines of hydrogen and magnesium, which are broad and winged, that is, shaded off on one or both sides into dusky bands less dark than themselves. Now at and up to the temperature of the flame of a spirit-lamp sodium vapour can give rise to such lines ; but at the temperature of a Bunsen's burner the sodium lines have begun to expand and be ill defined. Hence we learn that in those upper regions of the sun's sodium atmosphere in which these lines originate, the temperature is lower than that of the flame of a Bunsen's burner. Nor need we be astonished that this or a much lower temperature can prevail so close to the fierce heat of the photosphere, when we take into account how effectually the outer parts of the sun's atmosphere are screened from the glare beneath by the stoppage in the intermediate regions of almost every ray that could act upon them.

39. The absence of wings to the lines D indicates† to us that there is not in the sun's atmosphere enough of sodium vapour of temperatures intermediate between the temperature of a Bunsen's burner and the temperature of the photosphere to be in a sensible degree opaque to the wings of the rays which it emits. This both shows what a mere trace of sodium is diffused through the solar atmosphere, and also to what a vast height it rises as compared with the thickness of that part of the solar atmosphere which ranges in temperature between a temperature below that of a Bunsen's flame, and a temperature comparable with the intense heat of the photosphere. In fact, the atmosphere of sodium, owing to the small mass of its molecules, which is less than half the mass of molecules of iron, must spread to a vast distance beyond the iron atmosphere ; and through this immense space the temperature appears to vary very slowly, and to be nowhere high.

40. The outward stream of heat which reaches the upper layer of the iron atmosphere for the most part escapes into space from that neighbourhood through the numberless lines of iron, calcium, chromium, manganese, and through the darker of the lines of nickel and cobalt, all of which

* See *Comptes Rendus* of October 15, 1866, or *Philosophical Magazine* of January 1867. It would be very desirable to have observations made upon spectra of light taken from different parts of the sun's disk, brought one over the other into the same field.

† [I remain unsatisfied with part of this discussion of the absence of sodium wings. There is something in the limitation of the wings of the rays of this and of some other gases, especially of hydrogen, of which I do not see the explanation.—September 1868.]

drain off heat from this region. No heat passes beyond, except the small quantity necessary to keep up the feeble escape from the lines of hydrogen, sodium, and magnesium, and others of the same class, such as B, A, &c., which are not only of a lower temperature, but are also few in number, if we may deem those that fall within the visible part of the spectrum a sufficient sample of the whole. Since, then, there is so much greater an escape of heat from the upper layer of the iron atmosphere than from the regions outside, there will exist a surface of minimum temperature near the limit of the iron, beyond which there will be first a very slight recovery and then a gradual fading off of the temperature. The observations of the sodium lines indicate that this surface of minimum temperature which lies near the outer boundary of the layer from which iron lines originate, cannot be as hot as the flame of a Bunsen's burner.

41. Within the iron atmosphere, on the other hand, there is a rapid stream of heat directed outwards to supply the outpourings from near the boundary of the iron atmosphere, as well as what is feebly dispersed by lines such as those of hydrogen, sodium, and magnesium. Still further down the stream becomes a torrent, as it has there to supply also the lavish expenditure of heat by the multitude of lines more faint than the iron lines, which are not only more numerous than lines of an intensity comparable with the iron lines, but also each one of which discharges into space a flood of heat proportioned to its exalted temperature, or, in other words, to its faintness as a line in the spectrum. All this leads us to conclude not only that the temperature increases very rapidly within the iron atmosphere, but that the rate of this increase becomes more and more precipitate as we descend. And this is in exact accordance with the intelligence brought to us by the sodium lines, which, from being wingless, indicate that the interval from the surface of the iron to the region where the temperature first becomes comparable with that of the photosphere, is both intensely hotter, and of trifling extent when compared with the vast expanse from the surface of the iron up to the surface of the sodium atmosphere.

42. Molecules of magnesium have very nearly the same mass as molecules of sodium. The two gases therefore rise to nearly the same height in the solar atmosphere. Nevertheless the lines in the spectrum due to magnesium present a very different aspect from those of sodium, into which we must now inquire. The lines of sodium are narrow and sharp; those of magnesium broad and fringed, the borders being of the intensity that Kirchhoff represents by the number 4. Now, the iron lines in their neighbourhood are of intensities 5 and 6, which shows that the upper layer of iron in which the iron lines take their rise may be distinguished into two strata, the outer of which produces in that part of the spectrum lines of intensity 6, while both together produce lines of intensity 5. To produce a line of intensity 4, a third stratum below the layer in which iron lines originate must be in action. Light reaches us from this third stratum in

the wings of the magnesium lines ; and in fact the black part of the magnesium lines is due exclusively to the magnesium vapours between the top of the magnesium atmosphere and the plane of demarcation between the two strata into which we have distinguished the active layer of iron, while the wings are caused, at least in part, by the magnesium vapour which exists in the lower section of the active layer of iron and in the stratum which immediately adjoins it beneath. Thus the layer of magnesium which gives rise to the lines of the group *b* may conveniently be distinguished into two parts, the outer of which extends from the remote boundary of the magnesium atmosphere to the middle of the layer from which iron lines originate, and the second from this latter station through a hotter layer which lies further down. If magnesium vapour existed in the situation of this lower moiety only, the magnesium lines would be bands of their present breadth, but nowhere attaining the intensity 6 : the superposition of the central black stripe is the work of the magnesium vapour in the vast outer section.

43. When we take into account how much higher a specific opacity sodium and magnesium vapour have than iron for the principal rays which they respectively emit, we are led to conclude that while magnesium vapour is abundant when compared with the attenuated vestige of sodium in the sun's atmosphere, it may be but sparingly present when compared with such a constituent as iron; and that this is so is established by the absence from the sun's spectrum of any lines corresponding to the rays of magnesium, in reference to which the specific opacity of magnesium is low, such as the magnesium lines 44.92 and 46.06.

44. We have found that there is but the merest trace of sodium in the sun's atmosphere, and that this trace mounts to an immense height above the iron. To render this possible there must be some abundant gas which extends as far as or beyond the sodium, in which it may diffuse itself, and so be borne to the full height corresponding to the small mass of its molecules. The gas which does it this service appears to be hydrogen, which, having a molecular mass only one twenty-third of that of sodium, must soar to an almost inconceivably greater height.

Hydrogen seems to be a very large constituent of the sun's atmosphere. There are three considerable rays in the spectrum of incandescent hydrogen, and a fourth faint one has been lately pointed out by Ångström. To these four rays, even to the faintest, there correspond intensely black lines in the solar spectrum. This indicates an abundance of hydrogen. The wave-lengths of the four lines are 41.04, the new hydrogen line, Ångström's *h*, in the violet; 43.43, in the indigo, which is the second of the six very conspicuous lines seen in the sun's spectrum on the less refrangible side of G; 48.65 in the blue, which is Fraunhofer's F; and 65.68 in the red, which is Fraunhofer's C. All these lines are winged: the black stripe in the more refrangible lines is very broad, and in the others it is of considerable width. These circumstances also indicate an abundance

of hydrogen. The temperature of the sun's atmosphere above the surface of the iron is too low to dilate hydrogen lines. The breadth, therefore, of the black part of the hydrogen lines must be due to the quantity of this element which is to be found in the interval between the outer boundary of the iron and that situation in which the temperature first becomes too high to appear black when projected against the brightness of the photosphere. This interval is small in the part of the spectrum where the line C occurs; at the line F it extends through a considerable part of the thickness of the layer that gives out iron lines; at the hydrogen line near G it extends quite through this layer; and in the situation of the fourth hydrogen line it extends much further down. But even in the least of these intervals there is enough of hydrogen to give a very sensible breadth to the line C. This quantity must be very considerable; as also must the quantity which can produce, in the hotter regions below, the fringes which border all the hydrogen lines. To recapitulate,—the width of the hydrogen lines, the wings that fringe them, the intense line in the sun's spectrum which corresponds to a faint hydrogen ray, and the height to which hydrogen can support traces of other gases, and more especially the vestige of sodium in the solar atmosphere, all testify to the abundance of this element.

45. The sodium lines D are an open channel through which heat is poured from a very hot region into that immense upper expanse of the sun's atmosphere which is tenanted by sodium, magnesium, and hydrogen alone. This is not the case with the magnesium lines of the group δ , nor with the four hydrogen lines. These all stop heat before it has travelled to any great distance, by reason of the great abundance of hydrogen, and by reason of the specific opacity of magnesium for the rays δ , and its quantity, which, though small, is immeasurably greater than the quantity of sodium. And on a different account, the same may be true of the faint rays of the spectra of sodium and magnesium. Two such magnesium rays were observed by Kirchhoff of wave-lengths 44.92 and 46.06; and Huggins has recorded three faint pairs of sodium lines, of wave-lengths 51.6, 56.9, and 61.6, and a nebulous band at 49.9. It is not yet fully ascertained whether there are lines in the solar spectrum answering to any of these rays. If there are such lines, they are faint. Now, if it shall prove that no such lines can be detected, it will indicate that heat from beneath of these wave-lengths passes without sensible diminution through the cool parts of the sun's atmosphere and therefore does not heat them; and if it be found that they give rise to faint lines, this faintness is to be attributed to but little of the heat despatched from hot regions being entangled in its passage outwards. Similarly the heat which is so transmitted through the wings of conspicuous lines crosses with little obstruction the colder regions above; since at the temperatures that there prevail few of the periodic times of the atomic orbits deviate sufficiently from those central periodic times which correspond to the middles of the lines.

46. But of whatever kind these or other vehicles for the conveyance of heat beyond the atmospheres of calcium and iron may be, it is certain that no sodium or magnesium rays can carry heat beyond the limits of the sodium atmosphere. It is also certain that the heat borne outwards is unable to maintain beyond the iron atmosphere a temperature as high as that of a Bunsen's burner, and that, after passing a situation but little outside the iron, the temperature falls off from this maximum. It must have sunk very low where the next considerable escape of heat takes place—at the boundaries of the atmospheres of magnesium and sodium. Accordingly, we must regard the hydrogen in that still higher dreary waste which is tenanted by hydrogen alone, as a feebly conducting body, of immense depth, warmed but moderately beneath, and exposed on the outside to a chilling radiation towards the open sky. Its outer strata must be intensely cold.

47. The case of a comet consisting of a gas * not found in the solar atmosphere is altogether different. As it approaches the sun it is exposed to the full unveiled glare of the photosphere, and absorbs the heat of those wave-lengths which correspond to the lines of its spectrum. However small a part of the incident heat this may be, it may make the comet nearly as hot as an opaque body would become; since the comet can lose by radiation no heat except through these same spectral rays.

48. Having now examined in detail the lines of hydrogen, sodium, magnesium, calcium, and iron, we may treat in a more cursory manner the other elements that have been observed in the sun's atmosphere. Chromium, nickel, cobalt, copper, and zinc enter in small quantities into the

* If, indeed, a comet consist of gas, which, perhaps, we ought to deem highly improbable. The molecules of a gas pass most of their time beyond the reach of one another's molecular action, and, unless further confined by a sufficient force of gravity, would each pursue an independent orbit of its own. They would therefore tend gradually to extend like a stream of meteors along their common path; for the orbits being slightly different would have slightly different periodic times, which in the lapse of ages would operate in this way. It does not appear likely that the gravity of a body so large, and with so small a mass as a comet, could successfully withstand this tendency. But if the comet were kept together by a molecular cohesion, somewhat like a solid, there would be no such difficulty. Nor is it necessary to suppose that this solid, if such we are to call it, would retain this constitution when subjected to an intense gravity like the earth's: the hardest Archangel pitch flattens down under its own weight, and in time adapts itself to its containing vessel. The matter of comets may on our earth be gas.

And, again, it seems improbable that a comet can have been raised to the temperature of ignition at the distance from the sun that the earth is; yet this was the distance of Tempel's comet when its nucleus was seen by Mr. Huggins to emit a spectral ray. The only bodies we know to have the property of glowing at low temperatures are phosphorescent bodies; and we know from Becquerel's observations that the spectra of phosphorescent solids consist of bands, in some cases narrow.

The comæ of comets cannot be *transparent* gas, since transparent gas would not be conspicuous by reflected light. The phenomena of tails, too, suggest some entirely peculiar constitution.

composition of the sun's atmosphere. Probably nickel is the most abundant of them. Of the others no lines appear in the sun's spectrum, except those in reference to which they have a high specific opacity, in many cases higher than that which iron has for any of its rays. There are, therefore, but traces of them present; and the appearance of the lines agrees well with the situation in the sun's atmosphere assigned to them by the masses of their molecules: chromium, projecting quite through the iron atmosphere, produces a few lines of an intensity comparable with that of the iron lines in their neighbourhood; and the boundaries of cobalt, nickel, copper, and zinc, appear to lie within that upper layer of iron which sends forth iron lines.

49. The appearance of the zinc lines is not incompatible with this element's having the vapour-density usually supposed by chemists, viz., 32.5 instead of 65; but the evidence of the sun's spectrum, such as it is, for it is scanty, owing to the paucity of the lines, seems to lean against this hypothesis, unless a similar reduction is to be made in the case of all the other metals of the atmosphere. But whatever uncertainty may rest on this point, there is at least no doubt that barium cannot have a vapour-density anything like so high as 137. At most it cannot exceed half that number, which would barely raise the boundary of the barium atmosphere within the lower part of the layer from which iron lines proceed; and, if it were not for objections on chemical grounds, the strength of such lines as the barium lines 45.66, 49.37, and 61.43 would prompt us to suspect for the vapour of barium even a lower density. But the strength of these lines is probably due to the remarkably high specific opacity of the vapour of barium in reference to them. There is plainly only a small amount of barium in the sun's atmosphere.

50. It will readily be perceived that it is vain to look for the cause of any conspicuous line mapped by Kirchhoff, in any substance with a vapour-density more than 70 times that of hydrogen. This narrows very much the field in which to search for the origin of the darker of the lines enumerated in Table III., opposite, the table of unappropriated lines. Many of these, as, for example, three of the five lines of the group at 60.3, are probably due to manganese, and may be removed from this table, as soon as a list of the thirty manganese lines, lately identified by Ångström, shall have been published. Others of them are probably some of the 460 iron lines, produced by a continuous electrical current, or among the additional lines which may be produced under like circumstances in others of the elements which we have been heretofore examining. When all these are eliminated it does not seem likely that many conspicuous lines between G and B will remain to be traced to their source. Carbon is probably as devoid of volatility as it is infusible; or at all events the one probably bears some proportion to the extraordinary eminence of the other. If this be so, it cannot be a gas at the temperature of the situations from which *dark* lines come, or at least not in sufficient quantity to produce visible effect.

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Colours.....	Indigo.			
Standard rays	G.	f.		
Wave-lengths in eighth-metres	43	44	45	4
Hydrogen 1	1 2 6 2 1
Sodium 23
Magnesium 24.3	0
Calcium 40	5 4 4 5
Chromium 52.5.....	5
Manganese 55
Iron 56	4 3 2 6 6 6 4 3 3	3 6 6 3	5 6 5 5
Nickel 59	4
Cobalt 59	5 6	0 3
Copper 63.5
Zinc 65
Barium 137	4 6	1

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No. of rays of intensity 6	0	2	2	0
Ditto ditto 5	5	11	14	0
Ditto ditto 4	29	11	17	3
Ditto ditto 3	51	28	18	2
Ditto ditto 2	47	28	27	2
Ditto ditto 1	47	39	20	14
Wave-lengths in eighth-metres	43	44	45	4

* The numbers of T ngly in s



But it is very much to be wished that a comparison should be made of the spectra of boron, fluorine, sulphur, chlorine, titanium, and phosphorus, with the sun's spectrum, and especially of chlorine, if any weight is to be attached to the suspicion, founded on very insufficient grounds, that the solar lines 43·40—, 43·55—, 66·38, 66·50, 66·68, and 70·00, the group of three lines at 45·1, and several others, are to be referred to this element.

51. The absence from the sun's atmosphere of such gases as nitrogen and oxygen, and of hydrogen from the atmospheres of some other stars, and the fact that while some active chemical agents lose, like sulphuric acid, their energy under such increasing temperatures as our laboratories can provide, others, like boracic acid, become practically more powerful, give a considerable amount of colour to the presumption that compound bodies exist in the sun. The masses of the molecules of these compound bodies will in most cases be too high to permit them, however volatile, to reach the cool parts of the sun's atmosphere, so as to reveal themselves in conspicuous solar lines. But the probability of their so appearing is very much greater in the class of ruddy stars, as we shall find in the sequel; and, perhaps it is not impossible that the line B of the solar spectrum, or some of the lines less refrangible than B, may result from some compound of low vapour-density, such as hydrochloric acid*. It is certainly very remarkable that neither B nor any line less refrangible has up to the present been identified with a ray of any simple substance.

52. Upon a general view of all the lines of the solar spectrum it appears that their intensity continuously diminishes from the violet end of the spectrum up to the line B. At this point, owing to the sudden introduction of an entirely new set of lines, their intensity abruptly and very much increases. These new lines either have a terrestrial origin or come from substances which stand high in the solar atmosphere. The lines, however, which originate further down, do not attain their minimum of intensity until they reach a point further to the right than B. This appears both from the progressive diminution of their intensity up to B, and from the total, or almost total, absence of lines further on, wherever a vacuity is left between the lines which we must attribute to a different origin, as at wavelengths 71·1, 73·8, and in the wide spaces between the prominent lines from this situation up to the line A.

53. When this is considered in connexion with the cause to which the diminution of intensity is to be referred, it indicates that if two perfectly radiating bodies were gradually heated while the difference of their temperatures was kept constantly the same, the point of the spectrum at which the difference of their brightness is least would advance with increasing temperatures towards the red end of the spectrum. When the body of

* If there be chlorine in the sun's atmosphere, the presumption upon chemical grounds is very strong that there must be hydrochloric acid in the upper regions; and from its vapour density (18·25) the lines of hydrochloric acid would be black in whatever part of the spectrum they might occur.

lower temperature has but just begun to glow, we know that this situation of minimum difference of brightness is found in the orange ; at temperatures approaching that of the photosphere it has removed at all events as far as the line A, that is nearly to the extreme verge of the visible spectrum, and it has, perhaps, advanced beyond it. This, as we shall find further on, explains how some solitary stars can attain a depth of colour that approaches crimson.

54. It appears from the analysis which has been made that none other of the gases in the solar atmosphere that extend as far as the stratum from which iron lines come, can compare in quantity with hydrogen and iron ; and from what has been stated in § 36, we may be sure that there is no very abundant gas which comes to its limit in the hot regions that intervene between this stratum and the photosphere. Hydrogen and iron are accordingly the principal ingredients of the parts of the sun's atmosphere which extend beyond the photosphere.

Section IV.—*Of the Photosphere and the subjacent parts.*

55. In interpreting phenomena of solar spots we should never forget the disadvantages under which we attempt the enterprise. Our theory may be true, but it is incomparably more meagre than our knowledge of the causes of terrestrial weather. Our observations may be correct, but they give us only outside glimpses, and from such a distance that France or Spain would be specks too small to make out whether they are round or square. We must not imitate the peasant who saw from afar the smoke of a great city, and persuaded himself he had a very good idea of the kind of place a city is. If our explanations of the phenomena of terrestrial weather are dim and unsatisfying, we cannot reasonably ask from a theory of the corresponding phenomena of the sun, even though it were beyond a doubt the true theory, more than the first hazy and rude sketch of an interpretation.

56. Many fixed gases which are too heavy to extend at all, or in any abundance, through the stratum of minimum temperature, must wax in density very rapidly within it. Hence the density of the solar atmosphere becomes almost suddenly greater at the shell of luminous clouds. This may be the cause of an appearance not unfrequent in spots near the margin of the sun's disk, in which situation the further side of the umbra of a spot is often bordered by a bright crescent, giving to the umbra the appearance of a hole punched through a plate. This appears to be because there is, in these cases, in reality a depression of the dense strata at the umbra, shallow, perhaps, but yet with sides sufficiently inclined to enable light coming so obliquely as to suffer total reflection* against the flatter surface of the penumbra, to escape through it. A similar cause may, perhaps, and probably does, enable light to escape from patches of the penumbra when the surface of the penumbra is irregularly undulating in a sufficient degree.

* Such as that which produces Fata Morgana.

Local showers are in other cases the cause of brightness in the umbra and penumbra.

The sudden increase of density of the sun's atmosphere at the photosphere must serve to keep the luminous stratum in a nearly spherical form. The surfaces of the gases above the photosphere may be violently tossed about by the storms of the solar atmosphere, but the surface of the photosphere is never carried further than to the top of a facula or the bottom of the umbra of a spot.

57. The winds which affect the photosphere may be distinguished into two classes, those of the sun's outer atmosphere, and those of the regions within the photosphere. Both classes may coexist in different parts of the same storm. The former class sweeping through the open space above the photosphere, and through rarefied air, will often come from far, and as a general rule be the swiftest. Those below, moving in the dense part of the atmosphere, and perhaps within a confined space, can but seldom attain the same high velocity.

58. Both classes of wind tend to obliterate the cool film in which clouds usually exist, and to replace it by hotter air. But the hotter air substituted by winds from below, will be equally charged with moisture; while winds from above will tend to dilute with dry air both the cool film and the adjoining strata immediately under it. In both cases new and more transparent clouds will form; but in the former case the rain will not cease, and we have only facula; in the latter it may and often does,—in fact, whenever the film of clouds and the subjacent stratum with which it is mixed by convection, have been rendered sufficiently dry. When by prolonged convection this state of things is passing away, there will be a struggle between dry weather and wet, which we shall see in the patched appearance of the penumbra.

59. An umbra presents itself when the cloud, too, is removed, and the dusky body of the sun seen through the opening. It does not seem likely that this can take place so long as there is any of the moist stratum at a temperature below its boiling-point and exposed to radiation. If this view be correct, the umbra can only occur either when the depression caused by a rotatory storm, or by winds impinging from above, has obliterated the dense stratum and brought the air into contact with the ocean; or when, by the influx of hot air from above or the upheaving of the hot strata beneath, it has come to pass that throughout the whole of a vertical column there is no place where the vapour which forms cloud is at a temperature below its boiling-point. If this happen through the rise of subjacent strata, we should have an umbra without penumbra; and it does not seem impossible that the same appearance may sometimes present itself where a depression is caused by a wind impinging from above which has not exerted much horizontal friction against the surrounding parts of the photosphere.

60. It must often happen that a hot current sweeping over the surface of the penumbra dissolves away part of the cloud, diluting the vapour

with dry air up to the point of being but just unable to precipitate itself while exposed underneath to the heat of the penumbra. If a current so charged with vapour happen to cross the umbra, it will receive less heat from below, and some of the vapour in it will now be able by radiation to maintain itself as cloud. This cloud will be peculiarly circumstanced. It is formed from an isolated body of vapour, and once formed will continue in existence, since the hot currents which will rise at intervals through it when convection sets in, will consist of dry air unable to generate the cloud overhead, which would otherwise screen it from the open sky. It will accordingly often find itself under circumstances to become by reason of this prolonged existence progressively cooler; and as the temperature falls, more of the vapour is able to precipitate itself, until at length the cloud becomes so dense that rain sets in. The rain is probably caught and dissolved in the dry air below, long before it can reach the body of the sun; but if it last through a space of even a few thousand metres, it will give to the bridge of vapour the brightness of a facula. In other cases the vapour either carried into the umbra from around, or perhaps rising into it from a steaming ocean beneath, appears to form mere pellicles of cloud that mottle its deep shadow. When the storm is of the nature of a whirlwind, a current of dry outer air which has not lapped up moisture from the photosphere, usually seems also sucked in, and manifests its presence in the dark spot which Mr. Dawes has called the nucleus of the umbra.

61. It appears more reasonable to suppose that the phenomena which have hitherto been explained by the transference of ponderable matter over immense distances in incredibly short times, the filling up of gulfs, and the like, are phenomena of the rapid formation or dissolution of cloud, and lose much of their marvellous character. Terrestrial cloud may be seen to form within a very few minutes over the whole of the visible heavens, and often when there is no wind, or apparently advancing against the wind.

62. If there be a substance in the sun of low vapour-density, but not capable of existing in a state of vapour in the coolness of the height to which it would otherwise rise, and if this refractory substance is volatile at the temperature and pressure which exist lower down, it will behave in a very peculiar manner. In the lower strata of the sun's atmosphere it will exist as a vapour; and from this situation it will keep continually making its way upward in its effort to find its natural level. Before it reaches its destination, however, the gas incessantly streaming upward will as incessantly be precipitated. If the particles of the cloud so formed are heavier than the surrounding atmosphere, they will begin to subside. Not only so, but the chill caused by their radiation in their new solid or liquid state, will make the inverse flame spoken of in § 8 burn downwards, until it sinks to that level at which the upward supply of vapour, owing to its tendency to diffuse itself upwards, or caused by currents of convection, exactly balances the downward motion of the fiery cloud from subsi-

dence or the descending currents of convection. Here, then, if this substance be in sufficient abundance, we have all the conditions necessary for the sun's luminous clouds. And we are led almost irresistibly to conjecture that in carbon* we have such a substance. The mass of its molecules is very low, either six, or twelve, or twenty-four times the mass of a molecule of hydrogen. It appears to have just the requisite degree of fixedness; it shows no sign of volatility at any ordinary high temperature, but has been driven into vapour by one hundred elements of Bunsen's battery, each element consisting of six ordinary cells coupled side by side; that is at a temperature which may, quite consistently with everything we know, be that of the strata adjoining the sun's photosphere. There is enough of carbon in the sun to produce the luminous clouds, if carbon be as large a constituent of the sun as it is of the earth; and most of the carbon in the sun is probably uncombined, as carbon does not seem apt to form compounds likely to be abundant which can stand intense heat. It is, moreover, precipitated from its vapour as a black body with the most perfect power of emission of any known substance; and we are assured that the luminous clouds consist of some such material by the absence of bright lines from the solar spectrum. It would probably be impossible, in the present state of our knowledge, to put forward on behalf of any other substance, simple or compound, anything like the same claim to be deemed the material of which the luminous clouds consist. And I know of but one consideration to be set on the other side, viz. that if the luminous clouds be a smoke of carbon, and if the rain beneath is more properly to be described as a fall of soot, in flakes like snow, and if these flakes come to rest upon the surface of an ocean beneath, they must by their high radiating power render this surface eminently luminous, which we know from the phenomena of spots that it is not.

63. As, then, there are strong reasons for surmising that the luminous clouds consist of carbon, we are led to enquire what may exist to remove the one difficulty in which this hypothesis involves us. Now, in the first place, it would disappear if the heat in the space beneath the clouds melts the falling flakes, so that they reach the ocean like rain, and mix with the other liquids constituting it. And it would disappear if the heat and dryness of the space beneath the clouds enable it to evaporate the flakes ere they reach the ocean. And, finally, it would disappear if there be no such ocean, but only a continuation of the atmosphere becoming denser and hotter. It will be necessary to examine this last hypothesis with some care to see that it is compatible with the known phenomena of spots.

64. It is not likely that carbon is the only substance in the sun that

* In connexion with Dr. Frankland's discoveries respecting flame, it should not be forgotten that such solid particles as Davy supposed in flame are undoubtedly adequate to produce luminous effects, and possibly are a source of light in other cases as well as on the sun.

possesses the properties which are the conditions for the formation of cloud, although it is probable that carbon is, of such substances, that one which has by far the lowest vapour-density. It is, at all events, presumable that among such abundant elements as nitrogen, oxygen, silicium, and aluminium, or such of their compounds of low vapour-density as can exist in the sun, there may be some which, like carbon, are solid or liquid at the temperatures and pressures of the greatest heights to which they would, if gaseous, rise. And if the atmosphere of the sun extend to any great distance below the photosphere, there must be in the sun such a substance to account for the dusky background we see in the penumbrae and umbrae of spots. There must in this case be a second layer of clouds, formed not far beyond the photosphere, in the comparatively short space through which the temperature augments rapidly between the luminous clouds and the central parts of the sun. These clouds must, moreover, be of some transparent material to possess in a sufficient degree that property of scattering light which would render them as devoid of emissive power as we see them to be. For the same reason we must conclude that the sooty shower from above cannot reach them, as it would inevitably soil them, so as to deprive them of these essential qualities. We learn from this, that the point at which carbon boils must fall within the short interval between the two layers of clouds. This is not at all unlikely, inasmuch as the advance downwards of the inverse flame, of which mention has been so often made, would probably be arrested only by its close approach, either to the bottom of the atmosphere, or to the situation in which carbon boils, so as to be entirely dissipated in vapour. And the second layer of clouds would quickly follow, since its position depends on that taken up by the carbon clouds, as it must lie within the layer of *rapidly* varying temperature immediately under them. If this hypothesis, then, be the true account of what takes place on the sun, the penumbrae of spots are caused by our seeing the clouds beneath through a gauze-like film of carbon cloud which has ceased to send down rain; and the umbrae of spots are formed when a very shallow saucer-like depression of the photosphere has carried a part of its outer surface so far that it has reached the region in which carbon will boil. Here the filmy cloud of carbon, which nowhere else can entirely disappear, will be completely dissolved away.

65. In this branch of our enquiry we are often obliged to deal with hypothetical matter, and cannot in such cases look for conclusions which command our assent. We must be satisfied if we may hope that they will prove of use in guiding future investigations. Nevertheless, I am disposed to think that we should give the preference, as a provisional hypothesis, to the supposition of a layer of cloud lying under the photosphere, rather than to the only other alternative which seems in any considerable degree admissible, namely, a highly reflecting ocean. It is perhaps, on the whole, and in our present state of ignorance, encumbered with fewer difficulties.

Section V.—*Of Clouds in the Outer Atmosphere.*

66. But to return to what is more to be relied on, we may be sure that some small part of the carbon, or whatever else the luminous clouds mainly consist of, and similar traces of any other ingredients that enter in less quantities into their composition, must escape precipitation, and will diffuse themselves upwards, and the more freely as they come first to a region where they are raised to a higher temperature as well as subjected to less pressure. Through this hot stratum they will continue gaseous, but a short distance above it they will meet with a temperature low enough to condense them. Here, then, separated from the photosphere by the whole depth of the hot stratum, they will form a second film of luminous clouds, one, however, which is so attenuated as to be visible only during an eclipse, when it constitutes the lowest of the clouds that then present themselves. They may be traced in Dr. De La Rue's photographs of the eclipse of July 1860* as continuous arcs of cloud extending about 35° on either side of the points of first and last contact. Hence, and from the apparent magnitudes of the sun and moon on that occasion, we may conclude that this upper shell of clouds was at an apparent distance of about $11''$ of space from the edge of the sun's disk, which corresponds to an absolute height above the photosphere of 8 metre-sixes, or $1\frac{1}{4}$ time the earth's radius†. And as the clouds of which we are now speaking are a little outside the hot stratum that lies immediately over the photosphere, we shall not be far wrong in concluding this stratum to be about as thick as the earth's radius is long. The clouds outside it probably form a nearly continuous shell round the sun. They are everywhere of extreme tenuity, but may nevertheless be very variable in density; and it is probably owing to this that the concave

* Philosophical Transactions for 1862, p. 333.

† A metre-six means a metre multiplied by 10^6 .

Dr. De La Rue took two eye-sketches also of the eclipse, commencing the first about thirty seconds, according to his estimate, after the eclipse began. Now the arc of cloud about the point of first contact is represented on the first, and indeed on both drawings, and must have been at a greater height in the sun's atmosphere than I have assigned to it, to have been seen by Dr. De La Rue, unless we may suppose that he overestimated the interval of time which had elapsed by a few seconds. This, however, on an occasion of so much hurry, may perhaps have happened, and it seems difficult otherwise to reconcile the eye-draughts with the photographs. The data made use of to get out the result in the text are:—

Length of arc of cloud visible five seconds } after the moment of contact	=	0 ' "
		70

Sun's apparent semidiameter	=	0 15 45
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Moon's apparent semidiameter	=	0 16 33
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Sun's diameter = 13·7 metre-eights.

Earth's diameter = 12·7 metre-sixes.

Approach of the sun and moon's centres per minute of time = $25''$ of space.

The allowance of 5 seconds from the moment of contact has been made, because the cloud seems to have taken about that time to impress itself upon the photographic plate.

sides of the two arcs shown in the photographs exhibit such a ruggedness that, as Dr. De La Rue has pointed out, it cannot be accounted for by the mountainous edge of the moon. In fact the film of cloud seems to be so excessively thin that even during an eclipse it can only be seen where it is presented very nearly edgewise at the extreme margin of its disk, or for a short distance inside it, a distance which varies with the local density of the film, and so gives rise to the appearance in question.

67. This second shell of clouds, as they consist of the same materials as the clouds of the photosphere, and are higher in the atmosphere, and therefore subjected to less pressure, will evidently not form until they can do so at a somewhat lower temperature. But the difference may be so slight that in their normal position these clouds lose more heat by radiation towards the sky than they receive by absorption from the photosphere, which would cause them to imitate, but with a languor proportional to their flimsiness, all the phenomena of convection, &c. which we have traced in the principal layer of clouds.

68. But this behaviour would be altogether changed if by any cause a part of the film were borne upwards into the cool regions above. At whatever part of the atmosphere a cloud may find itself, it will be exposed to the unmitigated glare of the photosphere, and will be raised by it to a temperature bordering upon that of the photosphere itself*. A cloud in this situation will therefore warm, instead of cooling, the air in which it is dispersed, and will tend to float violently upwards until it gets to a part of the atmosphere so rare, that the particles of condensed vapour tend to sink in it from their specific gravity as fast as they are carried upwards by the body of heated air entangled with them. This may be the cause of the columnar clouds with overhanging tops which have been observed during eclipses. As they spread out at the top and become diffused, they will not as effectually heat the intermingled air, and will therefore begin to subside. Between clouds that are carried so violently upwards and those that repose in the luminous shells, any intermediate descriptions may exist, and were perhaps the cause of the mountainous projections from the upper shell that have been seen, and of several of the detached clouds.

69. But besides the materials that enter into the composition of the

* If we could trust at high temperatures, which of course we cannot, Dulong and Petit's law for the velocity of cooling, viz. :—

$$v = k(\alpha t - \alpha'),$$

where v is the fall of temperature per unit of time ;

t , temperature of the particle in Centigrade degrees ;

t' , the temperature of the radiations to which it is supposed to be subjected on all sides ;

k , a constant, depending on the nature of the particle and on the position we assume as the zero of our thermometric scale ; and

$\alpha = 1.0077$;

we should find that the temperature of a cloud exposed, on one side to the photosphere, and on the other to the sky, falls short of the temperature of the photosphere by little more than 90° Centigrade.

clouds of the photosphere, we must remember that there may exist other substances in the sun or in some other stars capable of giving rise to clouds. If there be materials of sufficiently low vapour-density, and in a sufficient degree more volatile than carbon, though not volatile enough to stand the cold of the height to which their vapour-density would otherwise lift them, they will be precipitated in cloud. Or gases in the solar atmosphere which are kept asunder by the temperatures of its lower strata, may be able to combine in the cooler regions above. If the new body be a solid or liquid, it will constitute a cloud. Even if it be gaseous, it will in general have other spectral lines than those of any lower-lying gas in the atmosphere, and will therefore be subjected to the direct radiations of the photosphere; it will accordingly become intensely heated, and in many respects behave like a cloud. Its density, too, will in most cases be greater than that of either of its constituents. And, finally, a gas which in the lower parts of the sun's atmosphere emits only rays of a spectrum of the second order, may in the upper regions find itself under circumstances to produce a spectrum of the first order. If this should happen, the gas in its new condition would be exposed to the full heat of the photosphere, and would conduct itself like a cloud.

70. From the exceeding transparency of the solar clouds, they are entirely without that abundance of internal reflections and refractions which are what give to a cloud of steam dense enough to be opaque, or a sheet of paper, or a piece of white marble, their lustre when illuminated. It is accordingly by their inherent splendour given to them by their being made intensely hot by the photosphere, not by borrowed light, that they shine. A cloud of dark opaque materials is therefore, *cæteris paribus*, the brightest. Those which Mr. De la Rue found impressed on the photographs, though not visible to the eye, must have been of substances transparent in regard to most visible vibrations, but opaque for some of higher refrangibility.

71. It is very likely that there may be substances in the sun's atmosphere, or in those of some of the stars, which reach a height at which they are unable to remain in the state of gas by reason of the surrounding cold, or to assume permanently the form of cloud because of the heat of the photosphere to which they would thereupon immediately become exposed. In such cases there will be a struggle between the two conditions, the vapour continually condensing and redissolving until it has by this process imported much heat into its neighbourhood. Wherever such a state of things exists, it must inevitably have the effect of raising some of the isothermal surfaces above the position in the atmosphere they would otherwise occupy. Similar consequences would ensue if two gases became so cool that they could no longer continue uncombined, and were so heated through their new spectral lines the instant they united that the new substance was at once resolved back into its constituents; or where a gas reaches a situation too cold for its existence in the state in which it sends

out spectral rays of the second order, and no sooner changes its condition than it absorbs through its new spectral lines heat to such an extent that it must fall back again. Such struggles may be the prolific source of storms when they are local, and perhaps of an appreciable variation in the brightness of stars when they are on a great scale.

Section VI.—*Of the Distribution and Periodicity of the Spots.*

72. We may catch a glimpse from the foregoing investigations of what appears at least a possible explanation of several phenomena of the solar spots, which we do not seem yet in a position to refer to their causes with confidence, such phenomena as the local distribution of spots and their periodicity. If from any cause a portion of the lower strata of the outer atmosphere is thrown upwards, it will carry a part of the second stratum of clouds above its natural level. The intermingled air will dilate and tend to cool down as it ascends; but its temperature will be restored by the heat absorbed and communicated to it by the cloud carried with it. Its thus remaining hot will convert what was perhaps at first only a gentle upheaval into a violent upward current, which will, from the operation of causes familiar upon the earth's surface, occasion a cyclone in the lower strata of the outer atmosphere. The inner atmosphere (that is, the dense atmosphere from the surface of the photosphere downwards) cannot be readily drawn into the vortex, by reason of its great specific gravity; but it will be swept round and round by the violence of the hurricane above, and a kind of whirlpool will result which will depress the central parts into the penumbra and umbra of a spot and lift its borders into faculæ. The formation of this whirlpool will be greatly assisted if, as we shall presently see we have reason to suspect, there are preexisting currents in the inner atmosphere setting in opposite directions along the zones of spots.

73. If, then, we are right in attributing a large proportion of the spots to ascending currents in the outer atmosphere, we must next seek some cause which can determine the existence of such upward currents in two bands parallel to the equator. It is natural to look for this in some phenomenon analogous to our trade-winds; and, as Sir John Herschel has observed, such a phenomenon may arise if the ellipticity of the sun bring about an unequal escape of heat from his poles and from his equator. The elliptic strata of the atmosphere could be *in equilibrio* only on the supposition that they are of precisely the same density throughout: but this they cannot be; for as the outer atmosphere is an imperfectly conducting plate, heated on the one side by the photosphere, cooled on the other by radiation towards the sky, at the poles, where the plate is thinnest, its outer strata will be sensibly hotter than their average temperature over the whole sun, and their inner strata very slightly cooler; and at the equator, where the plate is thickest, its inner strata will be hotter than the average, and its outer strata cooler. Hence at the poles, where the temperature of the outer parts of the atmosphere is higher than the average, they will diffuse

themselves upwards and overflow ; at the equator, where the temperature is less than the average, they will subside and tend to escape laterally at the bottom. Moreover, the lower strata being subjected at the equator to more pressure than the average, by reason of the coolness of the superincumbent strata, and to less pressure at the poles, will also contribute to produce an under-current in the outer atmosphere from the equator towards the poles. Hence if it were not that the rotation of the sun modifies the result, we should have a constant wind blowing steadily from the equator to the poles over the surface of the photosphere, and a counter-current in the upper regions of the atmosphere.

74. The effect upon the inner atmosphere is directly the reverse. Heat will escape from the photosphere very slightly more freely at the poles, less freely at the equator, than the average. The upper strata of the inner atmosphere will therefore be a little lighter at the equator, and will overflow towards the poles, tending to produce a feeble surface-current in the photosphere in the same direction as the wind which blows above it from the equator towards the poles.

75. But the sun's surface is all the time being carried round by his rotation from east to west. This will impart a strong westerly direction to the descending current where it reaches the photosphere at the equator*, and will further render it where it spreads out over the photosphere towards the poles, a south-east wind in the northern hemisphere, and a north-east wind in the southern. Thus these winds blow in such directions as to rotate more rapidly than the general body of the sun, and they therefore seek to raise themselves above the photosphere†. At the equator the

* [This first part of the effect of the sun's rotation was overlooked by the author when writing this paper. The omission has been supplied in the text above, and the reader is requested to correct the error in the abstract of the memoir (see Proceedings of the Royal Society for June, 1867) where a calm is spoken of as prevailing over the equatorial zone of the photosphere.—July 1868.]

† The similar centrifugal tendency in the current which overflows from the earth's equator would no doubt keep it throughout its whole course outside the polar current, were it not that being charged with moisture and cooling as it advances, some of its strata soon become so loaded with clouds, that they, and the clouds amongst which they are entangled, come to have between them so much higher a specific gravity that their downward tendency due to this cause overcomes the outward tendency of their superior rotatory motion. In the case of the sun, on the other hand, the clouds and the rotatory motion operate at first in the same direction upon the equatorial current, both tending to raise it ; but after some elevation has been attained, they there, as on the earth, act in opposite directions, the motion of rotation tending to depress the current as soon as it gets above a height which must be moderate, when compared with the vast extent of the sun's atmosphere.

Furthermore, the earth's polar current coming from regions of slower motion has a tendency to descend, and when it gets down, a tendency to creep along the surface of the ground. Both the currents accordingly seem to contribute to that descent of both which takes place in the earth's temperate zones. But on the sun the ascent of both the currents over the zones of spots seems to be brought about by the tendency of the equatorial current to rise being more powerful than the tendency of the polar current to cling to the photosphere.

upward tendency expends itself in somewhat retarding the descending current, but a few degrees on either side, where this obstacle has become sufficiently feeble, it determines extensive upheavals of the lower strata

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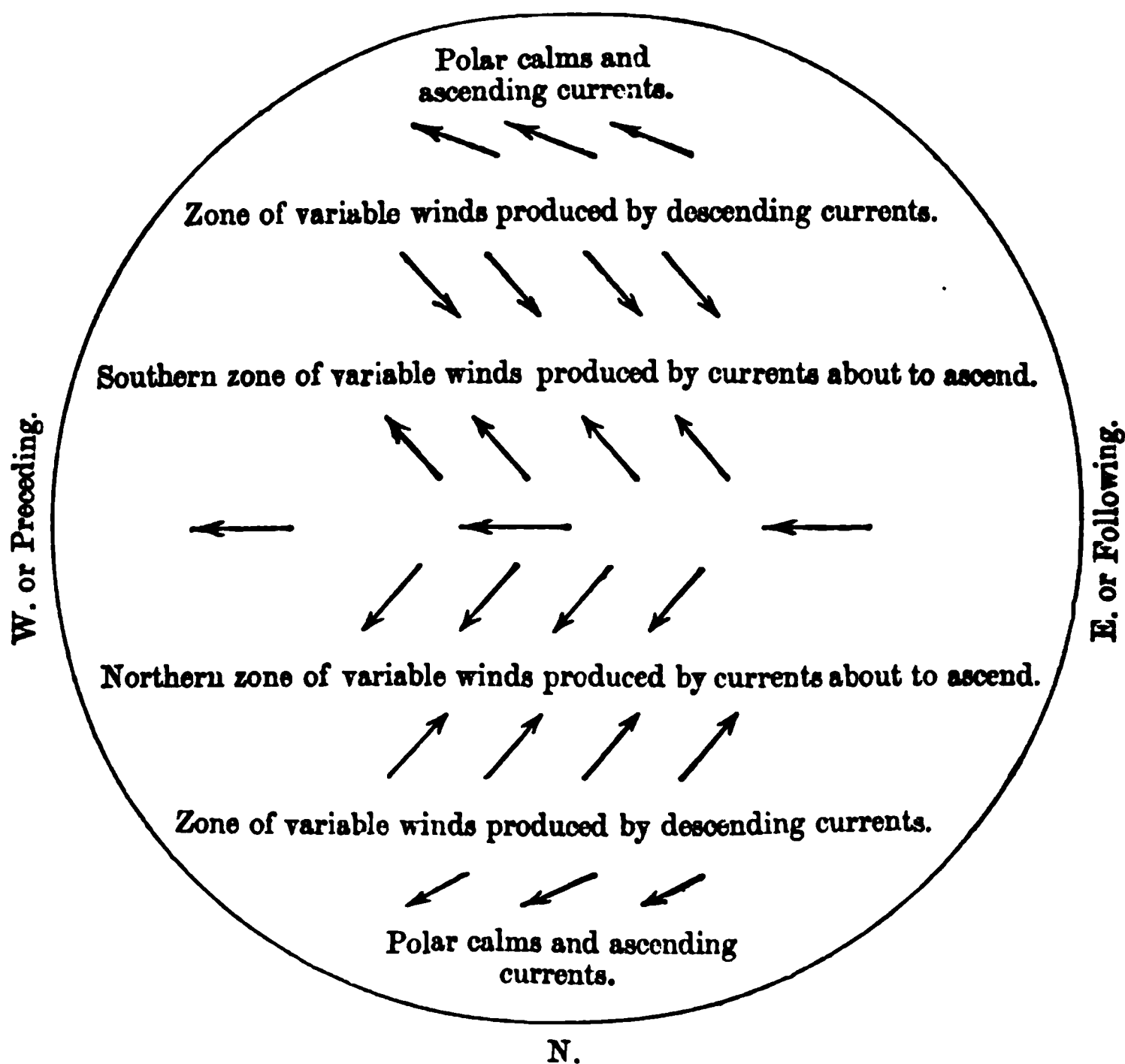


Diagram of the winds supposed to blow over the photosphere of the sun.

of the outer atmosphere, which, however gently they may begin, we have seen will terminate in a cyclone. Hence the two belts of spots on either side of the equator. In somewhat higher latitudes, the equatorial current having ascended, and, as it were, split the polar current into two sheets, has diverted one of them along the surface of the photosphere. In these regions, therefore, there is a constant wind blowing over the photosphere from the north-west in the northern hemisphere, and from the south-west in the southern. Accordingly, in the two zones of spots there are probably variable winds blowing over the photosphere, the polar and equatorial currents threading their way through each other, and both tending upwards, like the fingers of the two hands interlaced into one another. Meanwhile the equatorial current which had risen into the middle of the atmosphere over the zone of spots exchanges, in the northern hemisphere, its north-westerly direction, first for one due north, and then for one towards the north-east, as it ascends still higher.

But this temporary effect of its vertical motion will be lost, and it will again, when it ceases to ascend and advances only horizontally, direct its course to the north-west, until, in higher latitudes, the swifter rotation which its westward direction imparts to it no longer offers any sensible impediment to its sinking in the atmosphere*. And here it will be induced to do so by meeting in greater concentration the upper polar current, coming too from regions of slower rotatory motion, and therefore with a tendency to descend. Here, then, will end the space throughout which it has split the polar current into two sheets. It descends to the surface of the photosphere producing a zone of variable winds—in this case, however, caused by polar and equatorial currents which are both descending, and so unable to give rise to cyclones. Between this and the pole the equatorial current seems to be next the photosphere, and blows somewhat towards the pole, but chiefly from the east. There will be ascending currents about the poles; but they will breath upwards so gently, and over so great a space, that they are probably unequal to the task of heaving up the upper stratum of clouds in the vigorous way that leads to cyclones. Our conclusions may now be collected. The annexed diagram shows in one view the directions of the various trade-winds that seem to blow over the surface of the photosphere, and the prevailing character of the zones that separate them. The diagram is in the position in which the sun's disk is usually seen in a telescope.

76. We found that there is in the inner atmosphere a slight tendency to produce surface-currents from the equator towards the poles, owing to the greater escape of heat at the poles. But the influence of the winds in determining currents in the photosphere over which they sweep is probably so predominant, that both between and beyond the belts of spots they are able to determine the currents in the photosphere, those of middle latitudes being accordingly currents towards the east, whilst the equatorial and polar currents set in the opposite direction. Such currents would evidently conspire with the winds that blow over them to produce agitations in the photosphere. They would also contribute to that proper motion of the spots in longitude which has been observed.

77. We appear to be compelled to resort to some external cause to account for the periodicity of the spots. Among causes known to exist, that which seems to offer itself with most plausibility for our acceptance is a swarm of meteorites like those which visit us in November three times in a century, and those which visit us in other months every year†. To account for the periodicity of the spots, we must suppose the meteorites to describe their orbit in 11.11 years, the period of mutations of the spots. Hence

* Near the equator a swifter rotation tends to make a body fly outwards through the atmosphere; near the pole it tends to made the body retreat from the pole without much changing its level.

† [I find that Sir John Herschel in 1864 suggested such a swarm of meteorites operating in a different way, as a possible cause of this phenomenon. (See Quarterly Journal of Science, 1864, vol. i. p. 233.)—July 1868.]

the semiaxis major is 4.98 times that of the earth's orbit. The perihelion distance must be very small (say, 0.01) to admit of the swarm's grazing the sun's atmosphere at each perihelion passage. This would assign 9.95 to the aphelion distance, a quantity from which it cannot much deviate. Now the mean distance of Saturn is 9.54; so that we may safely conclude, if the explanation which is now offered is the correct one, that the meteorites in question were diverted into the solar system, by either Jupiter or Saturn, at no enormously remote period; just as the November meteors seem to have been brought in by the attraction of the planet Uranus in A.D. 126. At each perihelion passage some of the outlying members of the stream become entangled in the upper parts of the sun's atmosphere, dashing through it at a rate of about 41.4 kilometres per second*. The enormous friction they must undergo before they are brought to a state of relative rest will convert their immense *vis viva* into heat, which will be expended in raising the temperature of the upper strata of the part of the sun's atmosphere upon which they act. This part is of necessity more equatorial than polar, and is very much more equatorial than polar, except on the peculiar supposition, which we have no reason to select, that the plane in which the meteors move is very nearly perpendicular to the plane of the sun's equator. The heat imparted by the meteors to the sun's atmosphere therefore tends to diminish that defect of temperature in the upper parts at the equator which occasions the trade-winds of the sun. The influx of meteors, therefore, into the sun's atmosphere mitigates the violence of the trade-winds, and in this way enfeebles the cause of cyclones and of spots. Furthermore, except on the very improbable hypothesis that the axis-major of the orbit of the meteors lies exactly along the line of intersection of its plane with that of the sun's equator, the meteors must act more on one side of the equator than the other, and thus soften the trade-winds, and render the spots less frequent and extensive in one hemisphere than in the other. So that the hypothesis of a stream of meteors has the following points in its favour:—it is a *vera causa*; it accounts for two wholly distinct phenomena, the periodicity of the spots and their prevalence in one hemisphere more than in the other†; and it leads to such an aphelion dis-

* The velocity of the sun's equator is about 2 kilometres per second.

† [It also accounts for the approach of the zones of spots to the equator during the periods of minimum spot-frequency; inasmuch as when the current descending at the equator from great altitudes is enfeebled, the surface winds of middle latitudes, which have a tendency to cling to the photosphere, owing to their having a less rotation, will encroach further upon the equatorial current and will thus bring the junction between them, along which the spots lie, nearer to the equator.

If the decennial mutations of the spots be due to a current of meteors, this hypothesis ought to offer some indication of the cause of fluctuations of longer period, such as that of fifty-six years. This is perhaps to be sought in the great perturbation which the motion of the members of the stream must suffer from the attraction of Jupiter. This planet must act upon them with intense effect, since the planet and the meteors have nearly the same periodic time.—July 1868.]

tance of the orbit of meteors as assigns to them a position into which either Jupiter or Saturn could have brought them.

78. If these solar meteorites exist, they would seem to have had time to extend themselves round the greater part of their orbit, and leave the vacant space smaller at present than that which is occupied, so as to render the phenomenon depending upon their absence, viz. the *increase* in the number and size of spots, that which develops itself in the most marked manner. We may, then, perhaps presume that the epoch at which they came into the solar system was long before the year 126, though, cosmically speaking, a recent occurrence.

79. This appears the proper place to observe that the heat which is so lavishly dispersed by the sun cannot be kept up, as has sometimes been supposed, by the continual falling in of meteorites moving in orbits round him ; since if that were so, the outer parts of the solar atmosphere would be kept intensely heated, which is contradicted by all the phenomena. In the next part of this memoir, which will treat of other stars, I will offer what appears to me a possible account of the proximate source of solar heat.

PART II. OF OTHER STARS.

Section I.—*Of Solitary Stars.*

80. Observations with the spectroscope having apprised us of the presence in the sun and other stars of several of the elementary bodies with which we are familiar on the earth, we are bound to assume provisionally and until something offers to warrant a different belief, that those which are abundant on the earth and in the sun are abundant elsewhere also. Let us then consider how such differences as we must presume to exist between star and star would affect a body like the sun.

81. Star manifestly differs from star in mass ; they probably also differ in temperature. Let us therefore inquire how a great change in the sun's mass or in his average temperature would operate. Strange to say, an increase of his temperature would produce many of the same effects as a diminution of his mass. This is because the dilatation of the sun's bulk, and the consequent removal of the outer parts of his atmosphere to a greater distance from the centre would lessen the force of gravity upon them. In either case, therefore, the effect upon the atmosphere would be the same as if the gases constituting it became specifically lighter. They would all be able to maintain their footing with feebler molecular motions. In other words, each gas would rise in the atmosphere until the distance between its outer layer exposed by radiation to the intense cold of the sky, and the inner layer heated by the photosphere, interposes a space of such thickness as will, in obedience to the laws of conduction, reduce the temperature on the outside to the lower minimum which the gas can now endure. Accordingly, the spectral lines of a star, either hotter or less massive than our

sun, should be all of them more intense than the corresponding lines of the sun's spectrum. Moreover, many substances which by reason of the large mass of their molecules are unable to stand in the sun the low temperature* of the clouds of the photosphere, and are therefore confined to the regions within, are able, on a star which attracts with less force or whose centre is far removed, to pass through this obstacle and show themselves in the atmosphere above. Finally, such stars will be ruddy. The sun himself is a somewhat ruddy star, as may be seen by a glance at Tables II. and III. Both ends of his spectrum are subdued by lines. The yellow, orange, and scarlet are nearly as bright as they came from the photosphere; the green is sensibly shaded over by lines; the blue suffers somewhat more; the indigo about G and the crimson beyond B, very much; and the violet from G to H to such an extent, that it is difficult to find a spot where the full light of the photosphere appears to penetrate. The chemical rays beyond the violet are progressively more and more enfeebled as their wave-lengths shorten; so much so, that the fluorescent spectrum from several artificial sources is longer, and from some much longer, than the sun's. A similar deficiency seems to exist at the other end in that prolongation of the solar spectrum beyond A, which Sir David Brewster has dimly seen and succeeded in figuring. Now every encroachment upon the spectrum will be more marked when very dark lines become numerous, that is, in stars hotter, or of smaller mass; and if the lines themselves are pretty evenly distributed, it will subdue the different colours in proportion to their refrangibilities†. Ruddy stars, therefore, either have a less mass than our sun, or are more dilated by heat throughout the regions beneath the photosphere.

82. The consequences of the two other alternatives, of a star's mass being greater than the sun's, or of the temperature within the photosphere being less fierce, so that these regions are of less bulk, will be plain now. In such stars, some of the substances which range through the part of the sun's atmosphere above the photosphere are imprisoned within that luminous shell. Others of them, such as iron, calcium, and those of a like vapour-density, can only hold their ground while at a higher temperature, and would show faint though numerous lines in the spectrum. A few, such as sodium, magnesium, the substance that causes the line B (if this ray be of solar origin), and above all, hydrogen, would perhaps still continue dark. The lines of hydrogen, from its incomparably small vapour-density, would be so much the last to yield, that there is probably no star with gravity so intense as to produce any sensible impression upon them. And accordingly, in all very white stars which have been examined, these four lines stand out in extraordinary prominence.

83. It is now no longer a mystery why solitary stars are either white or of a red or yellow tinge. In all those cases in which the dilatation of the central parts by heat is so proportioned to the mass of the star as to render

* A variation of this temperature from star to star is another circumstance which must tell on the composition of the outer atmosphere.

† See § 52.

the force of gravity upon the outer atmosphere the same as it is upon the sun, the star will be equally white. The class of more brilliantly white stars with an almost violet gleam, such as Sirius and α Lyræ, are those with masses too great in proportion to their temperatures for this adjustment. And, on the other hand, those whose masses fall short of what the foregoing condition assigns, or, on the other side, whose temperatures are in excess, will, in proportion as they deviate from its fulfilment, have spectra more and more closed in upon that part in which the spectra of two incandescent bodies differ least in brightness when the luminous bodies are at nearly, but not quite, the same temperature—that is, upon the green, yellow, orange, and red rays, uniting into a tint which always inclines to either yellow, orange, scarlet, or crimson. The minute crimson stars which are met with here and there in the sky seem to be either very small stars, or stars enormously distended by heat. It is very desirable that the proper motion and parallax of these bodies should be inquired into when practicable, on the chance that some of them may be found to owe their colour to being very small, and therefore very close to us.

84. I need not say with what fidelity these many consequences of a change in the force of gravity in passing from star to star reproduce themselves in Mr. Huggins's observations. But before making the comparison, it will be well to consider rapidly what interfering causes may have to be taken into account.

85. We have hitherto spoken of the effects of the intensity of gravity in a star upon substances giving the same lines as we see in the sun. Our results are therefore subject to modification wherever the system of lines is itself changed. If, for example, the elements which give rise to the more prominent lines of the sun's spectrum are wanting in the stars, other lines, which perhaps are not, like those of the sun, pretty evenly spread over the whole spectrum, may take their place. If this should happen, some colours will be more absorbed by them than others; and this will tend to give to the star the complementary tint. In such cases the resultant effect will be mixed; the effect of the cause just mentioned being blended with that strengthening of the lines at the blue end of the spectrum which operates most when gravity on a star is weak. We shall presently find that this state of things, which would be improbable in solitary stars, may have been brought about in the case of the companions of some double stars. Or, again, elements which in the sun are free may in the stars be found only in a state of combination, and be either absent from the star's atmosphere or give rise in it to an entirely new set of lines. This* has perhaps been the

* Or the whole of the free hydrogen may have been thrown off into rings. If the star's rotation were such as to cast off the upper layer of hydrogen at his equator, and if the hydrogen could remain uncombined under these circumstances, fresh hydrogen diffusing upwards or flowing in from the poles would constantly fill the void; and each supply, as it arrived, would be in turn flung off, until in the end the whole of the free hydrogen of the star would be in this way drained away. The explanation in the text is, however, on many accounts the more probable one.

fate of hydrogen in α Orionis, β Pegasi, and the other stars in which there are no lines corresponding to the solar lines C and F. If, however, the lines that are in this way withdrawn be as few as the lines of hydrogen, their absence will not sensibly affect the colour of the star. And finally, such conditions may prevail upon particular stars as will enable a spectrum of the first order to present itself,—that kind of spectrum in which the usual scattered lines of a spectrum of the second order are replaced by a multitude of fine closely ruled lines arranged in groups of regularly shaded bands, so as to give to the spectrum of the gas the appearance of a fluted pillar. The bands in the spectra of α Orionis, β Pegasi, and some others probably arise in this way, and perhaps from some compound of hydrogen*. The lines constituting such bands will be affected by differences of the force of gravity in the same way as other lines, and will therefore, if distributed with tolerable impartiality over the spectrum, cooperate with them in producing that tendency towards a ruddy hue which belongs to stars that exercise a feeble attraction at their surfaces. It may be noted that in none of the figures which Mr. Huggins has given of the spectra of solitary stars with shaded bands, do they seem crowded abnormally over the yellow, orange, and red, but rather the reverse.

86. We are now in a position to appreciate the significance of the phenomena which the spectral examination of stars has brought to light. We can easily see why in the class of bluish-white stars of which Sirius and α Lyræ are types, stars at whose surfaces the force of gravity is greater than on our sun, “the dark lines they present in great number are all, with one exception, very thin and faint, and too feeble to modify the original whiteness of the light,” and why “the one exception consists of four very strong single lines, one line corresponding to Fraunhofer’s C, one to F, and another near G”†. There can be little doubt that the multitude of faint lines will prove to be due almost exclusively to iron and the substances near it in vapour-density, such as calcium, chromium, manganese, nickel, and cobalt, with of course sodium and magnesium. These, with the exception of sodium and magnesium, can produce only lines which are faint through the whole extent of the spectrum, since when attracted down with so much force as they are by the stars they cannot exist beyond regions of elevated temperature. And substances a little higher in vapour-density will be unable to endure even the chill of the photosphere, and therefore shrink within it. The violet and indigo rays being in these stars not subdued by lines in the same way as they are in the sun, gives to the whiteness of the stars a somewhat coloured tinge in eyes, like ours, accustomed to adjudge the sun’s light to be white.

* If this surmise is well founded, the compound must be sought among the compounds of hydrogen of low vapour-density, such as marsh-gas (mass of molecules 8), ammonia 8.5, water 9, olefiant gas 14, methylamine 15.5, sulphuretted hydrogen 17, phosphuretted hydrogen 17, hydrochloric acid 18.25, &c.

† Huggins’s Lecture before the British Association at Nottingham, published by Ladd.

87. On the other hand, Aldebaran is a good sample of a star which exerts less attraction at his surface than the sun, but which in other respects differs little from him. All the gases which cause solar lines can rise in the atmosphere of Aldebaran to colder heights than they can on the sun, and, as a consequence, they encroach more upon the violet end of his spectrum, and thus give to his light its rose-like tint. Another consequence is, that substances present themselves in the star's outer atmosphere with vapour-densities so high that the sun's superior attraction keeps them imprisoned within his photosphere. Mercury, mass of molecules 100; antimony, 122 (?); tellurium, 129; bismuth*, 210 (?).

88. All the foregoing appearances present themselves in α Orionis, which is therefore also a star on which the force of gravity is less than on the sun. They are found in α Orionis with the addition of a spectrum of the first order, one of whose bands has been observed to fluctuate in distinctness. We have reason to suspect, therefore, that the changes of brightness of this star, which is slightly variable, arise from some cause which alters periodically the temperature of the upper layer of that gas in its atmosphere from which the spectrum of the first order comes.

Section II.—Of Multiple Systems†.

89. Hitherto we have considered only the case of stars uninfluenced by one another. If, however, two stars should be brought by their proper motions very close, one of three things would happen. Either they would pass quite clear of one another, in which case they would recede to the same immensity of distance asunder from which they had come; or they would become so entangled with one another as to emerge from the frightful conflagration which would ensue as one star; or, thirdly, they would brush against one another, but not to the extent of preventing the stars from getting clear again. It is this last cause which we must now closely examine. After the stars disengage themselves they will be found moving in new orbits, which, if their motions before contact had been parabolic, will become elliptic. They will therefore return again and again, and at each perihelion passage will become engaged. If we take into account only the tangential resistance which the atmosphere of each presents to the motion of the other, we shall find‡ that the mean distance of the

* See Table I. p. 16.

† The following attempt to trace double stars, the solar system, and the amazing store of heat which we find in nature, to a proximate mechanical origin, is brought forward in the hope that it will prove of service in guiding inquiry, and in other ways; as an hypothesis always should, if not abused, which strikingly accords with many of the phenomena, and admits of being refuted or strengthened by future observations. I trust this will be accepted as a sufficient apology for offering to the scientific public what is as yet, of necessity, a speculation.

‡ These results appear from the following formulæ of elliptic motion:—

$$\frac{1}{a} = \frac{2}{r} - \frac{v^2}{\mu}, \dots\dots\dots(1)$$

$$\frac{2}{\beta} - \frac{2}{r} = \frac{v^2}{\mu} (\beta^2 - 1),$$

stars would be reduced. If the resistance acted only at the apse of the orbit, the diminution of the mean distance would be effected by a shortening of the aphelion distance exclusively, the perihelion distance remaining unaltered. But since the resistance is not confined to this spot, but acts also for some space on either side of it, the perihelion distance will at each passage undergo a slight decrease, which would inevitably cause the stars in the end to fall into one another, if the tangential resistance were the only force disturbing the orbits. But there will be normal forces also. The resistance to which each star is subjected in passing through the atmosphere of the other is a force neither directed through its centre, nor parallel to the tangent of its orbit, since an atmosphere is not a thing of uniform density. Since these forces are not parallel to the tangents of the orbits, they will produce normal components, which will be directed outwards; and since they are not directed through the centres of the stars, they will cause the stars to rotate, and these motions of rotation, which will take place in the same direction in which the stars are revolving in their orbits, will in the subsequent perihelion passages cause each star to sweep the atmosphere that opposes it downwards towards the other star while bursting through it. It will accordingly itself suffer an equal reaction, which will be another force normal to its orbit and directed outwards. Such forces will lengthen the perihelion distance, while they leave the mean distance undisturbed*. Accordingly the combined

where β is the perihelion distance, and the other letters have their usual significations.

A tangential resistance acting at any point of the orbit diminishes v , and therefore by equation (1) diminishes a , the mean distance.

To find its effect on β , the perihelion distance, transform the second equation by putting

$$\beta = p \cdot (1 - x); \dots\dots\dots(2)$$

whence, neglecting the higher powers of x , since we only seek the effect of a resistance acting in the neighbourhood of the perihelion where x is small,

$$\frac{1}{p} - \frac{1}{r} = x \left(\frac{v^2}{\mu} - \frac{1}{p} \right). \dots\dots\dots(3)$$

From equation (3) it appears that if v is diminished while p and r continue unchanged, x must increase, and therefore by equation (2) β , or the perihelion distance, is reduced.

* This appears from the foregoing equations by supposing p to receive an increment, while v and r remain unchanged. Equation (1) is not disturbed; in other words, the mean distance is unaffected. Equation (3) shows that x becomes less; and equation (2) that β , or the perihelion distance, is increased both by the increase of p and the diminution of x . The reverse effect upon β is produced by a decrease of p . Now p is increased by the normal forces from the time the stars touch up to the moment of the perihelion passage, and decreased during the second half of the transit. Accordingly β , the perihelion distance, is first increased and then diminished. If the stars behaved to one another like perfectly elastic bodies, these changes would be equal, and would cancel one another. But at each transit vis viva is converted into heat, in other words the stars do not behave like perfectly elastic bodies, and the mechanical forces elicited during the second half of the transit are feebler than those during the first. Hence there will on the whole be an increase of the perihelion distance.

effect of both forces will be at each revolution to shorten the ellipses in which the stars move, and at the same time to augment or reduce the perihelion distance, according as the effect of the normal or tangential component of the resistance preponderates. If the normal force carry the day, the stars will at successive passages gradually work themselves clear of one another, a result which may be very much promoted by the behaviour of the atmospheres.

90. If what I here venture to offer as a surmise with respect to the proximate cause of stellar heat and the origin of double stars, is what really took place, we must conclude the sky to be peopled with countless hosts of dark bodies so numerous, that those which have met with such collisions as to render them now visibly incandescent, must be in comparison few indeed. In the majority of those cases in which adequate collisions have taken place, the two stars must have emerged from the catastrophe, moulded into one, dilated by the conflagration to an enormous size*, and rotating. Occasionally, however, the circumstances of the collision must have favoured the disentanglement of the two stars from one another by a predominating influence in these cases of the normal force acting in the way that has been traced in the last paragraph. Wherever this happens, there is a prospect that a double star may form. The heat into which much of the previous *vis viva* of the two components has been converted will dilate both to an immense size, and thus enable the two stars gradually in successive perihelion passages to climb, as it were, to the great distance asunder, which we find in the few cases in which the final perihelion distance can be rudely estimated, a length comparable with the intervals between the more remote planets and the sun. While this is going on, the ellipticity of the orbits is at each revolution decreasing; but if the stars succeed in getting nearly clear of one another's atmospheres before the whole ellipticity is exhausted, the atmospheres will begin to shrink in the intervals between two perihelion passages more than they expand when the atmospheres get engaged, and will thus complete the separation of the two stars. When once this has taken place, a double star is permanently established.

91. It is a striking confirmation of this view to find that the astonishing phenomena witnessed last year† in T Coronæ were precisely what we should expect to arise towards the end of the process which has been described. The stars having been intensely heated by previous perihelion passages, and having begun to shrink, would, at ordinary times, present a spectrum subdued by an abundance of very dark lines; but immediately after one of the

* If any dependence is to be placed upon the records of Sirius's having formerly been a ruddy star, it would appear to argue that Sirius when he last met with a collision was heated only in the outskirts of his enormous mass: that these parts were so dilated as to render him a ruddy star, but that the store of heat laid up was so small that even within the little term of human history he has so cooled down as to have during it shrunk into an intensely white star.

† This was written in the spring of 1867.

last occasions upon which their atmospheres brush against one another, the outer constituent of their atmospheres, and the outer constituent alone, would be raised by the friction to brilliant incandescence, which would reveal itself by the temporary substitution of four intensely bright for four dark hydrogen lines in a spectrum which everywhere else continues to be filled with dark lines. And, moreover, these dark lines would for a while be rendered faint by the fierce heat radiated upon the outer parts of the atmosphere of each star by its companion*. It will be a matter of great interest to watch this star when sufficient time shall have elapsed to give a hope of seeing it double.

92. When a body of moderate dimensions enters the atmosphere of a great star, the resistance to which it is subjected will be very nearly the same per square metre over the whole of its front surface; but if it be of sufficient size to occupy a considerable height of the atmosphere through which it passes, it will be exposed to much more resistance beneath than above; and those conditions will have arisen which may terminate in a double star. The cases must be rare in which two stars that clash together happen to be of nearly equal mass. But when this does occur, the circumstances which are the *most* favourable to the formation of a double star have taken place. This seems to account for the very remarkable proportion of double stars which have nearly equal constituents. It would appear, too, that in this class we should expect to find those instances in which the perihelion distance is greatest, since it will be nearly the sum of the radii of the distended atmospheres of the two stars.

93. If two stars which are undergoing the process of formation into a double star, be of very unequal mass, the smaller one will be stripped at each perihelion passage of some of its atmosphere. All those parts which by the friction are brought into a state of rest relatively to the parts of the atmosphere of the larger star with which they come in contact, will, after the stars have been separated, settle down upon the larger star. They will, before the next perihelion passage, be replaced upon the smaller star by a fresh supply of the same gases diffusing upwards from beneath, and almost to the same height. When the stars come together again, this, in its turn, will be stripped off; and by a sufficient repetition of the process at successive perihelion passages several of the lighter constituents of the atmosphere of the smaller star will be transferred over to the larger. Upon the larger star this will not have any visible effect; the acquisition will

* γ Cassiopeix may perhaps be a similar system, in which the elliptic orbit has degraded either quite, or nearly into, a circular orbit, so as for the present to subject the outer layer of hydrogen to such a friction as keeps it constantly alight. If so, and if the stars are of materially unequal size, this must terminate, either by the atmosphere of the large star shrinking away from the companion, or by the companion's settling gradually down by a spiral motion through the atmosphere of its primary. If the latter be what is to occur, we shall have a splendid conflagration, the star first becoming intensely white, and afterwards deep red.

not even swell his bulk perceptibly *. But upon his satellite the consequences will be very remarkable. Hydrogen was the first gas to go ; then, in order, sodium, magnesium, calcium, chromium, manganese, iron. If the process has gone far enough to distil away all of these gases in the free state, the spectrum of the companion has been robbed of the principal lines found in solitary stars, to be replaced by an entirely new system emanating from substances of higher vapour-density, which, to judge from the spectra of the few coloured double stars that Mr. Huggins has succeeded in examining, are crowded abnormally over the scarlet, orange, yellow, and part of the green, giving to the companions of double stars those blue, violet, or greenish tints which are met with nowhere else. If the process be continued still further, more gases will be swept away, and the photosphere laid nearly bare ; as a consequence, the smaller star will appear white and nearly destitute of lines. This may have furnished that numerous class of double stars of which the companions are small and white.

94. No double star can come forth unless unequal pressure has acted so effectually on the smaller constituent as to communicate to it a swift motion of rotation. It is likely that cases may occur where the forces that accomplish this act with such inordinate strength that the cohesion of the smaller star is unable to withstand them, and there result two or more fragments spinning violently, and destined thenceforward to traverse slightly separate paths. This seems a not improbable account of such a multiple system as γ Andromedæ.

95. Upon the primary the consequences of the same violence would probably be entirely different. They would compel him to rotate at a great speed, perhaps so rapidly as to fling off his own equatorial parts†. These would form rings about him of the elliptic section which was investigated by Laplace ; at least, they would assume this form if they consisted only of gas, or of gas with cloud dispersed through it which is constantly dissolving and reforming, so as to keep always in a state of minute division, —so long, in fact, as the gaseous pressure caused by any accidental conden-

* A moment's consideration will make this plain. In fact, if the quantities of all the gases in the earth's atmosphere were doubled, it would add only $3\frac{1}{2}$ miles, or, more exactly, 5534 metres to its height. The result, after all disturbance had quieted down, would be the same as if a denser stratum of air of this trifling thickness were slipped in between the present atmosphere and the ground. To spectators from without, who would judge of our atmosphere chiefly as one which reaches upwards to a distance of about 200 kilometres (the height at which meteors begin to glow), the effect would be wholly insensible.

† This would be most likely to occur when the friction had acted chiefly on the superficial parts of the larger star, since under these circumstances a star might be enormously dilated without any considerable increase of its moment of gyration ; so that, *cæteris paribus*, such a star would rotate swifter than one whose bulk was due to the equal expansion of all parts.

Sirius may have been an instance of such a star (see footnote, p. 53). We have perhaps some reason, judging from the existing areolar momentum of the parts of the solar system, to suspect that it was in a considerable degree the case of our sun also.

sation in one part of the ring tends to disperse the gas which had accumulated there and so restore the balance, with better effect than the slightly superior attraction of the condensed knot can disturb it. The gases first cast off will soon be replaced on the star by a fresh supply of the same kinds diffusing upwards from below, to be in turn flirited off into the rings, if the star have retained sufficient rotation. It would seem, then, that the rings must of necessity consist of exceedingly light materials. These rings will obviously move nearly in the same plane as the companion, or fragments of the companion, as the case may be.

96. Now, as has been explained above, when the circumstances are such as favour the formation of a double star, the perihelion distance of the relative orbit is, after every revolution, on the increase, and the eccentricity on the decrease. If the two stars manage to get clear of one another before the eccentricity is worn out*, the process is complete, and a double star has come into being. But it must often happen, and is especially likely † where the companion is small, or has broken up into a number of fragments, that after the perihelion distance has become very considerable, but before the stars are quite clear of one another, the orbit will have degraded into a circular one. If this happen to any fragment of which the distance is at the time less than the radius of the distended primary, the two bodies must fall together and become one. But if the perihelion distance had attained a sufficient magnitude to place the fragment in one of the rings surrounding the primary, it will there play a very important part. It will by its attraction collect this ring about itself, and thus become covered with an enormous atmosphere, encircled by which it will continue to spin vigorously in the direction in which it moves in its now nearly circular orbit. If this rotation should be rapid enough, the new planet will itself throw off rings; and if any of these should afterwards become concentrated into satellites ‡, they will, like our moon, keep the same face

* The following eccentricities of double stars have been determined with more or less probability.

Star.	Eccentricity.	Authority.
61 Cygni	nearly circular.....	
ζ Cancri.....	0·23	Mädler.
η Cor. B.	0·29	Mädler.
ξ Ursæ Majoris.....	0·41	Mädler.
ζ Herculis.....	0·43	Mädler.
ρ Ophiuchi	0·47	Sir J. Herschel.
ξ Bootis	0·59	Sir J. Herschel.
δ Cygni	0·61	Hind.
ω Leonis	0·64	Villarcieux.
Castor	0·76	Sir J. Herschel.
γ Virginis.....	0·88	Sir J. Herschel.

† Since tangential resistance, which is what shortens the ellipse, acts with much more effect upon a small body than upon a large one of the same density.

‡ Can chemistry have intervened on the satellites, and formed heavy products out of materials originally very rare? or may the density and want of atmosphere of these bodies

always turned towards their primary. All this seems in a very remarkable degree to be what we see about us in the solar system.

be due to such a moderate change of temperature as, for instance, would convert the vapour of water into ice? It should be borne in mind that if the earth was at any time sufficiently hot, the ocean must have then formed an atmosphere of steam so vast, that it may perhaps have even reached to a ring which afterwards became the moon.

Possibly the giants of our system (Jupiter, Saturn, Uranus, and Neptune) owe their small density to their great mass, by reason of which they retain enough of their pristine heat to be still clothed in immense aqueous atmospheres.

If these surmises should prove to have any foundation, water was probably the material of rings thrown off originally by the sun, and is therefore not improbably an ingredient of the atmosphere of those dilated stars which do not exhibit hydrogen lines. (See footnote, p. 50.)

POSTSCRIPT.

[(*Continuation of the note on p. 26.*) I have during the present summer often received the impression that I saw several other faint bright lines in other parts of the spectrum, of which the principal is a line which is coincident with or very close to Kirchhoff's copper line of wave-length 52.23. It should be borne in mind that if such bright lines exist, they are due to constituents of the solar atmosphere which are eminently transparent to these rays, either from being intrinsically so, or from the excessive tenuity of the gas. Hence the gas adds in these rays, but only adds a little, to whatever brightness may be transmitted through it from beyond. It behaves like a faintish flame of very high temperature placed between the eye and a more conspicuous but less hot coal. Hence, if the background be the spectrum of the umbra of a spot, the bright line should be a *faint* streak across it. On the only occasion on which I had an opportunity of examining the spectrum of a spot, one of the rays I suspect to be bright lines presented this appearance to my eye.

Mr. Lockyer and Mr. Huggins have observed that *some* dark lines appear broader in the spectrum of a spot than in the spectrum of ordinary solar light. This is no doubt because the wings of these lines lose brightness which had before shone through them from beyond, and the duskier parts of them in consequence become dark enough to add to the breadth of the central black stripe. Wings appear to be always (except in the anomalous case of the iron line 49.61, which demands a careful experimental scrutiny) fainter than the central band. This may arise in either of two ways, either, 1°, because the gas is so rare, or else the perturbations which occasion the wings so evanescent, that the wings are in a considerable degree transparent, and much light from the photosphere streams through them; or 2°, because though opaque they come from a region hot enough to render them less dark than the central stripe. It is in the case of wings of the former kind only that the appearance recorded by Messrs. Lockyer and Huggins will present itself. Lines of which the wings are quite opaque ought, on the other hand, to appear narrowest when seen in the spectrum of the umbra of a spot, since the brighter parts of the wings would be then undistinguishable from the faint background, which would therefore seem to encroach upon them.—September 1868.]

COMMUNICATIONS RECEIVED SINCE THE END OF THE SESSION.

- I. "Second List of Nebulæ and Clusters observed at Bangalore with the Royal Society's Spectroscope ;" preceded by a Letter to Professor G. G. STOKES. By Lieut. JOHN HERSCHEL, R.E. Communicated by Prof. STOKES. Received July 20, 1868.

Bangalore, June 17, 1868.

MY DEAR SIR,—As it is now three weeks since I have been able to make any use of the Royal Society's telescope &c., owing to the setting in of the rainy season, and as circumstances oblige me shortly to move my quarters and start the instruments for the eclipse, it seems better to send all the observations I succeeded in getting since I last wrote up to the time of the change of weather.

I am sorry this second list is not a fuller one. But the fact is, that now that the planetary nebulæ list is exhausted, and the more conspicuous nebulæ, I find no small difficulty in seeing *anything*. It is true that the globular clusters alone form a long list for examination, a considerable number of which may be visible with the spectroscope ; but as they seem to show continuous spectra *without exception*, the interest attaching to this class is considerably diminished.

I have still a long list of nebulæ proper to examine ; but the proportion of these which exhibit monochromatic spectra seems very small—so small, indeed, that I cannot report a single new instance in this class. There are some conspicuous ones which will present themselves later in the year, among which one or two may possibly be recognizable as gaseous ; but the majority, I may say the large majority, seem otherwise, and therefore difficult of positive identification.

It will be necessary to despatch the instruments along with my camp equipage &c. in a fortnight or three weeks, as the spot selected for a station of observation is upwards of 300 miles distant from Bangalore. I cannot expect that the return journey will have been effected before the middle or end of September. For the next three months, therefore, the nebulæ must be allowed to pass unchallenged. Whether I shall have the opportunity of continuing my search then, or not, must depend on circumstances which I cannot now foresee. Should the Society see fit to allow the instrument to remain in my hands for a few months longer, I will at least undertake to prosecute the search during such intervals of leisure as my other duties may leave at my disposal. But at present, perhaps, the question of the disposal of the instrument after the eclipse is not an urgent one.

The station selected for the eclipse observations is Jamkandi (vulg. Jum-khundee), about midway between Belgaum and Sholapore. The selection has been determined chiefly by the small rainfall of that district, and by

the spontaneous offers of assistance of H. H. the Chief of Jamkandi. I have no great fears about the weather ; but of course one may be disappointed. There is certainly a better chance there than at any point on the east coast.

Yours truly,

J. HERSCHEL.

Nos. 2068.

3642.

4132.

4211.

4268.

4270.

4275.

4287.

4296.

4307.

4311.

These are all of the class globular clusters. They are all described as very bright or bright, and most of them well resolved. Their spectra were recognized, without exception, as *continuous*, and that generally without difficulty ; but 2068 and 4211 were very faint in the spectroscope. The spectrum of 4307 was unusually bright, and was visible from about D to about F. No further details appear necessary about these.

The following "planetary nebulae" have been recognized as showing monochromatic spectra :—

No. 1843. Faint monochromatic light at $D + 2.22$ (May 9th).

No. 2076. Faint monochromatic light, distinct, but not measured (May 10th).

No. 1565. Faint monochromatic light, spectrum not seen until the sixth time of examination (May 14th).

No. 1801. Monochromatic spectrum suspected only (May 16th).

No. 3229. "Very bright ; round ; barely resolvable." Spectrum clearly continuous.

No. 3606. "Very remarkable ; very bright ; very large." Continuous spectrum distinctly.

No. 4189. "Bright ; pretty large ; partially resolved." Continued spectrum (May 16th).

No. 4421. "Bright ; small ; well resolved." Continuous spectrum seen pretty easily — ? very bright ; pretty large (May 18th).

No other nebulae have been recognized (since the previous report) as having *positively* either monochromatic light or continuous spectra ; but the following list shows those which are *believed* to be *continuous*.

No. 3227. "Very bright ; large."

No. 3397. "Very bright ; pretty small."

No. 3477. "Very bright ; small ; round ; a star of the 10th magnitude following."

No. 3504. "Very bright ; pretty small ; round."

No. 3706. "Remarkable ; very bright ; very large."

No. 2341. "Bright ; pretty large."

No. 2586. "Bright ; pretty large ; barely resolvable."

No. 3214. "Bright ; pretty large."

The evidence in the case of these is purely negative. They have been brought on the slit with sufficient precision to make it in each case almost a certainty, taking into consideration their apparent brightness, that had their light been monochromatic in the sense in which other similar nebulae are so, it must have been recognized as such ; whereas, if dispersed, analogy would forbid any expectation of detecting the feeble continuous spectrum. In each case I have satisfied myself that the inference was a legitimate one before recording it, both by testing the focal adjustment on a neighbouring star and by repeating (if necessary) the adjustment in respect of direction, until the case appeared a hopeless one.

Some instances, in which the presumption was not so strong as to seem to justify the inference, are omitted from this list.

Several nebulae, the measurements of whose spectra were given in my previous list, were reexamined. I am rather inclined to believe that the position of the principal light is not quite constant for all. The newer and more careful measurements certainly throw doubts upon some of the earlier ones. Nevertheless I have reason to believe that the discordances are not wholly due to inaccurate determination. But on this and other points I hope to be able to write more fully on a future occasion, when a larger number have been re-measured. The following were reexamined.

No. 4066. Very bright spectrum ; 3 lines certainly ; faint continuous spectrum suspected.

No. 4510. Very bright spectrum ; 2 lines only seen, both hazy ; comparatively distinct continuous spectrum of some length on both sides of the principal line.

No. 4390. Seen easily in a bright field ; 3rd line seen certainly.

No. 4361. Very large ; 2 lines only ; ill defined ; decided continuous spectrum at the brightest point (*not* stellar).

P.S.—I have throughout used the word “monochromatic” in preference to “linear” intentionally, because, though not *strictly* correct, it appears more so than the latter. Unless a cylindrical lens is used, or unless the object examined is so great as to be partially stopped out by the slit (generally a very wide one), the term “linear” seems purely conventional. I have reason to believe that some of my earlier measures were erroneous, partly through not realizing the “play,” so to speak, of the small image within a wide slit, the *linear* character being illusory. The term has become so intimately associated with spectral appearances, that one is apt to forget how completely *mechanical*, and therefore conventional a characteristic of the quality of light is expressed by it. Thus I find that, in more than one instance, expectation is entertained of possible bright “lines” being seen during the coming eclipse, without the intervention of a slit, as though this were an inherent quality in light itself.

II. "On the Lightning Spectrum." By Lieut. JOHN HERSCHEL, R.E.
Communicated by Prof. STOKES. Received August 8, 1868.

I have had two or three opportunities of seeing this spectrum to advantage of late. The storms at the period of the setting in of the south-west monsoon here are very frequent, and supply for a time almost incessant flashes, many of which are of course very brilliant. The first time I examined the light in the spectroscope I had no idea of measuring, but was content to realize the principal facts of a continuous spectrum crossed by bright lines; but subsequently I made several attempts (with some success) to obtain measures. That I was unable to do more in this line is due partly to the difficulty of utilizing the short-lived appearance, partly to that fascination of waiting for "one more" bright flash to verify the intersection, which can only be thoroughly appreciated by the aid of a similar experience.

The principal features of the spectrum are a more or less bright continuous spectrum crossed by numerous bright lines, so numerous indeed as to perplex one as to their identity. This perplexity is increased by the constantly changing appearance due to a variable illuminating power. This variable character of the appearances is unquestionably the peculiar feature of the spectrum. It is not that the whole spectrum varies in brightness in the same degree, but that the *relative* intensities are variable, not only among the various lines, but between these and the continuous spectrum. The latter is sometimes very brilliant; and when that is the case, the red portion is very striking, though in general the spectrum seems to end abruptly at $D + 0.34$ ($E = D + 1.38$, Kirchhoff's $120.7 = D + 0.55$).

There is one principal line which I found equal to $D + 2.20$ as the result of five independent measures. The probable error of this value is about ± 0.02 . The general mean of all my measures of the principal *nebular* line (obtained from twelve different nebulae) is 2.18 , with a probable error of about ± 0.02 . I have therefore very little doubt that these are the same, viz. the nitrogen line identified in the case of nebulae by Mr. Huggins. This line in the lightning spectrum is narrow and sharply defined, and is conspicuously the brightest, except as noted below.

The next in prominence is situated about $D + 3.58$ ($F = D + 2.73$, Kirchhoff's $232.5 = D + 3.50$). It is broader and less vivid, and not so well defined at the edges.

There are several other conspicuous lines, but none comparable to the first. I noticed a sharp line in the red, but did not get a measure.

I said that at $D + 0.34$ the continuous spectrum ends abruptly. A faint continuation is, however, seen frequently in bright flashes, very bright ones bringing out a brilliant red end crossed by a bright line.

The whole of the ordinary spectrum *seems* green and blue, or rather greenish blue; but as the usual prismatic order of colours is recognizable in bright flashes, it is to be inferred that the region from E to F is so much

brighter as to give the character in question. What strikes one most, however, is the varying relative brightness of the continuous and linear spectra; sometimes the lines are scarcely seen, and sometimes very little else is seen. This may be nothing more than an illusion; but in the absence of any certainty that it is so, the impression left on the mind is worth recording.

The difficulty of discriminating between the many less prominent lines is immensely increased by the momentary character of the phenomenon. Before the mind has selected an individual, the feeble impression on the retina has vanished; and before another flash succeeds, the memory of the half-formed choice has vanished with it, and there is nothing on which to found a selection. Otherwise it would be easy enough to measure many more lines.

III. "Products of the Destructive Distillation of the Sulphobenzolates.—No. II." By JOHN STENHOUSE, LL.D., F.R.S., &c. Received September 8, 1868.

In a paper published by me in the Proceedings of the Royal Society, 1865, I described the manner of preparing sulphobenzolate of sodium and the products of its destructive distillation in a copper retort. These were chiefly sulphide of phenyl and a crystalline substance, of which too small a quantity was obtained to enable me properly to examine it.

As I wished to procure these products in larger quantities, instead of employing small copper retorts, which were rapidly destroyed, I conducted the operation in tolerably large cast-iron ones heated in a gas-furnace, and found that they were not sensibly corroded even after a great number of distillations. The quantity of sodium-salt decomposed in each distillation was about 200 grammes.

The oily products obtained by this process, after separation from the supernatant watery layer, were introduced into a copper retort having a bent glass tube luted into the neck and redistilled, the retort being heated to redness towards the close of the operation. In this way a considerable amount of impurity was removed. The bright yellow-coloured oil was then rectified in a glass retort. It began to boil at 80° C., and rose rapidly to 165° C., between which and 180° C. about one-fourth of the liquid came over. The temperature then again rose rapidly to 290° C., and from 290° to 300° a large quantity of nearly pure sulphide of phenyl distilled. The small quantity of dark-coloured residue in the retort was poured into a beaker, where it became semisolid on cooling from the deposition of the crystalline substance I have before mentioned*.

Phenyl-Mercaptan.

The portion boiling between 165° C. and 180° C., on being repeatedly rec-

* Proc. Roy. Soc. vol. xiv. p. 353.

tified, gave a liquid boiling constantly at $172^{\circ}5$. This was subjected to analysis with the following results :—

I. .5345 grm. oil gave 1.280 grm. carbonic anhydride and .265 grm. water.

	Theory.	I.
$C_6 = 72$	65.45	65.33
$H_6 = 6$	5.45	5.51
$S = 32$	29.10	
<hr/> 110	<hr/> 100.00	

The numbers obtained correspond very closely with the formula $C_6 \begin{smallmatrix} H_6 \\ H \end{smallmatrix} \} S$, *phenyl-mercaptan*. When pure it is colourless, having an aromatic but somewhat alliaceous odour, although not at all offensive. It has a high refractive index, and boils at $172^{\circ}5$ C. Insoluble in water, but readily miscible with alcohol, ether, and benzol. It is readily oxidized, yielding bisulphide of phenyl. This takes place even when exposed to the air in imperfectly closed vessels.

Vogt* has described an oil which he obtained by the action of zinc and dilute sulphuric acid on sulphobenzolic chloride, $C_6H_5ClSO_2$, and calls *benzyl-mercaptan*, C_6H_5S . He says it boils at "about 165° C., and has an extremely offensive odour."

Otto†, by the action of nascent hydrogen on sulphophenylenethylene, $C_6H_4SO_2 \begin{smallmatrix} \\ C_2H_4 \end{smallmatrix} \}$, obtained phenyl-mercaptan, which he showed to be identical with Vogt's benzyl-mercaptan, but the boiling-point is "between 170° C. and 173° C."

From the description given by Vogt and Otto, it is evident that the phenyl-mercaptan obtained by the destructive distillation of sulphobenzolate of sodium in an iron retort is identical with theirs, and that the offensive odour ascribed to it by Vogt is due to some slight impurity, probably arising from the phosphoric chloride employed in the preparation of the sulphobenzolic chloride.

Phenyl-mercaptide of lead.—On adding acetate of lead to an alcoholic solution of the mercaptan, a bright yellow crystalline precipitate was formed. This when heated fused, and at a higher temperature was decomposed.

Phenyl-mercaptide of copper was prepared in a similar manner, substituting acetate of copper for acetate of lead. On exposure to the air in a moist state it became oxidized, forming cupric oxide and bisulphide of phenyl, C_6H_5S , which may be extracted and crystallized from boiling spirit.

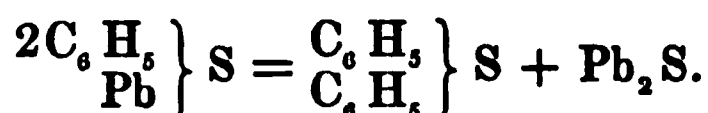
The compounds with mercury, chloride of mercury, and silver are identical with those described by Vogt.

* Ann. der Chem. und Pharm. vol. cxix. p. 144.

† Ibid. vol. cxliii. p. 211.

Decomposition of Phenyl-mercaptide of Lead.

When dry phenyl-mercaptide of lead is heated to a temperature superior to 280° C. it is decomposed, an oil distils over, and plumbic sulphide is left in the retort. This oil boils constantly at 292°·5 C., and corresponds in all its properties with *phenylic sulphide*. By oxidation it yielded a substance crystallizing in oblique prisms, and which was proved to be sulphobenzolene. The action of heat on the lead mercaptide is therefore as follows :—



This decomposition is especially interesting, as it proves the body obtained by the destructive distillation of the sulphobenzolates to be the true phenylic sulphide.

Bisulphide of Phenyl.

When the pure mercaptan was mixed with about an equal bulk of concentrated sulphuric acid, the latter acquired a dirty purple colour, and after the lapse of some time, with occasional agitation, became hot and gave off sulphurous anhydride. When cold the upper layer solidified to a mass of crystals, which, on being separated from the acid, washed with water, and crystallized several times from spirit, gave a white crystalline substance. It was dried *in vacuo* and analyzed.

I. ·308 grm. substance gave ·745 grm. carbonic anhydride and ·133 grm. water.

		Theory.		I.
C ₆ = 72	66·05	65·98
H ₅ = 5	4·59	4·80
S = 32	29·36		
	<hr/>	<hr/>		
	109	100·00		

The analysis corresponds to the formula C₆H₅S, *bisulphide of phenyl*. It is insoluble in water, soluble in alcohol, very soluble in ether, benzol, and bisulphide of carbon, melts at 61° C. (Vogt* gives 60° C. as the melting-point of his bisulphide of benzyl). It is again reduced to the mercaptan by zinc and dilute sulphuric acid, or better, by digestion with hydriodic acid and amorphous phosphorus.

As but traces of phenyl-mercaptan were obtained on decomposing the sulphobenzolate of sodium in a copper retort, while a considerable portion of the distillate consisted of the mercaptan when an iron one was used, I was induced to make some experiments in order to see whether it was the copper which caused this difference. This I ascertained to be the case by distilling sulphobenzolate of sodium mixed with copper cuttings in an iron retort, when the proportion of the mercaptan to the sulphide was comparatively small, and the surface of the copper was converted into cupric sulphide. Granulated zinc produced a similar result.

* Ann. der Chem. und Pharm. vol. cxix. p. 149.

Phenylene Sulphide.

The dark-coloured residue in the retort which did not come over at 300° C. was distilled from a copper retort having a bent glass tube luted into it. The orange-coloured distillate, on standing a few days, deposited a considerable quantity of large transparent plates. These were drained as much as possible, and freed from adhering oil by pressure between paper. The partly purified crystals were extracted with hot spirit to remove the bisulphide of phenyl and other impurities, and then crystallized from benzol or bisulphide of carbon. A crystallization from spirit rendered them quite pure.

I. .243 grm. gave .592 carbonic anhydride and .086 grm. water.

II. .221 grm. gave .539 carbonic anhydride and .076 grm. water.

III. .301 grm. gave .652 baric sulphate.

	Theory.	I.	II.	III.	Mean.
C ₆ = 72 ..	66.67	66.45	66.53	66.49
H ₄ = 4 ..	3.70	3.93	3.82	3.87
S = 32 ..	29.63	29.71	29.71
108	100.00				

The analyses of this substance lead to the formula C₆H₄S, *sulphide of phenylene*. It crystallized in long lustrous prisms, which are quite transparent and colourless. Is insoluble in water, slightly soluble in cold alcohol (about 400 parts), but more so in hot. It is far more soluble in benzol and bisulphide of carbon, from the latter of which it may be obtained in fine crystals, sometimes half an inch or more in length. It melts at 159° C. and solidifies at 153° C. It dissolves in concentrated sulphuric acid, forming a solution of a magnificent purple colour, and which when largely diluted with the concentrated acid appears purplish red. On the addition of water the colour disappears, and a crystalline precipitate is produced, apparently the unaltered phenylene sulphide. With concentrated nitric acid a reaction takes place, red fumes are evolved, and a crystalline substance produced, probably a nitro-substitution compound. This I have at present under investigation.

Sulphobromide of Phenylene.

When crystals of the phenylene sulphide are exposed to bromine-vapour they combine with it and turn black, forming sulphobromide of phenylene. The best method, however, to obtain this pure, is to add perfectly dry bromine in slight excess to a cold saturated solution of sulphide of phenylene in dry bisulphide of carbon, when the compound is precipitated in the form of minute black prisms. These are immediately collected, washed with cold dry carbonic disulphide, pressed, and the bisulphide of carbon removed by placing them under the receiver of an air-pump, rapidly exhausting the air, and allowing it to reenter several times. It is then

66 *On the Destructive Distillation of the Sulphobenzolates.* [Recess,

weighed and treated with excess of pure solution of sulphurous acid, to convert the combined bromine into hydrobromic acid. This is determined as bromide of silver. By this method the following result was obtained :—

·821 grm. substance gave 1·154 grm. argentic bromide. This gives 59·81 per cent. combined bromine. The formula $C_6H_4SBr_2$ requires 59·70 per cent. bromine. This substance is therefore analogous to the corresponding ethylene compound discovered by Carus*.

The sulphobromide crystallizes in black prisms, which slowly give off bromine on exposure to dry air, and are rapidly decomposed by moisture with evolution of hydrobromic acid. They are tolerably soluble in carbonic disulphide and tetrachloride.

Phenyl-hyposulphurous Acid.

Amongst the reactions which phenylic sulphide gave with various reagents†, that with sulphuric acid was particularly interesting. On treating pure sulphide of phenyl with an equal bulk of concentrated sulphuric acid, the oil changed first to a fine red colour, and as the heat increased it became purple, and ultimately dissolved, giving off traces of sulphurous acid. The compound thus produced, when cold, was semifluid, and gradually absorbed moisture from the air, becoming a semisolid crystalline paste. This was dissolved in a large quantity of boiling water, neutralized with pure baric carbonate, filtered from the insoluble sulphate, and the solution of phenyl-hyposulphite of barium evaporated until a pellicle formed on the surface, and then allowed to cool.

The crusts which come out consist of microscopic crystals. These, after one or two recrystallizations from boiling water, were dried at 100° and submitted to analysis.

·419 grm. substance gave ·175 grm. baric sulphate.

·334 grm. substance gave ·141 grm. baric sulphate.

·320 grm. substance gave ·312 grm. carbonic anhydride and ·075 grm. water.

		Theory.	I.	II.	III.
$C_6 = 72$..	26·13	26·61
$H_7 = 7$..	2·54	2·61
$Ba = 68·5$..	24·85	24·56	24·82	
$S_2 = 64$..	23·24			
$O_4 = 64$..	23·24			
		<hr/>			
		275·5	100·00		

These analyses agree tolerably well with the formula $C_6H_7BaS_2O_4, H_2O$, which I propose to call baric phenyl-hyposulphite. I have prepared the copper salt, which likewise forms crystalline crusts ; but neither the calcium nor sodium salt crystallizes as well as the barium.

* Ann. der Chem. und Pharm. vol. cxxiv. p. 113.

† Prcc. Roy. Soc. vol. xiv. p. 354.

It is not improbable that ethylic and methylic sulphides, &c., when treated with concentrated sulphuric acid, would form corresponding compounds.

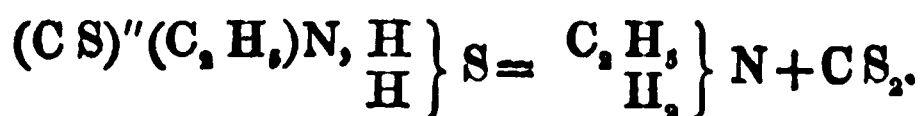
I cannot conclude this paper without acknowledging the very efficient aid I have received from my assistant, Mr. Charles E. Groves, in the preceding investigation.

IV. "Compounds Isomeric with the Sulphocyanic Ethers.—II. Homologues and Analogues of Ethylic Mustard-oil." By A. W. HOFMANN, Ph.D., M.D., LL.D. Received September 11, 1868.

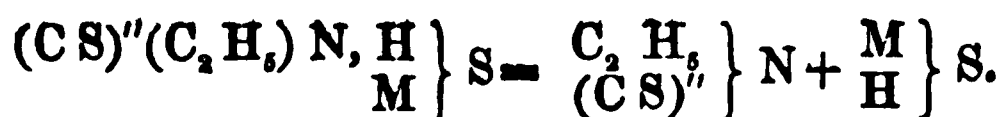
In a former Note submitted to the Royal Society some months ago*, I have sketched a series of compounds isomeric with the well-known sulphocyanic ethers; today I shall endeavour to delineate more in detail the bodies the existence of which I then pointed out.

In order to prepare these substances, which, from their analogy with the essential oil of mustard-seed, I have designated by the name of *mustard-oils*, the monamines were in the first place treated with bisulphide of carbon; the alcohol-sulphocarbonates of the monamines thus formed were then submitted to the action of heat and converted, by the loss of 1 molecule of sulphuretted hydrogen, into sulphuretted ureas, which were finally deprived of 1 molecule of monamine by means of anhydrous phosphoric acid. Circuitous as this process may appear, it has the merit of being a general one, furnishing, in fact, the mustard-oils both of the fatty and aromatic series. When working with fatty substances, however, the method may be very considerably curtailed. Let it be *ethylic mustard-oil* that is to be prepared.

Even on the threshold of my inquiry I had hoped to see ethyl-sulphocarbamic acid split up into sulphuretted hydrogen and ethylic mustard-oil; experiment, however, proved that the metamorphosis assumes another form, the acid yielding as products of decomposition its two components, ethylamine and bisulphide of carbon.



But a transformation which the free acid refuses, the metallic ethyl-sulphocarbamates undergo without difficulty, more especially in the presence of an excess of the metallic solution, a metallic sulphide being formed.



On adding, for instance, nitrate of silver to a solution of ethyl-sulphocarbamate of ethylamine, such as is produced by the action of bisulphide of carbon upon ethylamine, a white precipitate of ethyl-sulphocarbamate of silver is formed, nitrate of ethylamine passing into solution. After some time,

* Proceedings, vol. xvi. p. 254.

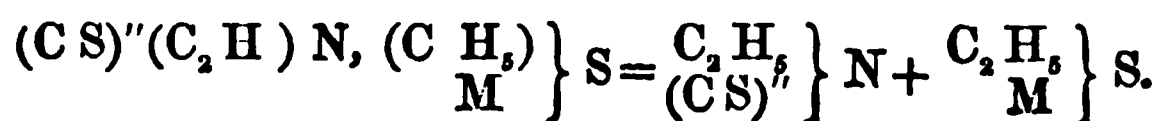
however, this precipitate blackens, even at the common temperature, more rapidly on heating, with formation of sulphide of silver. Simultaneously the odour of ethylic mustard-oil becomes perceptible; and if the liquid be heated to ebullition, this oil distils in large quantity with the vapour of water. The disengagement of sulphuretted hydrogen, which is observed at the same time, belongs to a secondary reaction, the unstable hydrosulphide of silver (which is formed in the first instance) splitting up into sulphide of silver and sulphuretted hydrogen.

In this experiment no excess of silver should be used. Ethyl mustard-oil, more especially upon protracted ebullition, exchanges its sulphur for oxygen, thus giving rise to the formation of cyanate of ethyl, easily recognizable by its fearful odour. Ultimately this ether is entirely decomposed into carbonic acid and ethylamine; after some time the solution contains nothing but nitrate of ethylamine.

Most of the metallic ethyl-sulphocarbamates, more especially the copper and mercury salts, behave exactly like the silver compound. I have almost invariably employed mercuric chloride for preparing ethyl-mustard-oil. In this case the hydrochlorate of ethylamine which is produced unites with the excess of corrosive sublimate to form an insoluble compound. Accordingly the ethylamine, which in this reaction is separated as salt, exists partly in solution, partly in the precipitate; it is easily recovered by treating with caustic soda the residue which is left after the mustard-oil has been obtained by distillation. When working with pure ethylamine, one half of the base may thus be regenerated for a new operation.

But it would be useless to employ pure ethylamine for this purpose. The crude mixture of bases, which is obtained by allowing alcoholic ammonia to stand for some time with iodide of ethyl and distilling the iodides thus formed with an alkali, is very well adapted for this operation. This mixture, as is well known, contains, together with ammonia, the primary, secondary, and tertiary monamines of the ethyl-series.

I have satisfied myself, in the first place, that diethylamine is just as easily converted into ethylic mustard-oil as ethylamine. The experiment was made with absolutely pure diethylamine prepared by means of diethyl-oxamic ether. Bisulphide of carbon, more especially in alcoholic solution, acts with great energy upon diethylamine, diethyl-sulphocarbamate of diethylamine being formed, which, when treated by a metallic salt, furnishes a metallic diethyl-sulphocarbamate together with a salt of diethylamine. On ebullition, the former is converted into ethylic mustard-oil; but instead of the metallic hydrosulphide generated in the analogous metamorphosis of ethylamine, in this case a mercaptide is formed.



I should not leave unmentioned, however, that the formation of mercaptide is still to be further proved by direct experiment. On working with

mercuric chloride, the precipitate which remains after the ethylic mustard-oil is separated by distillation, neither dissolves in boiling water nor in boiling alcohol. If this precipitate were pure mercaptide of mercury, it should be crystallizable from boiling alcohol. Probably it is a double compound of mercaptide and chloride; at all events, I have established by experiment that mercaptide and chloride of mercury unite to form a compound perfectly insoluble both in water and alcohol, even on boiling.

Triethylamine also unites with bisulphide of carbon; but this compound, as might have been expected, yields no longer any mustard-oil.

Ultimately, as regards the ammonia, which invariably occurs in the crude mixture of the ethyl-bases, its presence rather increases than diminishes the quantity of mustard-oil which is formed. This ammonia remains in the residue in the form of a salt, together with salts of ethylamine, diethylamine, and triethylamine; and a corresponding quantity of the primary and secondary ethyl-bases is converted into mustard-oil, the yield of which may thus be very considerably augmented.

The mercuric salts also attack ethylic mustard-oil, although much less easily and rapidly than nitrate of silver. A large excess of chloride of mercury, however, should be avoided. If the ethylamine be prepared from iodide of ethyl, it is convenient to employ the mercury salt and the mixture of bases in such proportions that 1 molecule of mercuric chloride reacts upon the bases generated by means of 2 molecules of iodide of ethyl.

In an experiment carried out upon rather a large scale a quantity of ethylic mustard-oil was obtained amounting to from 60 to 70 per cent. of the theoretical proportion which might have been expected from the weight of iodide of ethyl employed.

Ethylic Mustard-oil.

As to the physical properties of ethylic mustard-oil, I have not to add anything to what I have formerly stated, except a determination of the gas-volume weight, which was taken in the vacuum of the barometer at the temperature of 185° (in the vapour of boiling aniline).

	Referred to hydrogen.		Referred to air.	
	Theory.	Experiment.	Theory.	Experiment.
Gas-volume weight	43·5	43·75	3·02	3·03

When operating with the isomeric sulphocyanide of ethyl, the following numbers were obtained:—

	Referred to hydrogen.		Referred to air.	
	Theory.	Experiment.	Theory.	Experiment.
Gas-volume weight of sulphocyanide of ethyl (determined in the vapour of boiling water)	43·5	42·84	3·02	2·98

Methylic Mustard-oil.

I formerly obtained the methyl-compound as an oily liquid boiling at 120°, and powerfully smelling of horseradish. When a somewhat larger quantity of this body was prepared according to the process above described, the liquid, after distillation with the vapour of water, solidified to a splendid crystalline body.



Boiling-point 119°; fusing-point 34°; solidifying-point 26°.

	Referred to hydrogen.		Referred to air.	
	Theory.	Experiment.	Theory.	Experiment.
Gas-volume weight of methylic mustard-oil (determined in the vapour of boiling water)	36.5	37.89	2.52	2.61

Amylic Mustard-oil.

I have also prepared the amyl-compound on a larger scale by a slight modification of the process above described. Instead of separating the compound from the mercury precipitate obtained in the dilute alcoholic solution at once by distillation, it is advisable to return the vapours, condensed by a cooler, for some time to the boiling mixture. When the reaction is complete, the sulphide of mercury is filtered off, the amylic mustard-oil precipitated by water, dried over chloride of calcium, and ultimately purified by distillation. The odour of the compound is analogous to those of the methyl- and ethyl-body, but less pronounced.



Boiling-point 183° to 184°.

	Referred to hydrogen.		Referred to air.	
	Theory.	Experiment.	Theory.	Experiment.
Gas-volume weight of amylic mustard-oil (determined in the vapour of boiling aniline)	64.5	63.42	4.48	4.40

Tolylic Mustard-oil.

As has already been pointed out, the new process cannot be used for preparing the mustard-oils of the aromatic series, at all events in the narrower sense of the word. I may mention, however, that tolylic mustard-oil may be readily obtained by the process which I had formerly used for producing the corresponding compound of the phenyl-series. The ditolyl-sulphocarbamide required for this purpose is known; it was examined some years ago by M. Sell. If this body be distilled with anhydrous phosphoric acid, aromatic vapours are evolved which may be condensed to a yellowish oil rapidly assuming the crystalline form. The product of distillation

generally retains a minute quantity of ditolyl-sulphocarbamide, which may be separated by recrystallization from ether, tolylic mustard-oil being extremely, ditolyl-sulphocarbamide but slightly soluble in this liquid. The mustard-oil of the tolyl-series readily crystallizes in beautiful white needles, attaining often the length of several centimetres; they are easily soluble in alcohol, slightly so in water. Tolylic mustard-oil possesses to an almost deceptive degree the odour of oil of aniseed.



Boiling-point 237° ; fusing-point 26° ; solidifying-point 22° .

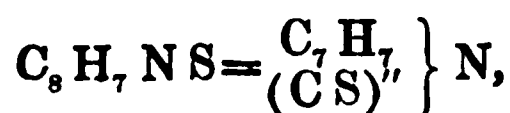
When gently heated with toluidine, tolylic mustard-oil is reconverted into ditolylsulphocarbamide. With ammonia it forms sulphuretted monotolyl-urea. Aniline gives rise to the mixed sulphuretted urea of the phenyl and tolyl-series, which is easily obtained in beautiful crystals.

Benzylic Mustard-oil.

Chemists are acquainted with a primary monamine isomeric with toluidine. This is benzylamine, discovered by M. Mendius. Since the beautiful experiments of MM. Fittig and Tollens have established the presence of the methyl-group in toluol, our views respecting the difference of constitution of the two isomeric monamines have acquired a solid foundation. In toluidine the substitution of the primary ammonia fragment ($H_2 N$) for hydrogen has taken place within the benzol nucleus; in benzylamine, on the other hand, the substitution occurs in the methyl-group engrafted upon the benzol nucleus. Benzylamine thus belongs, in a measure, to both the fatty and the aromatic series; and the residue of ammonia, which in fact is exclusively affected during the formation of mustard-oils, is present in the fatty portion of the compound. Under these circumstances it appeared rather probable that the base isomeric with toluidine would yield its mustard-oil by conversion into the bisulphide-of-carbon compound and distillation of the latter with perchloride of mercury. Experiment has verified this anticipation.

Benzylamine dissolves in bisulphide of carbon with evolution of heat, a beautiful white crystalline compound being formed, which, when distilled with alcohol and mercuric chloride, yields a liquid of a penetrating odour. On adding water to the alcoholic distillate, the mustard-oil separates in clear drops which are heavier than water.

Benzylic mustard-oil,



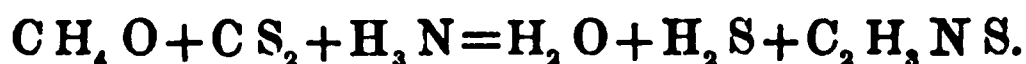
isomeric with tolylic mustard-oil, boils at about 243° , a few degrees higher than the tolyl-compound. The new body possesses in an eminent degree the odour of water-cresses. The resemblance is so striking, that it becomes desirable to examine the essential oil of water-cresses.

I may here mention that menaphtylamine, the preparation and pro-

perties of which I have lately described to the Royal Society *, yields likewise a mustard-oil, if it be successively treated with bisulphide of carbon and mercuric chloride. I have not, however, more minutely examined this compound.

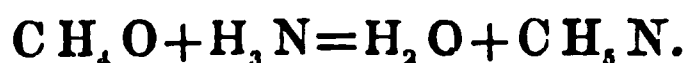
All the mustard-oils here mentioned exhibit, more especially with reference to ammonia and its derivatives, the same reactive power which belongs to ethylic mustard-oil, more minutely described in my former communication, and by which the mustard-oil *par excellence*, the well-known allyl-compound, has long fixed the interest of chemists. Of the legion of urea-like bodies which are here possible, I have prepared but few. It may be stated that the sulphuretted methylic and amylic ureas, and also the sulphuretted methylamylic and amylytolylic ureas vie with each other as to beauty and facility of crystallization. I have not, however, examined more minutely these ammonia compounds, their study promising but very little scientific gain. On the other hand, I have investigated with some care the metamorphoses of the mustard-oils, since their comparison with the corresponding transformations of the sulphocyanic ethers promised to elucidate the different construction of the two groups of bodies.

The results of these inquiries unequivocally confirm the view suggested by the formation of the two classes of compounds. It is indeed only necessary to trace their origin in order to understand the nature of this difference. We could not select better illustrations than the isomeric terms of the methyl-series. Both bodies, methylic mustard-oil and sulphocyanide of methyl, are in the last instance derived from exactly the same compounds, viz. methylic alcohol, bisulphide of carbon, and ammonia. Let the molecules of these three compounds unite with separation of 1 molecule of water and 1 molecule of sulphuretted hydrogen, and a body will be formed possessing the composition of both methylic mustard-oil and sulphocyanide of methyl.

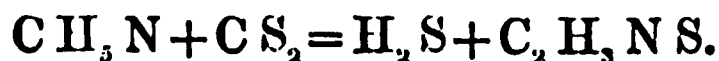


Accordingly the nature of the body produced must depend upon the conditions (it might be almost said upon the order) in which the molecules of water and sulphuretted hydrogen are separated from the aggregate of atoms.

Conceived in its simplest form, the first step of the generation of methylic mustard-oil consists in the action of ammonia upon methylic alcohol, when methylamine is produced with separation of water.



In a second phase of the process, methylamine is acted upon by bisulphide of carbon, the products being methylic mustard-oil and sulphuretted hydrogen.



* Proceedings, vol. xvi. p. 445.

The reactions occur in the inverse order when sulphocyanide of methyl is produced. Here the process commences with the reaction between bisulphide of carbon and ammonia, hydrosulphocyanic acid being formed with separation of sulphuretted hydrogen.

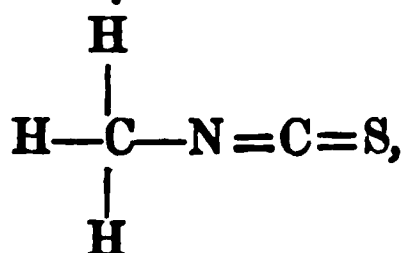


Hydrosulphocyanic acid and methylic alcohol, lastly, furnish water and sulphocyanide of methyl.

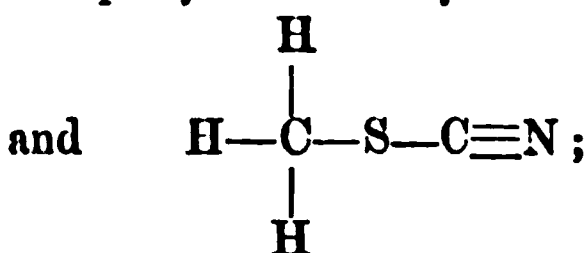


This inverse order, in which the reactions succeed each other, gives a definite direction to our speculations as to the arrangement of the atoms within the molecules of the two compounds. If bisulphide of carbon, SCS , in contact with methylamine, H_3CNH_2 , is found to disengage sulphuretted hydrogen, we cannot doubt that the carbon-atom of the bisulphide, meeting, as it were, with its two freed attraction-units those liberated in the nitrogen atom, associates with this nitrogen-atom, and consequently that it must be by the nitrogen that the carbon of the methyl-group is chained to the carbon of the bisulphide. On the other hand, if in hydrosulphocyanic acid we may conceive the hydrogen to be in union with the sulphur, we are, after this hydrogen has been converted into water by the hydroxyl of methylic alcohol, also justified in considering the sulphur-atom as the link of connexion between the two carbon-atoms of the compound, the attraction-unit, which has become available in the carbon-atom, being saturated by the free atomic power of the sulphur. The relative position of the atoms in the molecules of the two compounds would thus be indicated by the following diagram—

Methylic mustard-oil.



Sulphocyanide of methyl.



or more concisely in the subjoined formulæ—



If this conception be correct, it is obvious that whenever nitrogen and sulphur are found together in a molecule, this molecule must be capable of existing in two different forms, one corresponding to methylic mustard-oil, the other to cyanide of methyl.

In a paper which I hope shortly to submit to the Royal Society, I propose to show how far this conception is supported by experiment.

- V. "Account of Spectroscopic Observations of the Eclipse of the Sun, August 18, 1868, in a letter addressed to the President of the Royal Society. By Captain C. T. HAIG, R.E. Communicated by the President. Received September 21, 1868.

Poona, 24th August, 1868.

MY DEAR SIR,—I hasten to send you an account of the observations I have fortunately been able to make at Beejapoor of the total eclipse on the 18th instant with one of the hand-spectroscopes sent out by the Royal Society in the care of Lieut. Herschel, R.E., not waiting to let my report be forwarded by Colonel Walker, R.E., my departmental superior, on account of the delay which would necessarily be caused thereby.

I may state at once that I observed the spectra of two red flames close to each other, and in their spectra two broad bright bands quite sharply defined, one rose-madder and the other light golden. These spectra were soon lost in the spectrum of the moon's edge just before emergence, which had also two well-defined bright bands (one green and one indigo) about a quarter the width of the bands in the spectra of the flames, this spectrum being again soon lost in the bright sunlight.

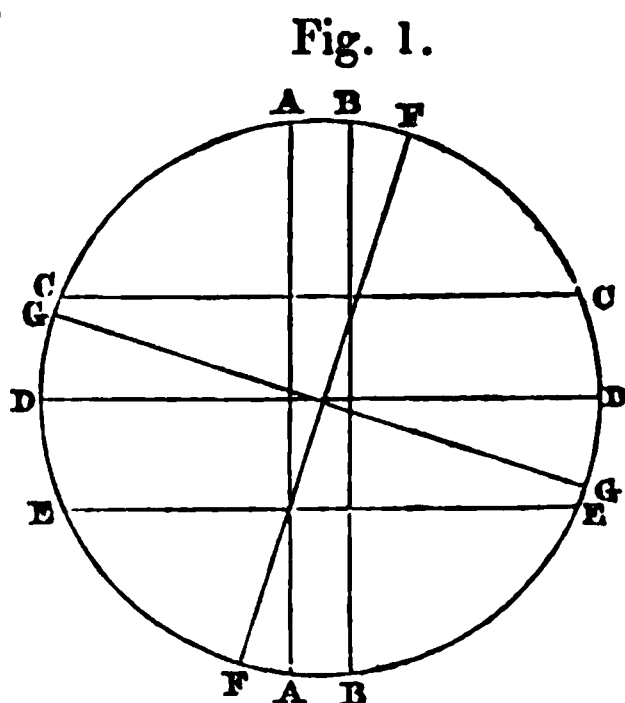
I will now proceed to give a somewhat detailed account of the observations, in which Captain Tanner, Bombay Staff Corps and of the Minar Survey (who, on my earnest solicitation, accompanied me), and Mr. Kero Laxuman, Professor of Mathematics in the Deccan College, took part, and during which Mr. Hunter, Bombay Civil Service, and Dr. Kielhorn, Professor of Sanskrit in the Deccan College, were present as non-professional observers.

Our instrumental equipment consisted as follows :—Mr. Kero Laxuman brought an ordinary pedestal-telescope of $2\frac{1}{4}$ -inch aperture and 36 inches focal length by Horne and Thornthwaite, which he temporarily mounted on a stand equatorially ; and he had a scale fitted inside a 60-power eyepiece, which, however, he was unfortunately not able to use, on account of a fall which his instrument sustained from being blown down by the high wind ; he therefore had to use another eyepiece of power 70, not furnished with a scale. He also had a pocket chronometer beating five times to two seconds, by Arnold and Dent.

Captain Tanner had an Everest theodolite by Troughton and Simms, having a remarkably good telescope, $1\frac{7}{8}$ -inch aperture and 18 inches focal length, and an eyepiece of power 46.

I had one of the Royal Society's small hand-spectroscopes, and a small 6-inch transit theodolite by Troughton and Simms, the cap of the object-glass of which I had cut so as to receive the prism-cap of the spectroscope, and had fitted one to the other, so that I could at once shift the prism-cap from its own telescope to that of the theodolite, and *vice versa*.

I had also a black frame about 2 feet long by 1 foot high with a slit in the centre, the width of which was regulated by turning a black excentric cylinder. This I had previously used in observing the lines in the solar and other common spectra, and I placed it 10 feet from the theodolite (the shortest distance the telescope could focus); and close at the back of it I placed a heliotrope held by a Survey Signaller, intending, if opportunity offered, to examine the lines in the spectrum of the corona. In the diaphragm of the theodolite-telescope I had a system of wires (shown by diagram below), which I had intended for assistance in general observations of the flames, in case I should find that I could make no satisfactory spectrum-observations, which, from the absence of any slit arrangement in the spectroscope, I was rather inclined to anticipate. The wires A A, B B were vertical, C C, D D, E E horizontal, F F the direction of the moon's path at the middle of the eclipse, and G G perpendicular to F F. This system gave so many fixed distances and points that I thought it would be useful both in estimating the position and the height of the flames. However, its utility was not put to the test; for the little time I had was given to the spectroscope. I also had an eight-day mean-time chronometer beating half-seconds, by Baker.



The sky in the early morning of the 18th was very cloudy, so that our hopes of success were very low; but as it afterwards brightened up for a while rather suddenly, we were somewhat encouraged to hope for a similar brightening during part of the eclipse. Soon, however, at about 7 o'clock, it darkened again, and remained so till after the total phase was over, occasional openings in the nimbi giving us glimpses of the sun through the cirrocumuli which were floating very high up. At 7 o'clock we had reached our station of observation, which was on a large solid tower called the Uparī Būrij, 67 feet high and about 60 feet diameter (on the top were two guns, one of which was 31 feet long)—one of the many ruins of the city, and a most favourable position from which to observe the phenomena of the eclipse and the general aspect of the surrounding country. On account of the prevailing very high wind, we planted our instruments on and near the top of the external stone staircase so as just to be protected by the tower from the wind. Mr. Kero Laxuman at first set up his telescope on the top of the tower; but it was blown down, as I have previously mentioned. This accident much interfered with the carrying out of our preconcerted plan of observation, which was as follows.

Mr. Kero Laxuman and Captain Tanner were to take the times of first and last contact, the latter by observing the actual occurrences, the former

by measuring several lengths of the common chord soon after first and before last contact, with the aid of the scale in his 60-power eyepiece and noting the times. Captain Tanner (an expert delineator) was, during totality, to take command of Mr. Kero Laxuman's telescope, measuring the heights of the flames at times which would be recorded by Mr. Kero Laxuman, whose whole attention during totality was to be given to recording the times of occurrence of any phenomena that he, or either of us, might observe. Captain Tanner was also to make rapid sketches of all he saw, and I was to confine myself to spectrum-observations.

Unfortunately, contact was not observed until about fifty seconds after the commencement, when Captain Tanner at once made a sketch of the obscuration, Mr. Kero Laxuman recording the time. The sketch made the common chord equal to 3' at 7^h 51^m 17^s local time, giving 7^h 50^m 17^s as the time of first contact. Captain Tanner afterwards tested that sketch by noting the time before last contact, when the chord appeared of a similar length, which gave an interval of 45^s; so that, taking the mean between the original estimate and its verification, we have 7^h 50^m 24^s.5 as the time of first contact.

While the obscuration was increasing, Captain Tanner, during the few peeps we got at the eclipse, made drawings of the sun's spots, and sketched the mountains on the moon's edge, of which there were two plainly visible even with my small theodolite. The darkness increased very slowly till just before totality, when the increase was very rapid and sudden, and a general spontaneous exclamation "Oh!" from all of us gave Mr. Kero Laxuman the time of beginning of totality, which he recorded as 9^h 1^m 49^s. The eclipse was at that time completely shut out from our view by the clouds—nimbi low down being carried past by the high wind; we therefore felt at leisure to make our remarks on the degree of the darkness, which we were surprised to find so far from total. We could easily write, read our writing, and read the seconds of our watches without the aid of artificial light. We were all lamenting our misfortune in not being able to observe the eclipse, and had given up all hope of witnessing the phenomena we had come so far to see, and Captain Tanner had just noticed the faint reappearance of light in the west, when, contrary to all expectation, and to our intense satisfaction, a sudden opening in the nimbi showed us the eclipse through the cirrocumuli. We were each at our telescopes in an instant. I immediately saw through the naked telescope of the small theodolite that red flames were visible, and at once pointed the spectroscope, using the theodolite-telescope as a rest. Very fortunately I directed the spectroscope with its "refracting edge" tangent to the moon where two red flames were protruding, separated from each other by a small interval; so that their spectra, which were identical, were extended over the dark background of the moon's disk, and stood out in most marked and brilliant contrast with the feeble but continuous spectrum of the corona; and in

their spectrum there were the two broad bright bands I have above described. Most fortunately also these red flames were on that part of the sun which first reappeared ; so that just before or just *at* emergence there appeared at the very part I was intently observing one brilliant wide spectrum with the green and indigo bands before described, remaining visible for an interval just long enough to enable me to make quite sure of the position of the bands, which were then obliterated by the bright light of the sun. Of course, observing with the spectroscope alone it would have been impossible to say whether the spectrum with the green and indigo bands appeared just before or just after emergence ; but I think it must have been just before, because Captain Tanner called out when totality was over ; and I immediately remarked that I thought he was rather late, but he was quite confident about the accuracy of his observation. What struck me as being very remarkable was the circumstance, that though the light of the red flames was to the naked eye so feeble as to be outshone to extinction by that of the corona, nevertheless, when viewed with the spectroscope, the spectrum of the corona was very weak, and that of the flames remarkably brilliant. On the first glimpse of the eclipse, before looking through the telescope, the corona appeared so bright, that it gave me the momentary impression (as it did to Captain Tanner) of its being an annular eclipse. We are divided in our estimate of the length of the interval during which we observed the totality. It appeared to me very short—so much so, that when it was over I was quite taken by surprise to hear that both Captain Tanner and Mr. Kero Laxuman had taken sketches of the flames ; and their sketches, both as to position and structure, were, with one slight exception, remarkably coincident. From the time of my first pointing the spectroscope to the bursting out of the sun's light I never once withdrew my eye, though it had been my intention to shift the prism-cap on to the telescope of the theodolite as soon as I should have carefully noted the spectrum of the flames ; but while I was intently gazing on the two bright bands to impress their colour well on my memory, the new spectrum of the moon's edge appeared, so that I was under the impression that the length of the time of observation was very short. On the other hand, Captain Tanner, judging from the amount of work he did in the time, estimated it at a minute. Mr. Kero Laxuman estimated it at 40 or 45 seconds. Immediately after the totality was over we all three made rough notes of our observations ; and Captain Tanner's and Mr. Kero Laxuman's notes agree together wonderfully in their description of the structure of the flames.

The accompanying rough sketch was made by Captain Tanner, who had not the means of making a more finished drawing. The sketch shows the *actual* appearance of the eclipse. It was observed by Captain Tanner wholly inverted, and by Mr. Kero Laxuman (who used a diagonal eyepiece) inverted vertically but not laterally. Captain Tanner and Mr. Kero Laxu-

man only differed in their position of the small flame *c*, the former placing it to the right, the latter at a similar distance on the left of the flames *b*; but Captain Tanner at once yielded his conviction to that of Mr. Kero Laxuman, which, therefore, we accepted as most likely to be true. The spectrum of *c* was not observed by me at all. I therefore think it could only have appeared simultaneously with the bright spectrum of the moon's edge. I so held the spectroscope that I could not see the spectrum of the flame *a*.

The following is an extract from Captain Tanner's notes, taken almost immediately after the eclipse:—"I at first saw three prominences—one long curved pointed tongue, and two close together, straight but flat-topped, about two-thirds the height of the former. They were of a rose-madder colour, and were decidedly more like flames than anything else, not only in their general appearance and colour, but by their being composed of smaller tongues of flame parallel (or nearly so) to the general axis of the flame, so that they had a streaky appearance and a ragged edge. At the first glance, when the sun was somewhat obscured by clouds, I thought they were homogeneous and had hard edges; but this idea was at once dispelled when the clouds cleared off. The two protuberances, which were close together, were not, as far as I could see, joined by any smaller shots of flame. I afterwards observed one small protuberance, and marked the position of it in my sketch. I did not observe that it was streaky, as the others were—perhaps on account of its being so small, and perhaps because I had not sufficient time to examine it properly. As regards the corona, when we first began to see the eclipse through the clouds, I was under the impression that the eclipse, instead of being total, was only annular, so bright was the corona near the moon's limb. I could not detect any irregularities in the structure of the corona, but the light appeared to be gradually shaded off all round."

Captain Tanner also says, "The most careless observer would notice the streaks of which the flames *b* were composed; but it required more careful inspection to determine the streaky nature of the flame *a*."

The following is from Mr. Kero Laxuman's notes:—"The protuberance *a* appeared like a red flaming torch, width $\frac{1}{2}$ a minute, height about 2 minutes, colour dark red, lines stretched over a less-red ground. The direction not perpendicular to the edge of the moon, but making an angle of 60° with it. Those marked *b* were broader and almost as high as *a*, but not pointed. They appeared to expand a little at the vertex. They were also streaked by several dark-red lines. That marked *c* appeared semicircular, with a breadth of about $\frac{1}{2}$ a minute. The flame *a* was visible for about 2 minutes after the end of totality; and had there been no clouds, I think it could have been seen longer."

Both Captain Tanner and Mr. Kero Laxuman also agreed in describing the form of the red flames *b* as somewhat similar to hands with fingers slightly separated.

There is a curious coincidence which I may here mention, though I imagine it can only be regarded as purely fortuitous, viz. that the flames were almost exactly opposite the spots on the sun's disk.

On the afternoon of the 18th, Captain Tanner and I went to Moolwar, eighteen miles south of Beejapoor, where the German astronomers had put up their instruments. We there learnt that they had only seen the eclipse for less than 5 seconds during totality, and that through an upper stratum of clouds which rendered photometric observations impracticable; but we were surprised to hear that neither a spectroscope nor a polariscope was attached to either of their equatorial telescopes at the time of visibility, but that both the observers with these instruments were intent on measuring the heights of the flames. They determined the normal height of flame *a* to be 3 minutes; but as they must have seen it at an earlier phase than Captain Tanner and Mr. Kero Laxuman, it would appear slightly longer to them than to us.

It is very curious how the darkness during totality seems to have differed in degree in different places. At Beejapoor we were told that down below in the town the darkness was so great that it was not possible to see one's own hand. We thought this account might be an exaggeration; but we afterwards learnt that at Moolwar a gentleman dropped part of an eye-piece of a telescope, and that it was not possible to find it even by placing the eye close to the ground, until after the end of totality.

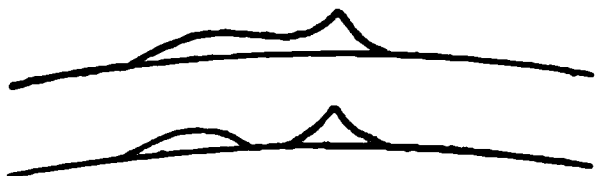
We have not had time during our continual travelling to compute the elements of the eclipse for Beejapoor for ourselves; and it might have been waste of time to have done so before we started on our journey, for we were uncertain of our being able to get so far south as Beejapoor; but I give below a statement of elements for Beejapoor as computed by Mr. Pogson, astronomer at Madras, and published in the 'Times of India,' and with it the times as observed or estimated by us.

	Mr. Pogson's elements.	Our elements.	Remarks.
	h m s	h m s	
First contact	7 50 54	7 50 25	Mean of two estimates.
Beginning of totality	9 2 9	9 1 49	Approximate.
End of totality	9 7 21	9 6 59	Actually observed.
Last contact	10 28 44	10 28 14	" "
Angle from } first contact... vertex of... } last contact...	1° right. 173° right.	At vertex. } 165° right. }	Approximate.

There was a difference in our times of last contact. Mr. Kero Laxuman made it at 10^h 28^m 9^s; I made it 10^h 28^m 14^s, and Captain Tanner 10^h 28^m 17^s. I was observing with the little theodolite, and distinctly saw the moon's

limb after Mr. Kero Laxuman had called out ; so I attributed his error to the vibration of his telescope caused by the wind. Capt. Tanner observed, I believe, *the* last contact ; but, strange to say, the point of the moon which made last contact was a mountain-peak of this

shape ; Capt. Tanner would make it thus,



dividing the mountain into two hills ; and he says I was a second too soon in my observation, which was of the spherical last contact ; and perhaps he was right, as he had a better telescope than I had. His observation at $10^h 28^m 17^s$ was the time of the *peak* leaving the sun's limb ; so that he and I differ only by 1 second, as to whether the spherical last contact occurred at $10^h 28^m 14^s$ or $10^h 28^m 15^s$.

I sent a native assistant to Moolwar (the station selected by the German astronomers) to take observations with a barometer, and with wet-, dry-, and black-bulb thermometers, continuously for some days before and after the eclipse, but I anticipate no interesting results (from the rough glance I took at the records on the evening of the 18th). The atmosphere was during the time in a very disturbed state.

Mr. Chambers, of the Bombay Observatory, went to a village called Mongoli, about six miles east of Moolwar, with the intention of observing the eclipse ; but he was disappointed, for it was completely obscured by clouds during the whole of the total phase.

I have not yet heard what success has attended Lieuts. Herschel and Campbell with the spectroscope and polariscope at Jamkhandi, so that I am quite ignorant of the value of our observations ; but I trust that even should other observers have succeeded in contributing to physics more definite information, ours may at least be valuable as corroborative evidence.

I am, dear Sir,

Yours faithfully,

C. T. HAIG,

Captain Royal Engineers.

General Sabine, R.A., P.R.S.

EXPLANATION OF THE PLATE.

Fig. 1 represents the total eclipse as it appeared during the last 20 seconds of the total phase.

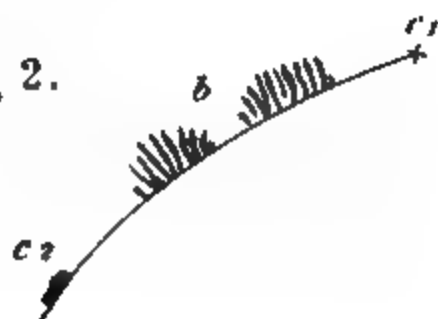
Fig. 2. Red prominences, drawn to larger scale, and showing the streaked structure of *a* and the radiating thicker lines composing the double prominence at *b*. (*Note.* A light-red colour showed itself between these streaks, which gave the prominences a greater appearance of solidity.) *c*₂, small red prominences as noted by Kero Laxuman ; *c*₁, the same as noted by me (*c* appeared just at the end of totality). The height of *a* was a little over 2', *b* about 1' 40'' ; *c* may have been 0' 20''.

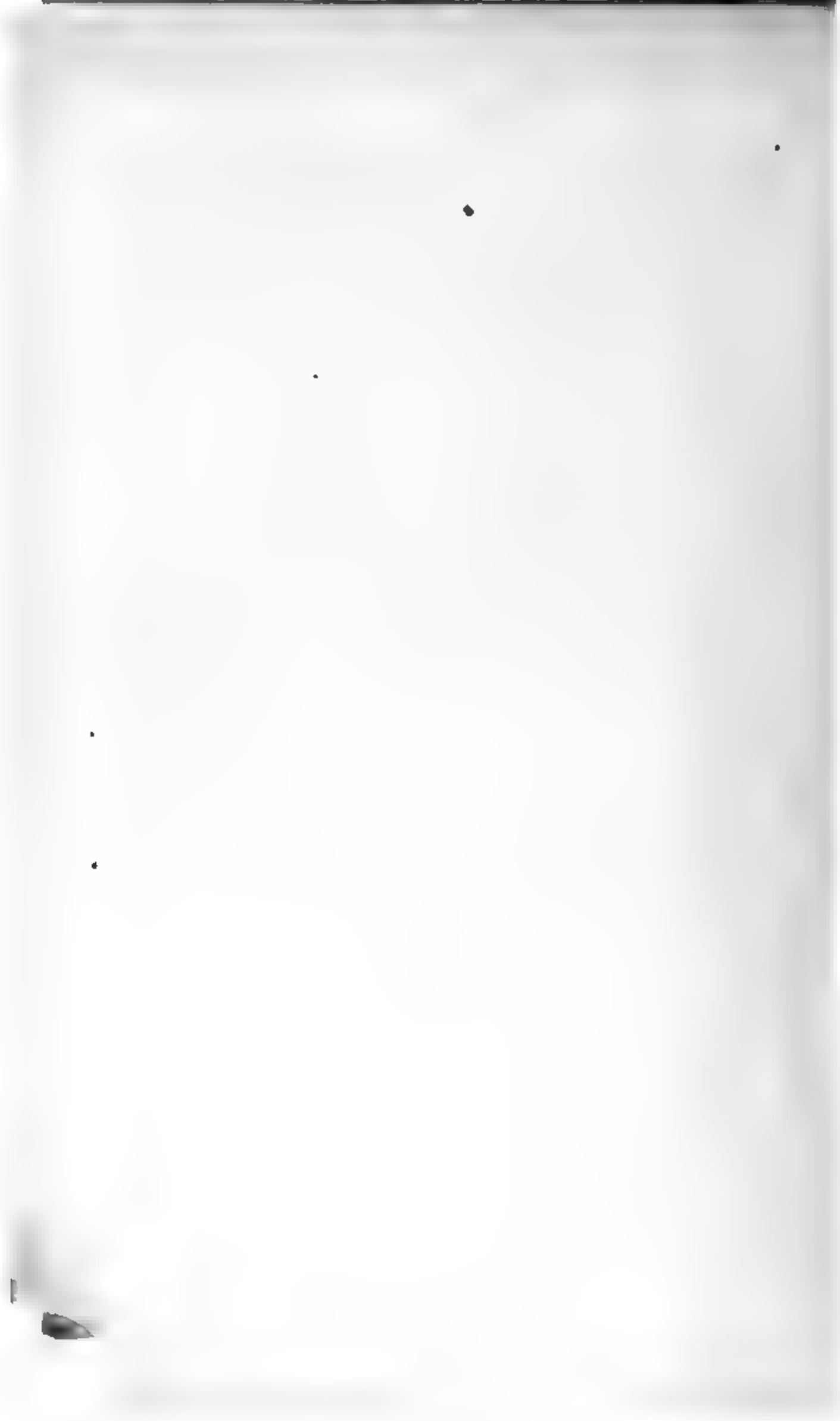


Fig 1.



Fig 2.





VI. "Account of Observations of the Total Eclipse of the Sun, made August 18th, 1868, along the coast of Borneo, in a Letter addressed to H.M. Secretary of State for Foreign Affairs by His Excellency J. POPE HENNESSY, Governor of Labuan." Communicated by the Right Hon. Lord STANLEY, F.R.S. Received October 8, 1868.

Government House, Labuan,
19th August, 1868.

MY LORD,—Seeing the interest which Her Majesty's Government and the scientific public in England have shown in the remarkable eclipse which occurred yesterday, I took steps to make such observations as I could along the coast of Borneo, and I have now the honour of laying them before your Lordship.

After passing from the Gulf of Siam across the China Sea, the line of total eclipse passed across the Island of Borneo, touching the colony of Labuan on the east, and stretching not far from the River Bintulu on the west.

Having ascertained that the precise centre of this band of total eclipse would be found at Barram Point (a place within my jurisdiction as Consul-General in the Island of Borneo), I made arrangements with Capt. Reed, of H.M.S. 'Rifleman,' to take my observations at that spot. As that well-known officer has been for years in charge of the important survey of the China seas, his ship afforded special facilities for such an expedition.

We left Labuan on Monday at noon, and arrived off Barram Point at five o'clock next morning, Tuesday the 18th.

A tent was fitted up on an open space between a Casuarina-plantation and the sea, and the following corps of observers landed at ten o'clock :—Captain Reed, Lieutenant Ray, Lieutenant Ellis, and myself, our four telescopes being securely adjusted on large tripod stands manufactured for the occasion. Four other officers landed with us :—Dr. O'Connor to note the physiological phenomena, Mr. Wright to watch the magnetic needle, and Mr. Doyley and Mr. Roughton to mark the time. A few intelligent sailors were in attendance to assist the observers, if necessary.

Mr. Petley and the other officers left on board the 'Rifleman' had charge of the barometrical and thermometrical observations, and they were also directed by Captain Reed to watch the vibrations, if any, in the magnetic compasses.

Before leaving the ship I made some observations upon the solar spots. At 8 A.M. I found some spots in a line from east to south *. The upper

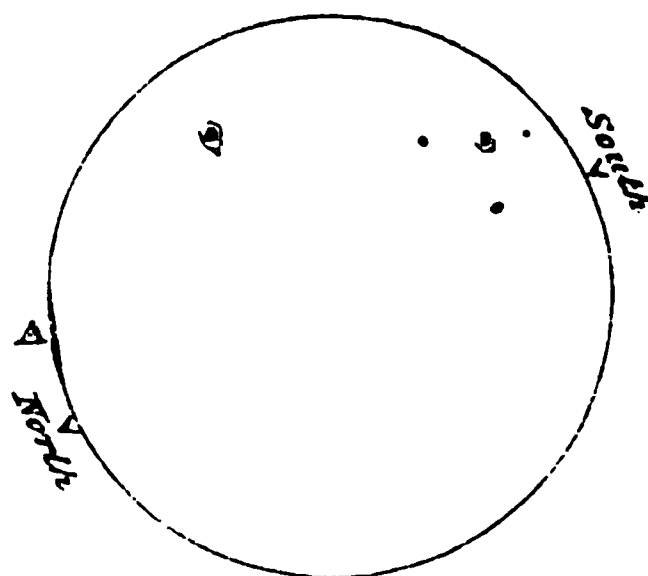
* It did not seem necessary to reproduce the sketch sent by the author, as the position of the spots may be represented by conceiving the next figure to be turned round through about 65° in the direction of the hands of a watch.

spot was surrounded by a penumbra stretching towards the centre of the sun, the second spot was small and sharply defined, the third spot, or group of spots, had a penumbra, and the fourth spot was surrounded by a space of very brilliant light.

These spots I subsequently refer to as Nos. 1, 2, 3, and 4.

Moving with the rotation of the sun, the line of spots gradually became horizontal, and at 12 o'clock (noon) they lay thus (see fig. 1).

Fig. 1.



At 15 seconds past 12 o'clock the first contact of the moon took place at the point marked A. At 12^h 24^m 20^s.2 the contact was observed of the moon with the penumbra of spot No. 1.

At 12^h 25^m 25^s.5 contact with left of No. 1 spot.

At 12^h 26^m 58^s the spot was completely covered.

During all this time no vibrations or change of any kind was noticed in the magnetic needle.

At 12^h 44^m 15^s the moon's limb had advanced to the sun's centre.

At 1^h 10^m 47^s.5 spot No. 2 was passed, and at 1^h 18^m 6^s spot No. 3 was passed.

All this time not the smallest magnetic change could be noticed.

At 1^h 27^m 22^s the total eclipse took place, and lasted for 6 minutes and 13 seconds; the first appearance of the sun's limb from behind the moon being at 1^h 33^m 35^s.

The spots reappeared as follows:—

No. 1	at	1 ^h	41 ^m	12 ^s
2	,,	2	33	14
3	,,	2	39	48
4	,,	2	48	7;

and at 2^h 52^m 39^s.8 the last contact of the edge of the sun and moon was noticed.

During the 6^m and 13^s of total eclipse not the slightest change of any kind could be observed in the magnetic needle, nor did it move or vibrate in any way on the appearance of the solar spots.

To complete the negative results of our magnetic observations, I have only to add that the officers who had been directed to watch the ship's compasses report that they could not detect the slightest movement of any kind.

I now proceed to describe the general phenomena of the eclipse; and in doing so I confine myself to copying from the rough notes I took at Barram Point, and from the note-books of Capt. Reed and his officers, also taken on the spot. As the mail closes for Singapore to-morrow morning, I have not time to arrange the materials before me in anything like scientific order; and the absence of any works of reference (we have not even this year's supplement to the Nautical Almanac) renders me still less able to do justice to the facts we collected.

We were very fortunate in the weather. The day was bright and clear; not a cloud was near the sun. A few round white clouds that lay on the horizon hardly moved. There was a slight breeze from W.S.W.

The sea was breaking heavily on the shore, and it had a slight brownish bluish tinge all over, except where the white breakers approached the land.

The grove of Casuarina trees behind us had the same deep-green colour which they always exhibit on a fine day in the tropics.

A few swallows were skimming about high in the air. We also noticed some dragonflies, butterflies, and a good many specimens of a large heavy fly like a drone-bee.

When we left the ship at 10 o'clock the barometer was 30·00; the mean of the two thermometers in the deck chart-room (in the shade) was 85°; the dry thermometer exposed to the sun was 91°, and the wet thermometer exposed to the sun was 83°·5.

During the progress of the eclipse the barometer fell steadily. At 12^h 0^m 15^s, first contact, it was 29·98.

		At 12 ^h 26 ^m 0 ^s it fell to 29·96 in.
	„ 12 44 15	it was 29·96
	„ 1 0 0	„ 29·94
	„ 1 15 0	„ 29·93
Total {	„ 1 27 22	„ 29·92
eclipse. {	„ 1 33 35	„ 29·92
	„ 2 0 0	„ 29·91
	„ 2 30 0	„ 29·91
	„ 2 39 48	„ 29·91
	„ 2 52 39·8	„ 29·91

The mean of the two thermometers in the shade was 85°, without any change whatever from 10 o'clock till the close of the eclipse. At the close of the eclipse, 2^h 52^m 39^s·8, it rose to 86°.

The dry-bulb thermometer, hung in the sunlight, stood at 91° at the first contact.

		At 12 ^h 44 ^m 15 ^s it fell to 90°		
	„	1	0	0 „ 88
	„	1	15	0 „ 87
Total eclipse.	{ „	1	27	22 „ 85
	{ „	1	33	35 „ 85
	„	2	0	0 it rose to 91
	„	2	30	0 „ 96
	„	2	52	39·8 „ 96

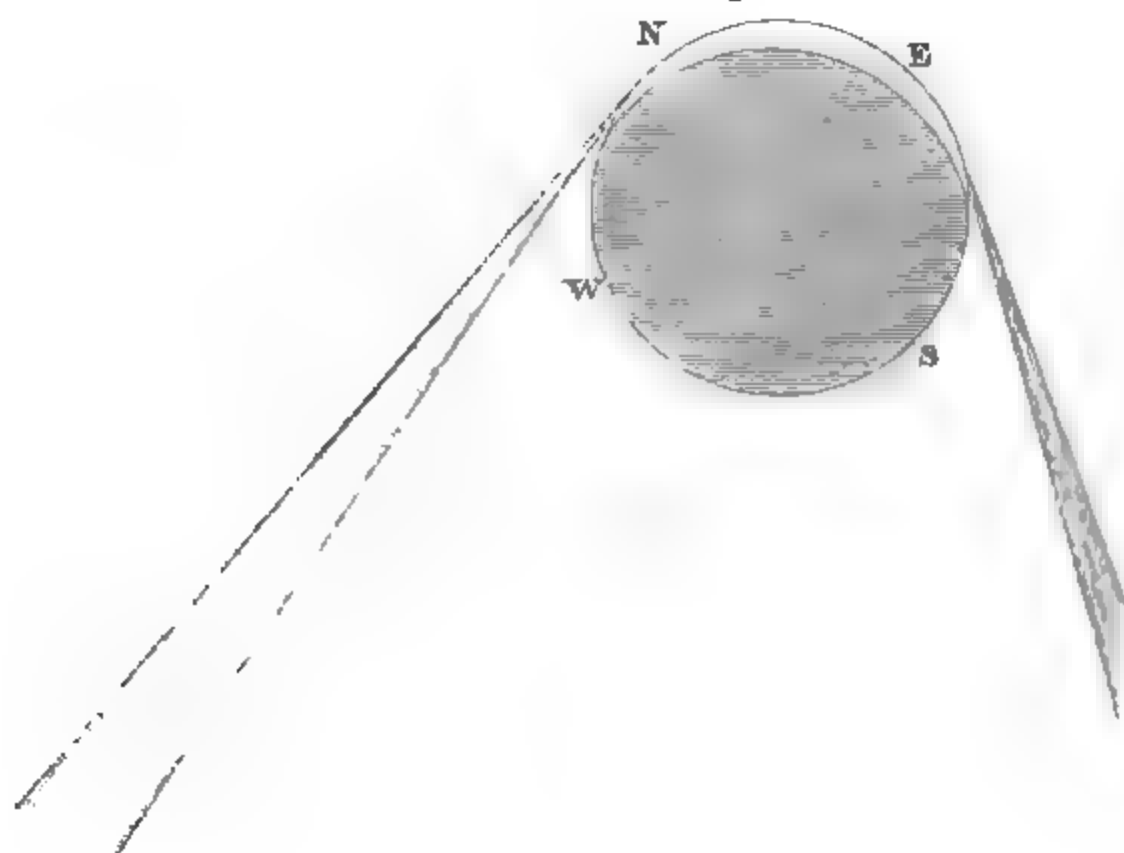
It thus appears that the thermometer fell from 91° to 85° as the moon was covering the sun, and that it rose from 85° to 96° as the sun was re-appearing.

The wet-bulb thermometer fell from 80°·5 to 83° at the total eclipse, and rose to 89° at the termination of the whole eclipse.

Ten minutes before the total eclipse there seemed to be a luminous crescent reflected upon the dark body of the moon*.

In another minute a long beam of light, pale and quite straight, the rays diverging at a small angle, shot out from the westerly corner of the sun's crescent. At the same time Mr. Ellis noticed a corresponding dark band, or shadow, shooting down from the east corner of the crescent (fig. 2).

Fig. 2.



At this time the sea assumed a darker aspect, and a well-defined green band was seen distinctly around the horizon. The temperature had fallen

* See note at the end.

and the wind had slightly freshened. The darkness then came on with great rapidity. The sensation was as if a thunderstorm was just about to break, and one was startled on looking up to see not a single cloud overhead. The birds, after flying very low, disappeared altogether. The dragonflies and butterflies disappeared, and the large drone-like flies all collected on the ceiling of the tent and remained at rest. The crickets and Cicadæ in the jungle began to sound; and some birds, not visible, also began to twitter in the jungle.

The sea grew darker, and immediately before the total obscuration the horizon could not be seen.

The line of round white clouds that lay near the horizon changed their colour and aspect with great rapidity. As the obscuration took place they all became of a dark purple, heavy looking, and with sharply defined edges; they then presented the appearance of clouds close to the horizon after sunset. It seemed as if the sun had set at the four points of the horizon.

The sky was of a dark leaden blue, and the trees looked almost black. The faces of the observers looked dark, but not pallid or unnatural.

The moment of maximum darkness seemed to be immediately before the total obscuration; for a few seconds nothing could be seen except objects quite close to the observers.

Suddenly there burst forth a luminous ring around the moon. This ring was composed of a multitude of rays, quite irregular in length and in direction; from the upper and lower parts they extended in bands to a distance more than twice the diameter of the sun. Other bands appeared to fall towards one side; but in this there was no regularity, for bands near them fell away apparently towards the other side. When I called attention to this, Lieut. Ray said, "Yes, I see them; they are like horses' tails;" and they certainly resembled masses of luminous hair in complete disorder.

I have said these bands appeared to fall to one side; but I do not mean that they actually fell or moved in any way during the observation. If the atmosphere had not been perfectly clear, it is possible that the appearance they presented would lead to the supposition that they moved; but no optical delusion of the kind was possible under the circumstances.

During the second when the sun was disappearing, the edge of the luminous crescent became broken up into numerous points of light. The moment these were gone, the rays I have just mentioned shot forth, and at the same time we noticed the sudden appearance of the rose-coloured protuberances.

The first of these was about one-sixth of the sun's diameter in length, and about one-twenty-fourth part of the sun's diameter in breadth. It all appeared at the same instant, as if a veil had suddenly melted away from before it.

It seemed to be a tower of rose-coloured clouds. The colour was most beautiful—more beautiful than any rose-colour I ever saw; indeed I know of no natural object or colour to which it can be, with justice, compared. Though one has to describe it as rose-coloured, yet in truth it was very different from any colour or tint I ever saw before.

This protuberance extended from the right of the upper limb, and was visible for six minutes.

In five seconds after this was visible, a much broader and shorter protuberance appeared at the left side of the upper limb. This seemed to be composed of two united together. In colour and aspect it exactly resembled the long one.

This second protuberance gradually sank down as the sun continued to fall behind the moon, and in three minutes it had disappeared altogether.

A few seconds after it had sunk down there appeared at the lower corresponding limb (the right inferior corner) a similar protuberance, which grew out as the eclipse proceeded. This also seemed to be a double protuberance, and in size and shape very much resembled the second one; that is, its breadth very much exceeded its height.

In colour, however, this differed from either of the former ones. Its left edge was a bright blue, like a brilliant sapphire with light thrown upon it; next to that was the so-called rose-colour, and, at the right corner, a sparkling ruby tint.

This beautiful protuberance advanced at the same rate that the sun had moved all along, when suddenly it seemed to spread towards the left, until it ran around one-fourth of the circle, making a long ridge of the rose-coloured masses. As this happened, the blue shade disappeared.

In about twelve seconds the whole of this ridge vanished, and gave place to a rough edge of brilliant white light, and in another second the sun had burst forth again.

In the meantime the long, rose-coloured protuberance on the upper right limb had remained visible; and though it seemed to be sinking into the moon, it did not disappear altogether until the lower ridge had been formed and had been visible for two seconds.

This long protuberance was quite visible to the naked eye, but its colour could not be detected except through the telescope. To the naked eye it simply appeared as a little tower of white light standing on the dark edge of the moon.

The lower protuberance appeared to the naked eye to be a notch of light in the dark edge of the moon—not a protuberance, but an indentation.

In shape the long protuberance resembled a goat's horn.

As I have not time to attempt an elaborate drawing of these objects, I content myself with inclosing to your Lordship two pages from my rough note-book, showing the sketches taken at the moment.

Though the darkness was by no means so great as I had expected, I was

unable to mark the protuberances in my note-book without the aid of a lantern, which the sailors lit when the eclipse became total.

Fig. 3.

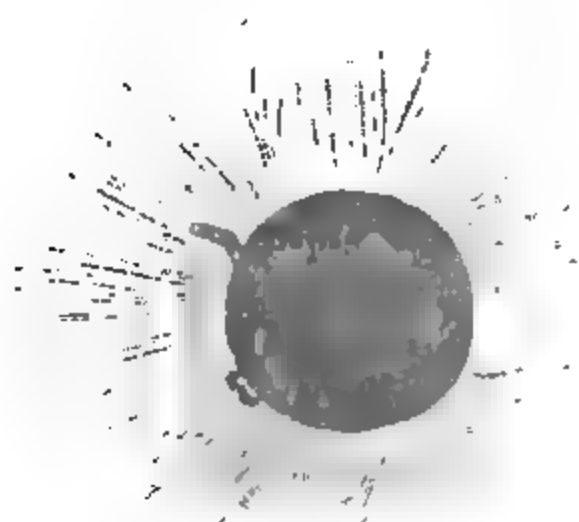


Fig. 4.



Those who were looking out for stars counted nine visible to the naked eye.

One planet (Venus?) was very brilliant. Its altitude at $1^h 31^m 0^s$ was $30^\circ 32'$ (Carey's Government Sextant, no error), and its distance from the nearest limb of the moon was $37^\circ 28'$. The altitude of the lower limb of the moon at $1^h 32^m 0^s$ was $66^\circ 30'$.

On board the 'Rifleman' the fowls and pigeons went to roost, but the cattle showed no signs of uneasiness; they were lying down at the time.

Whatever interest the foregoing observations may have for men of science, I am happy to be able to report that Capt. Reed has added to his public services by seizing this opportunity for determining the exact longitude of Barram Point.

Navigators have long been anxious to fix the precise longitude of some point along the coast of Borneo, and the event of yesterday has probably accomplished this. When Capt. Reed's calculations have been finally reduced, he will, no doubt, communicate them to the head of his department; and in the meantime he has kindly undertaken to place in a cover, directed to your Lordship, the true time as worked out from the observations, so that the times given in this despatch may be corrected before the despatch is used.

The time given in this despatch was taken from one of Parkinson and Prodsham's Government pocket chronometers, No. 1887.

As I believe we were the nearest group of observers to the Equator, and as the other conditions were unusually favourable for our work, I venture to hope that even the inadequate and very unscientific account I have

given may prove to be of some interest to your Lordship and to the men of science in England.

Before closing my despatch I received from Capt. Reed the error of the chronometer-watch used for taking the time. It was *fast* on the mean time at Barram Point $0^h 4^m 8^s.7$.

I have the honour to be, My Lord,

Your Lordship's most obedient humble Servant,

(Signed)

J. POPE HENNESSY,

Governor of Labuan, and Consul-General
in the Island of Borneo.

*To the Right Honourable Lord Stanley, M.P.,
Secretary of State for Foreign Affairs.*

[*Note.*—The phenomenon of the sun's crescent reflected on to the disk of the moon would seem to have been something accidental, perhaps (if seen by the writer only) a mere ghost, depending on a double reflection between the glasses of his instrument. The figure represents the "reflected" image as in the same position as the crescent itself, not reversed, indicating either a refraction or a double reflection.

The slender beams of light or shade shooting out from the horns of the crescent would seem to admit of easy explanation, supposing them to have been of the nature of sunbeams, depending upon the illumination of the atmosphere of the earth by the sun's rays. The perfect shadow, or *umbra*, would be a cone circumscribing both sun and moon, and having its vertex far below the observer's horizon. Within this cone there would be no illumination of the atmosphere, but outside it a portion of the sun's rays would be scattered in their progress through the air, giving rise to a faint illumination. When the total phase drew near, the nearer surface of the shadow would be at no great distance from the observer; the further surface would be remote. Attend in the first instance to some one plane passing through the eye and cutting the shadow transversely, and in this plane draw a straight line through the eye, touching the section of the cone which bounds the shadow; and then imagine other lines drawn through the eye a little inside and outside this. In the former case the greater part of the line, while it lay within the lower regions of the atmosphere, would be in shadow, the only part in sunshine being that reaching from the eye to the nearer surface of the shadow; but in the latter case the line would be in sunshine all along. In the direction of the former line, therefore, there would be but little illumination arising from scattered light, while in the direction of the latter the illumination would, comparatively speaking, be considerable. In crossing the tangent there would be a rapid change of illumination. Now pass on to three dimensions. Instead of a tangent line we shall have a tangent plane, and there will

of course be two such planes, touching the two sides of the cone respectively. Each of these will be projected on the visual sphere into a great circle, a common tangent to the two small circles, which are the projections of the sun and moon. In crossing either of these there will be a rapid change of illumination (feeble though it be at best) which will be noticed. According as the observer mentally regards darkness as the rule and illumination as the feature, or illumination as the rule and darkness as the feature, he will describe what he sees as a *beam* or a *shadow*. The direction of these beams or shadows given by theory, as just explained, agrees very well with the drawing sent by Governor Hennessy, which does not represent the left-hand beam so distinctly divided as it appears in the woodcut.

The times mentioned in the above despatch have *not* been corrected for the error of the chronometer-watch. In the following Tables, furnished by Staff Commander Reed, the *corrected* mean times alone are printed. The observations of time by Capt. Reed, Mr. Ray, and Mr. Doorly were made by Government pocket chronometer No. 1887, which was *fast* on mean time of place 0^h 4^m 8^s·7; those by Mr. Ellis by a gold pocket watch, compensation balance, which was *fast* on mean time of place 0^h 17^m 2^s·7; those belonging to the meteorological observations with a pocket watch, which was *fast* on mean time of place 0^h 2^m 47^s·9.

G. G. STOKES.]

Meteorological Observations taken on board H.M.S. 'Rifleman,' at Barram Point, during the Total Eclipse of the Sun, August 18th, 1868.

Mean time at place.				Marine barometer in Chart-house.	Mean of two thermometers in Chart-house.	Dry thermometer hung in the main rigging exposed to the sun.	Wet thermometer hung in the main rigging exposed to the sun.
A.M.	h	m	s	in.	°		
	7	57	12·1	29·98	81	°	°
	8	57	12·1	30·01	81	92	93·5
	9	57	12·1	30·00	85	86·5	83·5
	10	57	12·1	29·99	84	91	87
	11	57	12·1	29·98	85	88	85
P.M.							
	0	23	12·1	29·96	85	91	88
	0	42	12·1	29·96	85	90	87
	0	57	12·1	29·94	85	88	86
	1	12	12·1	29·93	85	87	85
	1	23	9·1	29·92	85	85	83
	1	32	12·1	29·92	85	85	83
	1	57	12·1	29·91	85	91	85
	2	27	12·1	29·91	85	96	90
	2	48	15·1	29·91	86	96	89

Jno. W. REED,
Staff Commander in charge of China Sea Survey.

Observations of the Total Eclipse of the Sun, August 18th, 1868.
H.M.S. 'Rifleman,' Barram Point.

Phenomena observed.	Mean Time at Place.			
	Captain Reed.	Mr. Ray.	Mr. Ellis.	Mr. Doorly.
First contact of moon with sun's limb*	h m s	h m s	h m s 11 56 07.1	h m s
Contact of moon's limb with pen- umbra of No. 1 spot	12 20 10.8	12 20 11.5		
Contact of moon's limb with left of No. 1 spot	12 21 16.3	12 21 16.8	12 21 19.8	
Contact of moon's limb with right of No. 1 spot	12 22 49.3	12 22 49.3	12 22 49.3	
Contact of moon's limb with sun's centre.....	12 40 06.3		
Contact of moon's limb with centre of No. 2 spot.....	1 06 38.8	1 06 38.8	1 06 43.3	
Contact of moon's limb with No. 3 spot (double)	1 13 58.3	1 13 57.3	1 13 57.3	
Contact of moon's limb with No. 4 spot	1 20 09.3			
Sun totally obscured	1 23 13.3	1 23 13.3	1 23 14.3	
Rose-coloured mass left side disap- peared	1 25 38.3			
Altitude of planet Venus 30° 32' (Carey's Govt. sextant, no error)...	1 26 51.3		
Planet Venus distant from nearest limb of moon 37° 28'	1 27 01.3		
Rose-coloured protuberance appeared below	1 27 51.3	1 27 19.3	
Altitude of lower limb of moon 66° 30'	1 27 51.3		
First appearance of sun's limb†.....	1 29 25.3	1 29 25.3	1 29 26.3	
No. 1 spot reappeared (centre)	1 37 03.3		
Venus disappeared from view.....	2 07 36.3			
No. 2 spot reappeared (centre)	2 29 05.3	2 29 05.3	2 29 05.3
No. 3 spot (double) reappeared (centre)	2 35 39.3	2 35 39.3	
No. 4 spot (double) reappeared (centre)	2 43 58.3		
Last contact of limbs ‡.....	2 48 31.1	2 48 32.1	2 48 31.1

JNO. W. REED,
Staff Commander in charge of China Sea Survey.

Remarks.—11^h 0^m A.M., wind W.S.W. 2 b. c., a hazy appearance about the horizon.

* Accompanied by a figure representing the sun touching the moon on the *upper right*, the line joining the centres being inclined about 45° to the vertical.

† Accompanied by a figure representing the sun as emerging *vertically beneath* the moon.

‡ Accompanied by a figure representing the sun touching the moon on the *lower right*, the line joining the centres being inclined about 45° to the vertical.

VII. "Further particulars of the Swedish Arctic Expedition, in a Letter addressed to the President, by Prof. NORDENSKIÖLD." Communicated by the President. Received October 15, 1868.

Kobbe Bay, Sept. 16th, 1868.

SIR,—In my last letter from Stockholm I promised to send you, with the returning naturalists, a detailed relation of the first scientific part of the Swedish Arctic Expedition of 1868; but unfortunately our last coal-ship, with which five of our fellow travellers, with the rich geological, zoological, and botanical collections, made during this season in the arctic regions, return to Tromsö, and which gives us the last occasion of communicating with Europe, leaves this harbour *in some hours*, and that makes it impossible for me to keep my promise. However, a detailed report will immediately be sent to you by one of the returning naturalists, Dr. Malmgren, a member also of the expeditions of 1861 and 1864. The remaining part of our expedition will from here go, first, to Seven Island, and then (perhaps one of the first days of October), after having deposited a boat and a depôt of provisions on *Ross Islet*, further northward. The polar sea was in the end of August quite covered with ice north of $81^{\circ} 9'$, the highest latitude hitherto reached by our steamer. But a week later the sea was open to Walden and Table Island, and the 8th of September I could, from one of the highest peaks of Parry Island, discern only traces of ice further *northward*.

I remain, Sir, respectfully yours,

A. I. NORDENSKIÖLD.

VIII. "Notice of an Observation of the Spectrum of a Solar Prominence, by J. N. LOCKYER, Esq., in a Letter to the Secretary." Communicated by Dr. SHARPEY. Received October 21, 1868.

October 20, 1868.

SIR,—I beg to anticipate a more detailed communication by informing you that, after a number of failures, which made the attempt seem hopeless, I have this morning perfectly succeeded in obtaining and observing part of the spectrum of a solar prominence.

As a result I have established the existence of three bright lines in the following positions:—

- I. Absolutely coincident with C.
- II. Nearly coincident with F.
- III. Near D.

The third line (the one near D) is more refrangible than the more refrangible of the two darkest lines by eight or nine degrees of Kirchhoff's

scale. I cannot speak with exactness, as this part of the spectrum requires remapping.

I have evidence that the prominence was a very fine one.

The instrument employed is the solar spectroscope, the funds for the construction of which were supplied by the Government-Grant Committee. It is to be regretted that its construction has been so long delayed.

I have &c.,

J. NORMAN LOCKYER.

The Secretary of the Royal Society.

IX. "On a New Series of Chemical Reactions produced by Light."

By JOHN TYNDALL, LL.D., F.R.S., &c. Received October 24, 1868.

I ask permission of the Royal Society to draw the attention of chemists to a form or method of experiment which, though obvious, is, I am informed, unknown, and which, I doubt not, will in their hands become a new experimental power. It consists in subjecting the vapours of volatile liquids to the action of concentrated sunlight, or to the concentrated beam of the electric light.

Action of the Electric Light.

A glass tube 2·8 feet long and of 2·5 inches internal diameter, frequently employed in my researches on radiant heat, was supported horizontally. At one end of it was placed an electric lamp, the height and position of both being so arranged that the axis of the glass tube and that of the parallel beam issuing from the lamp were coincident. The tube in the first experiments was closed by plates of rock-salt, and subsequently by plates of glass.

As on former occasions, for the sake of distinction, I will call this tube *the experimental tube*.

The experimental tube was connected with an air-pump, and also with a series of drying and other tubes used for the purification of the air.

A number of test-tubes (I suppose I have used fifty of them in all) were converted into Woulfe's flasks. Each of them was stopped by a cork through which passed two glass tubes: one of these tubes (*a*) ended immediately below the cork, while the other (*b*) descended to the bottom of the flask, being drawn out at its lower end to an orifice about 0·03 of an inch in diameter. It was found necessary to coat the cork carefully with cement.

The little flask thus formed was partially filled with the liquid whose vapour was to be examined; it was then introduced into the path of the purified current of air.

The experimental tube being exhausted, and the cock which cut off the supply of purified air being cautiously turned on, the air entered the flask

through the tube *b*, and escaped by the small orifice at the lower end of *b* into the liquid. Through this it bubbled, loading itself with vapour, after which the mixed air and vapour, passing from the flask by the tube *a*, entered the experimental tube, where they were subjected to the action of light.

The power of the electric beam to reveal the existence of anything within the experimental tube, or the impurities of the tube itself, is extraordinary. When the experiment is made in a darkened room, a tube which in ordinary daylight appears absolutely clean is often shown by the present mode of examination to be exceedingly filthy.

The following are some of the results obtained with this arrangement :—

Nitrite of amyl (boiling-point 91° to 96° C.).—The vapour of this liquid was in the first instance permitted to enter the experimental tube while the beam from the electric lamp was passing through it. Curious clouds were observed to form near the place of entry, which were afterwards whirled through the tube.

The tube being again exhausted, the mixed air and vapour were allowed to enter it in the dark. The slightly convergent beam of the electric light was then sent through the tube from end to end. For a moment the tube was *optically empty*, nothing whatever was seen within it; but before a second had elapsed a shower of liquid spherules was precipitated on the beam, thus generating a cloud within the tube. This cloud became denser as the light continued to act, showing at some places a vivid iridescence.

The beam of the electric lamp was now converged so as to form within the tube, between its end and the focus, a cone of rays about eight inches long. The tube was cleansed and again filled in darkness. When the light was sent through it, the precipitation upon the beam was so rapid and intense that the cone, which a moment before was invisible, flashed suddenly forth like a solid luminous spear.

The effect was the same when the air and vapour were allowed to enter the tube in diffuse daylight. The cloud, however, which shone with such extraordinary radiance under the electric beam, was invisible in the ordinary light of the laboratory.

The quantity of mixed air and vapour within the experimental tube could of course be regulated at pleasure. The rapidity of the action diminished with the attenuation of the vapour. When, for example, the mercurial column associated with the experimental tube was depressed only five inches, the action was not nearly so rapid as when the tube was full. In such cases, however, it was exceedingly interesting to observe, after some seconds of waiting, a thin streamer of delicate bluish-white cloud slowly forming along the axis of the tube, and finally swelling so as to fill it.

When dry oxygen was employed to carry in the vapour, the effect was the same as that obtained with air.

When dry hydrogen was used as a vehicle, the effect was also the same.

The effect, therefore, is not due to any interaction between the vapour of the nitrite and its vehicle.

This was further demonstrated by the deportment of the vapour itself. When it was permitted to enter the experimental tube unmixed with air or any other gas, the effect was substantially the same. Hence the seat of the observed action is the vapour itself.

With reference to the air and the glass of the experimental tube, the beam employed in these experiments was perfectly cold. It had been sifted by passing it through a solution of alum, and through the thick double-convex lens of the lamp. When the unsifted beam of the lamp was employed, the effect was still the same; the obscure calorific rays did not appear to interfere with the result.

I have taken no means to determine strictly the character of the action here described, my object being simply to point out to chemists a method of experiment which reveals a new and beautiful series of reactions; to them I leave the examination of the products of decomposition. The molecule of the nitrite of amyl is shaken asunder by certain specific waves of the electric beam, forming nitric oxide and other products, of which the *nitrate* of amyl is probably one. The brown fumes of nitrous acid were seen to mingle with the cloud within the experimental tube.

The nitrate of amyl, being less volatile than the nitrite, could not maintain itself in the condition of vapour, but would be precipitated in liquid spherules along the track of the beam.

In the anterior portions of the tube a sifting action of the vapour occurs, which diminishes the chemical action in the posterior portions. In some experiments the precipitated cloud only extended halfway down the tube. When, under these circumstances, the lamp was shifted so as to send the beam through the other end of the tube, precipitation occurred there also.

Action of Sunlight.

The solar light also effects the decomposition of the nitrite-of-amyl vapour. On the 10th of October I partially darkened a small room in the Royal Institution, into which the sun shone, permitting the light to enter through an open portion of the window-shutter. In the track of the beam was placed a large plano-convex lens, which formed a fine convergent cone in the dust of the room behind it. The experimental tube was filled in the laboratory, covered with a black cloth, and carried into the partially darkened room. On thrusting one end of the tube into the cone of rays behind the lens, precipitation within the cone was copious and immediate. The vapour at the distant end of the tube was in part shielded by that in front, and

was also more feebly acted on through the divergence of the rays. On reversing the tube, a second and similar cone was precipitated.

Physical considerations.

I sought to determine the particular portion of the white beam which produced the foregoing effects. When, previous to entering the experimental tube, the beam was caused to pass through a red glass, the effect was greatly weakened, but not extinguished. This was also the case with various samples of yellow glass. A blue glass being introduced, before the removal of the yellow or the red, on taking the latter away augmented precipitation occurred along the track of the blue beam. Hence, in this case, the more refrangible rays are the most chemically active.

The colour of the liquid nitrite of amyl indicates that this must be the case; it is a feeble but distinct yellow: in other words, the yellow portion of the beam is most freely transmitted. It is not, however, the transmitted portion of any beam which produces chemical action, but the absorbed portion. Blue, as the complementary colour to yellow, is here absorbed, and hence the more energetic action of the blue rays. This reasoning, however, assumes that the same rays are absorbed by the liquid and its vapour.

A solution of the yellow chromate of potash, the colour of which may be made almost, if not altogether, identical with that of the liquid nitrite of amyl, was found far more effective in stopping the chemical rays than either the red or the yellow glass. But of all substances the nitrite itself is most potent in arresting the rays which act upon its vapour. A layer one-eighth of an inch in thickness, which scarcely perceptibly affected the luminous intensity, sufficed to absorb the entire chemical energy of the concentrated beam of the electric light.

The close relation subsisting between a liquid and its vapour, as regards their action upon radiant heat, has been already amply demonstrated*. As regards the nitrite of amyl, this relation is more specific than in the cases hitherto adduced; for here the special constituent of the beam which provokes the decomposition of the vapour is shown to be arrested by the liquid.

A question of extreme importance in molecular physics here arises:—What is the real mechanism of this absorption, and where is its seat†?

I figure, as others do, a molecule as a group of atoms, held together by their mutual forces, but still capable of motion among themselves. The vapour of the nitrite of amyl is to be regarded as an assemblage of such molecules. The question now before us is this:—In the act of absorption, is it the molecules that are effective, or is it their constituent

* Phil. Trans. 1864.

† My attention was very forcibly directed to this subject some years ago by a conversation with my excellent friend Professor Clausius.

atoms? Is the *vis viva* of the intercepted waves transferred to the molecule as a whole, or to its constituent parts?

The molecule, as a whole, can only vibrate in virtue of the forces exerted between it and its neighbour molecules. The intensity of these forces, and consequently the rate of vibration, would, in this case, be a function of the distance between the molecules. Now the identical absorption of the liquid and of the vaporous nitrite of amyl indicates an identical vibrating period on the part of liquid and vapour, and this, to my mind, amounts to an experimental demonstration that the absorption occurs in the main *within* the molecule. For it can hardly be supposed, if the absorption were the act of the molecule as a whole, that it could continue to affect waves of the same period after the substance had passed from the vaporous to the liquid state.

In point of fact the decomposition of the nitrite of amyl is itself to some extent an illustration of this internal molecular absorption; for were the absorption the act of the molecule as a whole, the *relative* motions of its constituent atoms would remain unchanged, and there would be no mechanical cause for their separation. It is probably the synchronism of the vibrations of one portion of the molecule with the incident waves which enables the amplitude of those vibrations to augment until the chain which binds the parts of the molecule together is snapped asunder.

The *liquid* nitrite of amyl is probably also decomposed by light; but the reaction, if it exists, is incomparably less rapid and distinct than that of the vapour. Nitrite of amyl has been subjected to the concentrated solar rays until it boiled, and it has been permitted to continue boiling for a considerable time, without any distinctly apparent change occurring in the liquid *.

I anticipate wide, if not entire, generality for the fact that a liquid and its vapour absorb the same rays. A cell of liquid chlorine now preparing for me will, I imagine, deprive light more effectually of its power of causing chlorine and hydrogen to combine than any other filter of the luminous rays. The rays which give chlorine its colour have nothing to do with this combination, those that are absorbed by the chlorine being the really effective rays. A highly sensitive bulb containing chlorine and hydrogen in the exact proportions necessary for the formation of hydrochloric acid was placed at one end of the experimental tube, the beam of the electric lamp being sent through it from the other. The bulb did not explode when the tube was filled with chlorine, while the explosion was violent and immediate when the tube was filled with air. I anticipate for the liquid chlorine an action similar to but still more energetic than that exhibited by the gas. If this should prove to be the case, it will favour the view that

* On the 21st of October, Mr. Ernest Chapman mentioned to me in conversation that he once exposed nitrite-of-amyl vapour to the action of light. With what result I do not know.

chlorine itself is *molecular* and not *monatomic*. Other cases of this kind I hope, at no distant day, to bring before the Royal Society.

Production of Sky-blue by the decomposition of Nitrite of Amyl.

When the quantity of nitrite vapour is considerable, and the light intense, the chemical action is exceedingly rapid, the particles precipitated being so large as to *whiten* the luminous beam. Not so, however, when a well-mixed and highly attenuated vapour fills the experimental tube. The effect now to be described was obtained in the greatest perfection when the vapour of the nitrite was derived from a residue of the moisture of its liquid, which had been accidentally introduced into the passage through which the dry air flowed into the experimental tube.

In this case the electric beam traversed the tube for several seconds before any action was visible. Decomposition then visibly commenced, and advanced slowly. The particles first precipitated were too small to be distinguished by an eye-glass; and, when the light was very strong, the cloud appeared of a milky blue. When, on the contrary, the intensity was moderate, the blue was pure and deep. In Brücke's important experiments on the blue of the sky and the morning and evening red, pure mastic is dissolved in alcohol, and then dropped into water well stirred. When the proportion of mastic to alcohol is correct, the resin is precipitated so finely as to elude the highest microscopic power. By reflected light, such a medium appears bluish, by transmitted light yellowish, which latter colour, by augmenting the quantity of the precipitate, can be caused to pass into orange or red.

But the development of colour in the attenuated nitrite-of-amyl vapour, though admitting of the same explanation, is doubtless more similar to what takes place in our atmosphere. The blue, moreover, is purer and more sky-like than that obtained from Brücke's turbid medium. There could scarcely be a more impressive illustration of Newton's mode of regarding the generation of the colour of the firmament than that here exhibited; for never, even in the skies of the Alps, have I seen a richer or a purer blue than that attainable by a suitable disposition of the light falling upon the precipitated vapour. May not the aqueous vapour of our atmosphere act in a similar manner? and may we not fairly refer to liquid particles of infinitesimal size the hues observed by Principal Forbes over the safety-valve of a locomotive, and so skilfully connected by him with the colours of the sky?

In exhausting the tube containing the mixed air and nitrite-of-amyl vapour, it was difficult to avoid explosions under the pistons of the air-pump, similar to those which I have already described as occurring with the vapours of bisulphide of carbon and other substances. Though the quantity of vapour present in these cases must have been infinitesimal, its explosion was sufficient to destroy the valves of the pump.

Iodide of Allyl (boiling-point 101° C.).—Among the liquids hitherto subjected to the concentrated electric light, iodide of allyl, in point of rapidity and intensity of action, comes next to the nitrite of amyl. With the iodide of allyl I have employed both oxygen and hydrogen, as well as air, as a vehicle, and found the effect in all cases substantially the same. The cloud column here was exquisitely beautiful, but its forms were different from those of the nitrite of amyl. The whole column revolved round the axis of the decomposing beam; it was nipped at certain places like an hour-glass, and round the two bells of the glass delicate cloud-filaments twisted themselves in spirals. It also folded itself into convolutions resembling those of shells. In certain conditions of the atmosphere in the Alps I have often observed clouds of a special pearly lustre; when hydrogen was made the vehicle of the iodide-of-allyl vapour a similar lustre was most exquisitely shown. With a suitable disposition of the light, the purple hue of iodine-vapour came out very strongly in the tube.

The remark already made as to the bearing of the decomposition of nitrite of amyl by light on the question of molecular absorption applies here also; for were the absorption the work of the molecule as a whole, the iodine would not be dislodged from the allyl with which it is combined. The non-synchronism of iodine with the waves of obscure heat is illustrated by its marvellous transparency to such heat. May not its synchronism with the waves of light in the present instance be the cause of its divorce from the allyl? Further experiments on this point are in preparation.

Iodide of Isopropyl.—The action of light upon the vapour of this liquid is at first more languid than upon iodide of allyl; indeed many beautiful reactions may be overlooked in consequence of this languor at the commencement. After some minutes' exposure, however, clouds begin to form, which grow in density and in beauty as the light continues to act. In every experiment hitherto made with this substance the column of cloud which filled the experimental tube was divided into two distinct parts near the middle of the tube. In one experiment a globe of cloud formed at the centre, from which, right and left, issued an axis which united the globe with the two adjacent cylinders. Both globe and cylinders were animated by a common motion of rotation. As the action continued, paroxysms of motion were manifested; the various parts of the cloud would rush through each other with sudden violence. During these motions beautiful and grotesque cloud-forms were developed. At some places the nebulous mass would become ribbed so as to resemble the graining of wood; a longitudinal motion would at times generate in it a series of curved transverse bands, the retarding influence of the sides of the tube causing an appearance resembling, on a small scale, the dirt-bands of the Mer de Glace. In the anterior portion of the tube those sudden commotions were most intense; here buds of cloud would sprout

forth, and grow in a few seconds into perfect flower-like forms. The most curious appearance that I noticed was that of a cloud resembling a serpent's head: it grew rapidly; a mouth was formed, and from the mouth a cord of cloud resembling a tongue was rapidly discharged. The cloud of iodide of isopropyl had a character of its own, and differed materially from all others that I had seen. A gorgeous mauve colour was developed in the last twelve inches of the tube; the vapour of iodine was present, and it may have been the sky-blue produced by the precipitated particles which, mingling with the purple of the iodine, produced this splendid mauve. As in all other cases here adduced, the effects were proved to be due to the light; they never occurred in darkness.

I should like to guard myself against saying more than the facts warrant regarding the chemical effects produced by light in the following three substances; but the physical appearances are so exceedingly singular that I do not hesitate to describe them.

Hydrobromic Acid.—The aqueous solution of this acid was placed in a small Woulfe's flask, and carried into the experimental tube by a current of air.

The tube being filled with the mixture of acid, aqueous vapour, and air, the beam was sent through it, the lens at the same time being so placed as to produce a cone of very intense light. Two minutes elapsed before anything was visible; but at the end of this time a faint bluish cloud appeared to hang itself on the most concentrated portion of the beam.

Soon afterwards a second cloud was formed five inches further down the experimental tube. Both clouds were united by a slender cord of cloud of the same bluish tint as themselves.

As the action of the light continued, the first cloud gradually resolved itself into a series of parallel disks of exquisite delicacy; the disks rotated round an axis perpendicular to their surfaces, and finally they blended together to produce a screw surface with an inclined generatrix. This surface gradually changed into a filmy funnel, from the end of which the "cord" extended to the cloud in advance. This also underwent modification. It resolved itself into a series of strata resembling those of the electric discharge. After a little time, and through changes which it was difficult to follow, both clouds presented the appearance of a series of concentric funnels set one within the other, the interior ones being seen through the spectral walls of the outer ones; those of the distant cloud resembled claret-glasses in shape. As many as six funnels were thus concentrically set together, the two series being united by the delicate cord of cloud already referred to. Other cords and slender tubes were afterwards formed, and they coiled themselves in spirals around and along the funnels.

Rendering the light along the connecting-cord more intense, it diminished in thickness and became whiter; this was a consequence of the enlarge-

ment of its particles. The cord finally disappeared, while the funnels melted into two ghost-like films, shaped like parasols. The films were barely visible, being of an exceedingly delicate blue tint; they seemed woven of blue air. To compare them with cobweb or with gauze would be to liken them to something infinitely grosser than themselves.

In a second trial the result was very much the same. A cloud which soon assumed the parasol shape was formed in front, and five inches lower down another cloud was formed, in which the funnels already referred to were considerably sharpened. It was connected as before by a filament with the cloud in front, and it ended in a spear-point which extended 12 inches further down the tube.

After many changes, the film in front assumed the shape of a bell, to the convex surface of which a hollow cylinder about 2 inches long attached itself. After some time this cylinder broke away from the bell and formed itself into an iridescent ring, which, without apparent connexion with anything else, rotated on its axis in the middle of the tube. The inner diameter of this ring was nearly an inch in length, and its outer diameter nearly an inch and a half.

The whole cloud composed of these heterogeneous parts was animated throughout by a motion of rotation. The rapidity of the rotation could be augmented by intensifying the beam. The disks, funnels, strata, and convolutions of the cloud exhibited at times diffraction colours, which changed colour with every motion of the observer's eye.

Moisture appeared to be favourable to the production of these appearances; and it hence became a question how far they were really produced by the light: hydrobromic acid, even from its solution, fumes when it comes into contact with the aqueous vapour of the air; its residence in water does not appear to satisfy its appetite for the liquid. The same effect, as everybody knows, is observed in the solution of hydrochloric acid. Might not, then, those wonderfully shaped clouds be produced by an action of this kind, the presence of the light being an unnecessary accident?

The hydrobromic acid was permitted to enter the experimental tube and remain in diffuse daylight for five minutes. On darkening the room and sending the electric beam through it, the tube was optically empty. Two minutes' action of the light caused the clouds to appear, and they afterwards went through the same variety of changes as before.

No matter how long the hydrobromic acid was allowed to remain in the tube, no action occurred until the luminous beam was brought into play. The tube filled with the mixture of air, aqueous vapour, and hydrobromic acid was permitted to remain for fifteen minutes in the dark. On sending the beam through the tube it was found optically empty; but two minutes' action of the light developed the clouds as before.

Permitting the beam to pass through a layer of *water* before entering

the experimental tube, no diminution of its chemical energy was observed. Permitting it to pass through a solution of *hydrobromic acid*, of the same thickness, the chemical energy of the beam was wholly destroyed. This shows that the vibrations of the dissolved acid are synchronous with those of the gaseous acid, and is a new proof that the constituent atoms of the molecule, and not the molecule itself, is the seat of the absorption.

Hydrochloric Acid.—The aqueous solution of this acid was also employed and treated like the solution of hydrobromic acid. I intend to invoke the aid of an artistic friend in an effort to reproduce the effects observed during the decomposition, if such it be, of hydrochloric acid by light. But artistic skill must, I fear, fail to convey a notion of them. The cloud was of slow growth, requiring 15 or 20 minutes for its full development. It was then divided into four or five sections, every adjacent two of which were united by a slender axial cord. Each of these sections possessed an exceedingly complex and ornate structure, exhibiting ribs, spears, funnels, leaves, involved scrolls, and iridescent fleurs-de-lis. Still the structure of the cloud from beginning to end was perfectly symmetrical; it was a cloud of revolution, its corresponding points being at equal distances from the axis of the beam. There are many points of resemblance between the clouds of hydrochloric and hydrobromic acid, and both are perfectly distinct from anything obtainable from the substances previously mentioned; in fact every liquid appears to have its own special cloud, varying only within narrow limits from a normal type. The formation of the cloud depends rather upon its own inherent forces than upon the environment. It is true that, by warming or chilling the experimental tube at certain points, extraordinary flexures and whirlwinds may be produced; but with a perfectly constant condition of tube, specific differences of cloud-structure are revealed, the peculiarity of each substance stamping itself apparently upon the precipitated vapour derived from its decomposition.

When the beam before entering the experimental tube was sent through a layer of the aqueous acid, thirteen minutes' exposure produced no action. A layer of water being substituted for the layer of acid, one minute's exposure sufficed to set up the decomposition.

Hydriodic Acid.—The aqueous solution of this acid was also employed. On first subjecting it to the action of light no visible effect was produced; but subsequent trials developed a very extraordinary one. A family resemblance pervades the nebulae of hydriodic, hydrobromic, and hydrochloric acids. In all three cases, for example, the action commenced by the formation of two small clouds united by a cord; it was very slow, and the growth of the cloud in density and beauty very gradual. The most vivid green and crimson that I have yet observed were exhibited by this substance in the earlier stages of the action. The de-

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velopment of the cloud was like that of an organism, from a more or less formless mass at the commencement, to a structure of marvellous complexity. I have seen nothing so astonishing as the effect obtained, on the 28th of October, with hydriodic acid. The cloud extended for about 18 inches along the tube, and gradually shifted its position from the end nearest the lamp to the most distant end. The portion quitted by the cloud proper was filled by an amorphous haze, the decomposition which was progressing lower down being here apparently complete. A spectral cone turned its apex towards the distant end of the tube, and from its circular base filmy drapery seemed to fall. Placed on the base of the cone was an exquisite vase, from the interior of which sprung another vase of similar shape; over the edges of these vases fell the faintest clouds, resembling spectral sheets of liquid. From the centre of the upper vase a straight cord of cloud passed for some distance along the axis of the experimental tube, and at each side of this cord two involved and highly iridescent vortices were generated. The frontal portion of the cloud, which the cord penetrated, assumed in succession the forms of roses, tulips, and sunflowers. It also passed through the appearance of a series of beautifully shaped bottles placed one within the other. Once it presented the shape of a fish, with eyes, gills, and feelers. The light was suspended for several minutes, and the tube and its cloud permitted to remain undisturbed in darkness. On re-igniting the lamp, the cloud was seen apparently motionless within the tube; much of its colour had gone, but its beauty of form was unimpaired. Many of its parts were calculated to remind one of Gassiot's discharges; but in complexity and, indeed, in beauty, the discharges would not bear comparison with these arrangements of cloud. A friend to whom I showed the cloud likened it to one of those jelly-like marine organisms which a film barely capable of reflecting the light renders visible. Indeed no other comparison is so suitable; and not only did the perfect symmetry of the exterior suggest this idea, but the exquisite casing and folding of film within film suggested the internal economy of a highly complex organism. The *twoness* of the animal form was displayed throughout, and no coil, disk, or speck existed on one side of the axis of the tube that had not its exact counterpart at an equal distance on the other. I looked in wonder at this extraordinary production for nearly two hours*.

The precise conditions necessary to render the production of the effects observed with hydrobromic, hydrochloric, and hydriodic acids a certainty have not yet been determined. Air, moreover, is the only vehicle which has been employed here. I hazard no opinion as to the chemical nature of these reactions. The dry acids, moreover, I have not yet examined.

* "It is as perfect as if turned in a lathe." "It would prove exceedingly valuable to pattern-designers," were remarks made by my assistants as they watched the experiment. Mr. Ladd, who is intimately acquainted with the phenomena of the electric discharge through rarefied media, remarked that no effect he had ever seen could compete in point of beauty and complexity with the appearance here imperfectly described. I mention this to indicate how the phenomena affected other eyes than mine.

November 19, 1868.

Lieut.-General SABINE, President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting for the Election of Council and Officers was given from the Chair.

Mr. Currey, Mr. Hudson, Mr. Newmarch, Mr. Prestwich, and Mr. Stainton, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

Dr. Bastian, Rear-Admiral Cooper Key, and Mr. Vernon Harcourt were admitted into the Society.

The following communications were read :—

“On the Physical Constitution of the Sun and Stars.” By G. JOHNSTONE STONEY, M.A., F.R.S., F.R.A.S., Secretary to the Queen's University in Ireland. Received May 15, 1867. (See page 1.)

I. “Second List of Nebulæ and Clusters observed at Bangalore with the Royal Society's Spectroscope ;” preceded by a Letter to Professor G. G. Stokes. By Lieut. JOHN HERSCHEL, R.E. Communicated by Prof. STOKES. Received July 20, 1868. (See page 58.)

II. “On the Lightning Spectrum.” By Lieut. JOHN HERSCHEL, R.E. Communicated by Prof. STOKES. Received August 8, 1868. (See page 61.)

III. “Products of the Destructive Distillation of the Sulphobenzoates.”—No. II. By JOHN STENHOUSE, LL.D., F.R.S., &c. Received September 8, 1868. (See page 62.)

IV. “Compounds Isomeric with the Sulphocyanic Ethers.—II. Homologues and Analogues of Ethylic Mustard-oil.” By A. W. HOFMANN, Ph.D., M.D., LL.D. Received September 11, 1868. (See page 67.)

V. “Account of Spectroscopic Observations of the Eclipse of the Sun, August 18, 1868.” In a Letter addressed to the President of the Royal Society by Captain C. T. HAIG, R.E. Communicated by the President. Received September 21, 1868. (See page 74.)

VI. “Account of Observations of the Total Eclipse of the Sun, made August 18th, 1868, along the coast of Borneo.” In a Letter addressed to H.M. Secretary of State for Foreign Affairs, by His Excellency J. POPE HENNESSY, Governor of Labuan. Com-

municated by the Right Hon. Lord STANLEY, F.R.S. Received October 8, 1868. (See page 81.)

VII. "Further Particulars of the Swedish Arctic Expedition." In a Letter addressed to the President, by Professor NORDENSKIÖLD. Communicated by the President. Received October 15, 1868. (See page 91.)

VIII. "Notice of an Observation of the Spectrum of a Solar Prominence." By J. N. LOCKYER, Esq., in a Letter to the Secretary. Communicated by Dr. SHARPEY. Received October 21, 1868. (See page 91.)

IX. "On a New Series of Chemical Reactions produced by Light." By JOHN TYNDALL, LL.D., F.R.S., &c. Received October 24, 1868. (See page 92.)

X. "Account of the Solar Eclipse of 1868, as seen at Jamkandi in the Bombay Presidency." By Lieut. J. HERSCHEL, R.E. Communicated by Prof. G. G. STOKES, Sec. R.S. Received October 19, 1868.

To the President, Council, and Fellows of the Royal Society.

GENTLEMEN,—The time has arrived when I must offer for your acceptance a connected report of the employment of the instruments intrusted to me for the special purpose of observing the late solar eclipse.

1. Plan of this Report.

In framing this Report I propose in the first place to describe those instruments sufficiently in detail to render unnecessary such explanations as would otherwise be required in the course of my narrative, and then to show the circumstances which preceded their actual employment on that occasion.

2. Description of Telescope and clockwork.

The principal instrument is an equatorially mounted telescope, with a lens of 5 inches aperture and 62 inches focal length. The mounting is adapted to any latitude (except very low and very high ones), the polar axis being a moveable tangent to the circular-arched roof of the chamber containing the clockwork. The latter, as well as the rest of the instrument, is by Messrs. Cooke and Sons, of York, and is, as I understood from Mr. Cooke, of a somewhat novel description. I have not examined the mechanism closely, and therefore cannot describe it very accurately; but I believe the peculiarity consists in the maintenance of continuous motion in a fan-wheel, regulated by a pendulum time-keeper acted on through a remontoir escapement, whereby the irregularity of the surplus energy of the driving-weight, while it is prevented by the latter from interfering with the time-keeper at all, is modified in its action on the tube by the former. The *mean* rate of motion is

thus uniform ; and though there is very perceptible irregularity in the *actual* motion, it is not intermittent. Thus, when the image of a star, for instance, is brought on the slit of the spectrum-apparatus, the spectrum is fitful in appearance, if the slit is perpendicular to the direction of diurnal motion. The mean motion may be easily regulated as in a pendulum-clock. The motion is communicated by friction to the first of a series of wheels which terminates in an endless screw working in the circumference of a large toothed arc attached to the hour-axis. Motion imparted by hand to one of these wheels, grooved and provided for this purpose with an endless cord, is thus communicated directly to the tube without greater strain on the clock than is implied in overcoming the connecting friction.

3. *Its Mounting.*

The declination-axis terminates in a T-shaped head carrying two circular collars, in which the telescope-tube rests. For convenience in mounting and dismounting, these collars are attached to the T-head by nut and pins, so that they lift off with the tube, while the balance can be adjusted by releasing their grasp of the tube when required. This is a great convenience in a portable instrument. The tube can be dismounted and taken indoors readily without assistance ; and the body of the instrument (which, besides being far less easily handled, has cost hours of adjustment) may be left under a suitable waterproof case when no observatory has been constructed.

4. *Its Stand.*

The stand is a strong wooden one, of remarkably firm construction, considering that it is of the three-legged portable kind. Its upper surface is a stout brass annulus, on which the clock-chamber rests and rotates, if required, for adjustment in azimuth. Two of the legs have foot-screws for adjusting the level and completing the adjustment for latitude.

5. *Of the Spectroscope.*

The spectroscope intended for use with the above telescope was constructed by Messrs. Simms, on a pattern or design supplied (I believe) by Mr. Huggins ; but its construction was too much delayed to allow of a practical examination of all its parts before packing. It consists of a single flint-glass prism, of refracting angle 60° , contained in a cylindrical brass chamber, from which radiate three tubes in such directions as to fulfil the several purposes of (1) receiving the light to be analyzed, (2) delivering it after refraction and separation to the eye, and (3) admitting external light for reflection to the eye off the second surface of the prism. The first consists externally of a long connecting tube for insertion into the telescope in place of the ordinary eye-tube, where it is grasped in the focusing-slide. Internally it carries a smaller tube, carrying at one end a lens, and at the other, at the principal focal distance of the latter, a beautiful piece of workmanship by which a slit is obtained whose sides

approach each other equally. Half the length of this slit may be obscured by the intervention of a right-angled prism, which reflects a side light through it if required. The converging rays from the object-glass falling on the slit are admitted, while those which do not are stopped. The former diverging again, as though from a luminous line, emerge from the next lens and fall on the prism as parallel rays, are independently refracted and dispersed in traversing it, and after emergence are again condensed, but not reunited, by the object-glass in the small telescope composing the second of the above-mentioned tubes, and, forming a spectrum in its focus, are viewed as such by an eyepiece.

Means of measurement.—The direction of emergence defines the position in the spectrum ; and the difference of direction is measured by the change of direction of the small telescope necessary to receive the several refracted rays directly. This change of direction is effected and measured by a tangent screw, whose complete revolutions are indicated by the march of a graduated scale (attached to the telescope-arm) over a circle marked on the circumference of the divided cylindrical head of the screw. The position of the centre of motion of the telescope-arms, it should be said, though optically unimportant, is practically within the prism. By the help of a reading-lens the revolutions, and tenths and hundredths of a revolution, can be easily read off by a very slight movement of the eye from the eyepiece.

New graduated Scale for micrometer measures.—A mistake having occurred in graduating the scale, I substituted one of my own making. As I was fortunate in this, I may venture to describe how it was effected. The graduation required was too fine for any ink lines I could make ; I therefore varnished a piece of card, and drew fine lines at the proper intervals on the shellac-coating with a sharp blade ; and applying a little ink, these were instantly rendered visible. I then cut the card across the lines and glued the scale so formed over the old one with varnish, giving the whole a dash of varnish for the sake of protection. When dry I was gratified to find the graduation correspond well with the revolutions ; for it was rather a delicate job, and I did not succeed without failures.

6. *Graduated Scale in the Field of View.*

The third tube was intended to present in the field of view of the telescope last described, by external reflection off the second surface of the prism, an illuminated image of a photographed scale placed at one end of the tube, in the principal focus of a lens at the other. The tube carries a small moveable mirror outside. Upon this mirror was intended to be thrown the light of a small lamp, held in position by a bent arm projecting from the prism-chamber. I am sorry to say that this ingenious contrivance proved, in my hands, more unsatisfactory than perhaps it should have done. As in *not* using it I departed from the letter of my instructions, I am in a measure bound to explain my reasons for discarding it.

Reasons for discarding it.—In the first place, I never could with any kind of illumination train my eye to read the scale, partly because (whether from diffraction or irradiation) the image was never distinct, partly because the figures were illegible. In the next place the little lamp was capricious; either it refused to keep alight, or it boiled its own oil and melted off its handles, and ended by burning my fingers! Thirdly, it was an additional weight at the eye-end of the telescope and involved a counterpoise when not in use, and an additional projection to be avoided in every movement—in the dark,—all implying additional distractions and sources of failure. Lastly, I found I could do very well without it—in the preliminary training which I underwent on examining the nebulae. At the same time I must confess that I made an oversight in trusting too much to the illuminating power of a hand-lamp, as will be apparent when I come to describe the actual eclipse-observations.

7. Smaller Telescopes and Polarizers.

The second instrument supplied was an achromatic refractor of 3 inches aperture mounted with vertical and horizontal axes, the socket of the former being supported on a three-legged wooden stand, afterwards replaced by one of greater stability and more convenient height. Two cells, containing a double-image prism and quartz plate, and the combination known as Savart's polariscope, respectively, were supplied for use with this telescope, but without any connecting adaptation.

8. Hand Spectroscopes.

The other instruments were hand spectroscopes for direct vision, four in number, which I was directed to distribute according to circumstances. It is needless to describe these instruments, as they are well known; but I must venture to correct a statement made at a meeting of the Royal Asiatic Society last December, that they have a magnifying-power of 8 or 10. I do not think they can be credited with a higher power than 3; and I was never able to recognize any of the peculiar characteristics of nebular or stellar spectra, the recognition of which might have been expected with the higher magnifying-power.

9. Arrival in India and communication with Colonel Walker, R.E.

Soon after my arrival in India I communicated with Colonel Walker, with the object of receiving his instructions and of ascertaining whether he had decided on any plan, and, if not, to learn his views with reference to the assistance I might expect from the Survey Establishment. The choice of a station of observation and the disposal of the instruments were also discussed in the course of correspondence.

10. His Reply, and application to the Indian Government.

Colonel Walker's action in this matter has been most gratifying. He immediately promised me the assistance of Lieut. W. Maxwell Campbell,

of the Bombay Engineers, one of the executive officers of our Department, at that time engaged with myself and others in measuring a base-line in the neighbourhood of Bangalore, for the polarization-observations or otherwise, as I might arrange with him. He also placed at my future disposal for the occasion the services of Lieut. Campbell's assistants, in case such should be required, at the same time presenting to the Indian Government an urgent proposal to give the Royal Society's expedition both countenance and support. I enclose a copy of the reply to this proposal, in which it will be observed that the Governor General in Council "cordially approves," and "sanctions the necessary expenditure," and pledges the Government "to do everything in its power towards securing full and accurate observations" on the occasion—a pledge fully redeemed by the ready assent given to more than one other application. I am accordingly enabled to submit to your Society my present Report unaccompanied by any further appeal to your Treasurer.

11. *Steps taken to procure local information as to weather &c.*

The local Governments were also applied to to give effect to the circulation of a series of queries calculated to elicit local information as to probable climate at numerous points situated along the line of shadow. This was the more necessary, as my position at Bangalore (in the very centre of the peninsula) seemed to give a so much greater range of choice. In this respect also a warm interest was evinced. I wish I could add that the mass of correspondence which resulted was productive of an equal amount of valuable information. The practical value was chiefly confined to extracts from rain-registers, the principal question relating to probable cloudiness or otherwise being perhaps necessarily replied to too vaguely to form legitimate grounds for decision, owing in great measure to the fact that August is one of the most uncertain months in the year, in that respect, in southern India.

Rough notion of rain distribution across the peninsula in August.—On the whole, however, it appeared that across the whole width of the peninsula cloudy weather was to be expected at that season; and there was therefore no choice but what could be based on rainfall. The annexed diagram represents the impression (necessarily a vague one) remaining on



my mind after considering the reports. On the west coast anything up to 25 inches a week has been recorded in August; on the eastern slopes of

the western Ghats the fall seems both smaller and more regular, 6 to 10 inches being the usual fall in the month of August. Further inland we come to a tract notorious for its dryness, several places, such as Gokāk, Jamkandi, Bējápūr, and others thereabouts, being favoured with occasional showers only. I attributed this to the descent into a lower and hotter region of the prevailing south-west current, the greater part of whose moisture had been deposited during the disturbance of strata caused by passing over the sudden barrier of the Ghats. Beyond this again, eastwards, there is a gradual rise in the amount due in August, until towards the east coast the average fall is again 6 or 8 inches.

12. *Jamkandi selected.*

Jamkandi, the residence of a native chief, was among the first to attract my attention, partly owing to the offers of assistance which were made in the name of the chief; and this place was eventually selected for the advantages of climate which it appeared to offer.

13. *Distribution of the Instruments. Lieut. Campbell, R.E.*

In the meantime the distribution of the instruments was attended to. The smaller telescope with polarizing eyepieces was made over to Lieut. Campbell with a copy of the "Instructions," in the full assurance that he would acquaint himself with the theory and practice necessary to turn them to account. I annex a copy of his Report, the perusal of which will show that the instrument was in good hands. It is much to be regretted that he was not permitted to give more practical evidence of the forethought which characterized his preparations. I am also sorry that he has not given a fuller description of the ingenious contrivance which he designed and constructed for the ready application of the analyzers to the eyepiece. The annexed rough sketch (from memory) may help to give a correct idea of the contrivance.



I apprehend that in the event of fair weather he would be able to settle the question of polarity readily, and would have leisure to make use of a hand spectroscope as well. One of these instruments also was therefore made over to him.

14. *Captain Haig, R.E.*

Colonel Walker had further consented to allow another of our executive officers (Captain Haig, R.E.) to leave his regular duties for a time if he wished. As he was stationed at Poona and could avail himself of the railway as far as the border of the shadow's path, I offered him, and he accepted, the charge of another of the hand spectroscopes.

15. *Peninsular and Oriental Steam Navigation Company's Agent,
Captain Henry, Superintendent at Bombay.*

Lastly, I communicated with the agents of the Peninsular and Oriental Company at Calcutta and Madras Bay, and eventually intrusted the remaining two spectroscopes to the latter for employment on board two vessels, outward- and homeward-bound, which would probably be on the track at the right time.

16. *Memorandum of explanations and suggestions for use of Hand Spectroscopes.*

It was necessary, however, not only to distribute these instruments, but also to provide for their being intelligently employed in unpractised hands. I accordingly drew up a short memorandum with the object of putting it into the power of those interested to understand as much of the subject as seemed necessary, and of suggesting the probable appearances which might be presented. A copy of the pamphlet accompanies this Report.

17. *Examination of Nebulæ as bearing on the main subject.*

While these arrangements were in progress I was myself engaged with the equatorial in the examination of the southern nebulæ, to which I devoted as much time as the duties of my profession enabled me to do. The weather was very favourable in March (towards the middle of which month the base-line was completed), in April, and until the middle of May; but from that time until the latter end of June, when the instrument had to be despatched, I hardly got a single observation, owing to the setting-in of the south-west monsoon. I congratulated myself on having been able to use the fine nights we had had. The results, showing the nature of the spectra of about fifty nebulæ, have been already communicated to your Secretary; there is therefore no occasion to enter into particulars on this subject here, except as bearing on instrumental peculiarities not previously touched upon, and as suggesting the probability that a considerable familiarity with the special kind of observation had been acquired, as well as with the individual instrument. Those who are acquainted with the spectroscope as applied to a telescope will remember that it involves several additional screws to be attended to, and that the finding of these mechanically in the dark is no inconsiderable perplexity until habit has taught the way. But this by the way.

18. *The Finder, and the trouble it gave.*

The finder attached to the telescope has a very low magnifying-power and decidedly bad definition—so much so that even Saturn can scarcely be recognized with it; none but the most conspicuous nebulæ and clusters are visible; I have looked in vain for the planetary nebula in Lyra with it, though it was certainly in the field; and of all the planetary nebulæ in the southern hemisphere, only two (Nos. 2102 & 4510) are noted by me as

“visible in finder.” It was therefore almost always necessary to *find* with the principal, by the setting; and afterwards either to exchange the light eyepiece for the heavy spectroscope (removing at the same time a counterpoise) without disturbing the direction, if possible, or to take the bearings of the most conspicuous stars visible in the finder. But as there never was and never could be any certainty that in the act of insertion a disturbance sufficient to displace the image from the position the slit should occupy would not take place, the latter method became the surest, if the most troublesome. [The connecting-tube, I should remark, cost me, literally, days of worry and grinding before I could induce it to slide in and out at all.] If after these precautions the result of a blind search was negative, the whole had to be done *de novo*. What with removing and replacing the spectroscope, inserting eyepieces and counterpoises, setting the readings, searching in both finder and telescope, winding the driving-clock over and over again, in endless combination, all by the light of a bull’s-eye lantern, perhaps without catching a single spectrum all night, I often found four or five hours’ observing (?) more fatiguing than a long walk.

It may appear strange that I did not replace the finder by a better telescope. I can only say that India is not England, and Bangalore is not London. The idea did not occur to me as a practical one, and I was nervously afraid of making any alteration which *might* leave me worse off than I was. A bad finder was after all no great matter, for the eclipse and the nebulae could wait. At the same time I wish now that my finder *had* been more serviceable as a telescope for I got; but a poor sight of the eclipse with it.

19. *Further preparations, Observatory, &c.*

To return to my preparations. In the utter absence of any precise knowledge of the appearances which would be presented, but anticipating a faint spectrum as the most probable, all my preliminary arrangements had in view as complete an exclusion of external light as practicable. A wooden frame was constructed for an observatory with a revolving roof, the latter being covered with painted canvas. A large black curtain was provided, through the centre of which were to be passed the observing-end of the telescope and finder, and the declination-clamp and slow-motion screw. A segment of the octagonal observing-chamber would thus be in a great measure protected from the light which might be expected to enter the limited aperture in the roof.

20. *The Expedition leaves Bangalore.*

The instruments, observatory, and camp-equipage started from Bangalore on the 7th of July, and reached on the 7th of August—a creditable march of 390 miles in 31 days (including halts) in the height of the rainy season. My subsequent experience of the state to which so-called “made” roads may be reduced, in these parts of India, by a few days’ rain, afforded grounds

for self-congratulation that the journey was accomplished as quickly as it was. I followed on the 1st of August, and reached Jamkandi on the morning of the 14th. The journey was so exceedingly disagreeable a one that I shall say no more about it.

21. *Arrival at Jamkandi.*

By the evening of the 14th the observatory was put together and the telescope &c. ready for adjustment.

22. *Prospects.*

I was surprised and considerably disappointed to learn that the weather had been for some days past as cloudy as I found it. I had left heavy rain behind me at Belgaum, and found none at Jamkandi certainly; but the sky was thick with passing cloud. I was told that it was quite unusual, and that it could not last; but by the morning of the 18th both Lieut. Campbell and myself had made up our minds not to be disappointed (if we could help it), should we be denied more than a few glimpses.

23. *Bad weather not unusual at this season.*

I learned afterwards that at some time or other at that season a burst usually takes place on the Ghauts, causing a sudden and violent flood in all the rivers, and that the influence of this extends beyond their limits and occasions the fortnight of cloudy skies and scanty rainfall which such places as Jamkandi enjoy once a year. This periodical flood had occurred between the time of our camp's and our own arrival, and we were now experiencing the cloudy season. It was very unfortunate, but could hardly have been foreseen. Not only our own party, but others in the neighbouring district of Bájápúr were unlucky. Three days later the whole aspect of the country was changed. The rivers subsided; the heat which we had expected, but missed, began to make itself felt; the villanous black soil hardened; and the natives said confidently that their rainy season was past, and that the rivers would not rise again till next year.

24. *Lieut. Campbell's Station.*

On the 17th Lieut. Campbell selected his position on a hill about a mile distant. We had agreed that the character of the clouds was such that a greater separation was unnecessary, owing to their uniform distribution and regular current.

25. *Final preparations.*

I come at length to the more interesting part of my narrative. The three days and nights which preceded the event were occupied in adjusting the polar axis, in examining every adjustment that could or could not require it, in exchanging the broad coarse pointer which I had used for night work for a stout but sharp needle, in going over and over again a mental review of the probable appearances and the possible contingencies which might arise. The three months' disuse, too, since I had to give up the

nebulae, made fresh exercise necessary. Among other things, I concluded not to alter the pendulum, long ago adjusted for sidereal time. The difference of rate being only 1 in 365 for mean time (and 1 in 388 for solar time at that date), the telescope would only gain on the sun by less than one second during the $5\frac{1}{2}$ minutes of totality; so that even supposing I should wish to keep it directed on one and the same point the whole time, the practical effect would only be that that point would move along the slit by perhaps $\frac{1}{10}$ part of its visible length (estimating that length, or the width of the field, at $5'$). I mention this as the "Instructions" direct the adjustment to apparent solar time.

26. *Disuse of the Barlow Lens accounted for.*

In one other respect, too, I must plead guilty to a departure from the letter of those instructions, which hardly perhaps needs justification; I allude to the disuse of the Barlow lens. My reason was principally this, that its insertion keeps the observer some 6 inches further from the body of the instrument, and, besides involving a complete disturbance of equilibrium, puts him out of reach of the declination screw—results which I could not but think had not been contemplated. I should add that I was quite confident of the practicability of catching a prominence, without having its image doubled in size, though I was by no means so sure that I could spare any of the light, which would be reduced one-fourth.

27. *Care in adjusting the Pointer during the approach of the Moon.*

During the advance of the moon, and up to the last available moment, I paid particular attention to the collimation (I use the word in its true sense of *aim*) of the needle-point, being perhaps unnecessarily anxious to avoid my old difficulty of finding my object in the spectroscope. The sharp cusps were well suited to this purpose, and the sun-spots were good tests. I had been fortunate in getting the pointer very exact, and was therefore not troubled with any collimation-error to allow for.

28. *Spectrum at the Moon's centre.*

While thus employed I had occasion to remark that at the centre of the moon, some nine or ten minutes before totality, the intensity of the solar spectrum was much about the same as that of the full moon.

29. *Measurement of Solar Lines.*

Intensity of Spectrum of Limb at D.—The principal solar lines were measured at intervals during the advancing eclipse. A few minutes before totality, in going over these lines for the last time, the slit being as wide as was allowable for full sunlight, i. e. very narrow, I recorded an increasing brilliancy in the spectrum in the neighbourhood of D, so great in fact as to prevent any measurement of that line till an opportune cloud moderated the light. I am not prepared to offer any explanation of this. The clouds were

arranged in two distinct strata, the lower one containing masses hurrying past with the monsoon-current at no great height, the upper consisting of light, thinly scattered cirri showing very little motion. It is conceivable that the latter may have been obstinately interposed until the time when I remarked the recorded brilliancy; but I cannot say that I should be satisfied with such an explanation.

Whiteness of the Crescent.—I also remarked that the *whiteness* of the crescent, as seen in the finder, was apparently intensified as it grew narrower. Possibly this was the effect of contrast with the darkening background; for at this time I began to be annoyed by the appearance of five or six phantom crescents, which seemed to be trying to rival the legitimate one. I imagine I was indebted to the dark glass for these apparitions; but whatever called them up, they most effectually confused the view of the closing scene; whatever might otherwise have been seen at this stage was swamped in the confusion.

30. *Restlessness during approach of shadow.*

Up to within about ten minutes of totality I was every now and then outside watching progress through one or the other of two smaller telescopes of moderate power, one of which I had borrowed from the chief, who indulges a taste for the possession of English manufactures to an extraordinary degree. I noticed no marked inequalities of surface in the advancing limb, nor any bluntness of the cusps; but I must allow that I was not in a sufficiently composed state of mind to observe critically anything not bearing directly on the special problem before me. I was impressed with a notion that everything must be subordinated, in my case, to the requisite freedom of attention when totality commenced, and was specially anxious to *save my eyesight*. I studiously avoided looking at the sun except under cover of a cloud; and though I had provided the telescopes with graduated smoked glasses, I was nervously afraid to look through them too long or too intently—all which can only be understood by referring to what has been said about the absence of any foreknowledge of the impending revelation. My last view of external appearances showed nothing very striking—a few deeply neutral-tinted patches of sky in the zenith, and an increasing gloominess in all directions, being all the phenomena whose impression has outlived the excitement of the shortlived minutes which ensued. I reentered the observatory, and retired behind my black curtain to watch the event.

31. Gentlemen, I have thus far endeavoured to lay before you, as far as possible, in an orderly manner, an outline of the preliminary arrangements for the employment of your Society's instruments, and a sketch of my proceedings up to the hour of the eclipse. If in so doing I have been unnecessarily tedious, I would ask you to remember that these few pages but faintly represent the months of anxious study and preparation which have passed since I accepted the responsibility involved in the charge of an ex-

pedition deputed by the illustrious body I have now the honour of addressing—a responsibility more engrossing, it may be, but not lessened, by the specific but novel character of the proposed object of the expedition. I proceed now to describe how far that object has been attained; and here I feel that I cannot well indulge in too great a minuteness of detail.

32. Relative positions of Pointer, Slit, and Sun's Limb.

The spectroscope may be inserted, and employed with its slit in any direction perpendicular to the optical axis of the telescope. It is therefore competent to the observer to place the slit perpendicular or tangential to the sun's circumference at any point; and there can be no doubt that, were the observations conducted at leisure, it would be desirable to examine the whole circumference in both positions; but the operation of turning the spectroscope is not so very simple a one but that the advantages and disadvantages of any such proceeding require to be well considered where time is of the first importance. I decided on employing the slit in one direction only, that which corresponded with the diurnal motion. It so happened that this corresponded nearly with the direction of the relative motion of the sun and moon, so that the widest part of the crescent could be made to fall nearly perpendicularly across the slit. The needle (in the finder) and its point accurately represented the direction and centre respectively of the slit; therefore, when the needle-point touched the sun's limb at the centre of the crescent, a solar spectrum of definite width appeared in the field, of which one edge (the right-hand) continued stationary, while the other (the left) advanced slowly but perceptibly towards it, the solar spectrum decreasing visibly in width.

33. Last view of Solar Spectrum.

About a minute's breadth remained. A few seconds more and it would vanish suddenly. Whatever spectrum the corona could show must then be revealed, unless indeed a "prominence" or "sierra" should happen to be situated at that precise spot, in which case the double spectrum should be presented. The nervous tension at the moment may be conceived: what would be seen? what call for action would be made? and for what action? or, if nothing were seen, what would have to be done? I cannot say that I was prepared, at that moment, either with these questions, or with ready answers to them; but that was the sensation. With regard to the last, I suppose I should have instinctively widened the slit; and had that failed, should then have gone to the finder to look for a prominence. As it was, the spectrum faded out as I looked, while it had still appreciable width, and I knew a cloud had intervened.

Totality commences unseen.—A few seconds more and the spectrum of diffuse light vanished also, and told me the eclipse was total, but behind a cloud.

34. On the watch for a glimpse.

I went to the finder, removed the dark glass, and waited; how long, I

cannot say ; perhaps half a minute. Soon the cloud hurried over ; following the moon's direction, and therefore revealing first the upper limb, with its scintillating corona, and then the lower.

A prominence seen and aimed at.—Instantly I marked a prominence near the needle-point, an object so conspicuous that I felt there was no need to take any precautions to secure identification. It was a long finger-like projection from the (real) lower left-hand portion of the circumference. A rapid turn of the declination-screw covered it with the needle-point, and in another instant I was at the spectroscope. A single glance and the problem was solved.

Its Spectrum.—THREE VIVID LINES, RED, ORANGE, BLUE ; NO OTHERS, AND NO TRACE OF A CONTINUOUS SPECTRUM.

35. *Measurement of lines undertaken, with partial success.*

When I say the problem was solved, I am of course using language suited only to the excitement of the moment ! It was still very far from solved, and I lost no time in applying myself to measurement. And here I hesitate ; for the measurement was not effected with anything like the ease and certainty which ought to have been exhibited. Much may be attributed to haste and unsteadiness of hand, still more to the natural difficulty of measuring intermittent glimpses ; but I am bound to confess that these causes were supplemented by a failure less excusable. I have no idea how those five minutes passed so quickly ! Clouds were evidently passing continually ; for the lines were only visible at intervals—not for one-half the time, certainly—and not always bright ; but still I ought to have measured them all. My failure was in insufficient illuminating power ; but *why*, I cannot tell. I never experienced any difficulty of the kind with the nebulae, which required that I should flash in light suddenly over and over again. I had found the hand-lamp the surest way ; but it failed me here in great measure. The *red* line must have been less vivid than the *orange* ; for after a short attempt to measure it, I passed on to secure the latter.

Two lines measured.—In this I succeeded *to my satisfaction*, and accordingly tried for the *blue* line. Here I was not so successful. The glimpses of light were rarer and feebler, the line itself growing shorter and, what remained of it, further from the cross. I did, however, place the cross wires in a position certainly very near the true one, and got a reading before the reillumination of the field told me that the sun had reappeared on the other limb. These readings were called out, as those of the solar lines had been, to my recorder ; and it was only afterwards that I compared them.

I need not dwell on the feelings of distress and disappointment which I experienced on realizing the fact that the long-anticipated opportunity was gone, and, as it seemed to me then, *wasted*. I seemed to have failed entirely. Almost mechanically I directed the telescope to the bright limb, to verify the readings of the solar lines ; and in so doing my interest was

again awakened by the near coincidence, as it seemed, of the line F with the position of the wires; but a little reflection convinced me that the distance of the former was greater than the error which I might have made in intersecting the blue line.

Their readings and those of the solar lines.—I read F, and then D & C. The following were my readings up and down :—

	C.	D.	b.	F.
Before	{ 1.91	2.96	4.58	5.64
	{ 1.90	2.94	4.56	5.61
	{ 1.93	2.98	4.60	5.65
	{ 1.92	2.97	4.58	5.62
Bright lines	...	[3.00]	...	[5.56]
After.	1.93	3.00	...	5.65

36. *Identity of the Orange Line.*

I consider that there can be no question that the ORANGE LINE was identical with D, so far as the capacity of the instrument to establish any such identity is concerned.

37. *Of the Blue Line : doubtful.*

I also consider that the identity of the BLUE line with F is not established; on the contrary, I believe that the former is less refracted than F, but not much.

38. *Of the Red Line : uncertain.*

With regard to the RED line, I hesitate very much in assigning an approximate place: B and C represent the limits; it might have been near C; I doubt its being so far as B; I am not prepared to hazard any more definite opinion about it. Its colour was a *bright* red. This estimate of its place is absolutely free from any reference to the origin of the lines C and F.

39. *Subsequent mental aberration : not unusual.*

It is a fact not unworthy of notice that in all the accounts of eclipses, written soon after the event, which I have read, the record hurries rapidly to a close after the sun has reappeared; the reason, no doubt, is that a reaction takes place after the excitement of witnessing the actual eclipse, and phenomena which might be noticed after, quite as well as before, pass unregarded on that account. For my part I was surprised to find how utterly indifferent I felt to the appearance of things when I came out of my observatory. I am almost ashamed to confess that I went straight to my tent, and tried to write down what I *had* seen, instead of going to the telescope to watch for what still might be seen. It never even occurred to me to remove the spectroscope and use the fine telescope I had at command.

40. *Afterconsideration of the phenomena witnessed.*

I have not quite exhausted the statement of my observations, though

what I have still to state was rather the result of subsequent reflection than of actual cognizance at the time. I said that the prominence was situated close to the needle-point. I estimate its position as at the east point, a few degrees to the left of the lowest, of the sun's limb. Its form was that of a projecting *finger* slightly curved to the southward, and its height nearly 2'. The slit was at right angles to the hour-circle, and therefore perpendicular to the sun's limb at this point. A vertical section (so to speak) of the prominence was therefore admitted through the slit. It appears, then, that the length of the lines corresponded with the height of the prominence, being limited (as in the case of the spectrum of the section of the crescent) on the one hand (the left) by the advancing moon's limb at the centre of the field, and on the other by the natural summit of the prominence, or flame, as we are now entitled to call it.

Spectrum of Corona not seen.—Beyond this summit the light of the corona was free to enter ; it was also free to enter *with* that of the flame ; but I saw the spectrum of the latter *only*. I thence conclude that the spectrum of the corona was a faint solar one,—a conclusion quite in accordance with the other characteristics of this phenomenon, such as the radiated appearance and the evidence from polarity, indicating a central source of light. With regard to the latter, it is clear that the light of the corona is polarized in planes passing through the sun's centre (as the gist of Lieut. Campbell's Report), and therefore that the corona shines mainly by reflected light. At the same time it is possible that the absence of a spectrum of the corona at this particular spot may have been accidental. I have since heard that the corona was particularly feeble at this point. I had no opportunity of studying the corona myself. After first catching sight of the eclipse in the finder, I never left the spectroscope but once, when a long interval of cloudiness sent me to the finder to make sure. I then caught a few seconds' glimpse again, and remarked a red blot (I recognized no shape) of a prominence at about the north point, or rather to the west of it.

41. *Remarks on the ease with which the lines might be measured, and suggestions for future observations.*

I have now a few remarks to add which may be of use to future observers, if not of any present value. It is difficult to say what might or might not have been done but for the clouds ; but I am pretty certain that (even labouring, as I was, under the difficulty of bad illumination) not only might all three lines have been satisfactorily measured, but time would have sufficed for further examination. The course which that examination should take is a question which it is of the highest importance for an observer to decide on previously. I believe I was right in using a narrow slit to begin with, *not anticipating such a totally dark field* ; but I should not do so again ; or if I did, with the object of getting exact measures of the three principal lines, I should be prepared to widen the slit

to look for faint ones, the positions of which I should *estimate* with reference to those three. I should then direct the telescope at the brightest part of the corona, taking very good care to *prefer* a part free from any appearance of sierra, and if possible near the east or west points, so that the slit might admit a vertical section. Assuming that the corona does not emit *tosochromatic* light—if I may be allowed to coin a word to indicate definite but unspecified colours, both in respect of number and tint (or pitch)—of very distinct character, the spectrum of such a vertical slice *might* indicate by its varying *width* that the light was not uniformly constituted. Another point to be ascertained is whether all flames are constituted alike. This would require a more or less rapid glance at the spectra of several. I have spoken of “the three principal lines” because I saw no others. I have, however, heard rumours of a greater number having been seen by other observers, whether of equal brilliancy or not I do not know; but it inclines me to enforce the statement I have already made of “three vivid lines—no more,” as seen with a narrow slit. I had no suspicion whatever of the presence of any but those three; and as I first saw them they were as sharp and bright as one could well wish to see. Whether the prominence which I looked at was the same as those in which more than three lines were seen I do not know.

42. *Lieut. Campbell's Observations satisfactory in their result.*

The determination of the polarization-plane of the corona is as satisfactory as can be desired, and Lieut. Campbell's account is so clear that I have little to say about it. It is to be regretted that he did not see the effect of polarization *all round* at the same time, with a power low enough to include the whole of the phenomena; but the view fortunately obtained with the higher power remedies this in great measure by showing what *would* have been seen at points 90° distant from that which he describes.

43. *Results with Hand Spectroscopes unknown.*

With regard to the hand spectroscopes I have scarcely any report to make. Lieut. Campbell had no opportunity. Capt. Haig has sent no report. Neither have I heard anything of one of the two sent to sea. The only record I have received is that of Capt. Rennoldson, of the ‘Rangoon,’ P. & O. Co.'s Steam Ship, which I enclose. He mentions having seen with the spectroscope a prominence not seen by others with (I presume) ships' glasses of greater power. This is difficult to understand, except on the supposition that the light of the corona was weakened by dispersion, while that of the flame was not, or not in so great a degree. Should it turn out that the prominence he describes was a reality, it is barely possible that the above explanation may be the true one; in which case it suggests the possibility of seeing the prominences with a heavy battery of prisms when the sun is *not* eclipsed, especially if they are made of yellow glass; nay, even of seeing them, without the help of dispersion, through a medium calculated to stop all light but that of the sodium flame.

44. *Mr. Chambers prevented by Clouds from using two other Spectroscopes.*

Two other hand spectroscopes in my possession were lent to Mr. Chambers, Government Astronomer at Bombay, who stationed himself not far from Bégápúr; but I am sorry to say he was denied the opportunity of using them by the clouds.

Gentlemen, I beg to apologize for the length of my narrative, and to subscribe myself, with much respect,

Your obedient Servant,

J. HERSCHEL, Lieut. R.E.

Bangalore, Sept. 1868.

LIEUT. CAMPBELL'S REPORT.

"I was deputed to accompany Lieut. Herschel on his expedition to observe the phenomena of the total eclipse, and to use the instruments supplied by the Royal Society for the observation of polarized light in the corona and red flames.

"The instruments in question were as follows:—A telescope of 3-inch aperture, mounted on a rough double axis, admitting of motion in azimuth and altitude by hand only, unaided by any appliance for clamping and slow motion. The telescope was provided with three eyepieces of magnifying-powers 27, 41, and 98; and with it were furnished two analyzers for polarized light, viz. a double-image prism and a 'Savart's polariscope.'

"The first gives two images of the object viewed, which, when polarized light is present, become strongly coloured with complementary tints, by whose changes, according to the position in azimuth of the analyzer, the plane of polarization may be found.

"The second shows the presence of polarized light by the formation, across the image of the object viewed, of coloured bands, which alter in arrangement and intensity according to the position of the polariscope with reference to the plane of polarization, and hence afford a means of arriving at a knowledge of the latter.

"With the former, slight polarization would probably be more readily recognized at a glance; while with the latter the plane of polarization could be more easily and accurately determined.

"To carry these arms I had a pair of jointed arms constructed, so attached by a collar and screw to the eye-tube of the telescope as to admit of the eyepiece being changed. Each arm carried one of the analyzers in a cell, in which a rotatory motion could be given for analyzing purposes.

"Either analyzer could in this way be brought instantly into position before the eyepiece of the telescope, or both could be turned aside and the telescope used by itself at pleasure.

"Immediately behind the apparatus a circular piece of cardboard of about 12 inches diameter and neatly graduated was firmly attached to the eye-tube, and to each analyzer was affixed a long pointer by which its

azimuth could be referred to the graduations on the card circle, should measures of position or change of azimuth appear desirable.

“I was also furnished with a hand spectroscope for direct vision.

“The point chosen for my station was on the northern slope of a low range of hills, about $1\frac{1}{2}$ mile W. by S. of Jamkandi. The flatness of the hills on top offered no point from which an uninterrupted view could be obtained in all directions; and from my station I only had a view of the northern half of the distant horizon over the plains extending in that direction for many miles, above the level of which I was raised about 200 feet,

“Early on the morning of the 18th I proceeded to the spot, having previously sent up the instruments and a tent for shelter in case of necessity.

“At sunrise the sky was beautifully clear, except in the northern horizon, where there were low clouds lying over the river Kistna. There was a gentle breeze from S.W. by W. A little later light flocculent clouds began to rise and form in an arch overhead from west to east, continuing to increase as the morning wore on; then a light scud set in, and turned gradually into broken masses of thick dark clouds.

“Before the commencement of the eclipse I took observations for time with a small theodolite, from which I computed the error of my chronometer (a mean time one by M'Cabe) to be $1^h 14^m 55^s.5$ *fast* on local apparent time; and by that quantity I have accordingly corrected all observed chronometer times in the statements of time which follow.

“I observed the first contact, which took place at $7^h 45^m 13^s$ (local apparent time), about 15° from the vertex; after which I watched the progress of the eclipse, and noted the times of occultation of three sun-spots. No. 1 was a large double ragged spot, No. 2 a small well-defined one, No. 3 also double, but not so large or distinct as No. 1. After totality I saw a fourth spot very near the sun's limb.

“During the progress of the eclipse I observed no unevenness in the moon's limb, nor any want of sharpness in the cusps, using magnifying-power 27.

“The following notes were taken on the spot:—

At first contact. Sun very slightly obscured by clouds.

At $8^h 0^m$. Clouds thick, and gathering from S.W. and W. Wind higher and gusty.

At $8^h 10^m$. Clouds overhead, increasing and thickening and rising steadily from west.

At $8^h 20^m$. Sky nearly entirely overcast; clouds thickest in neighbourhood of sun.

At $8^h 25^m$. A clear break.

At $8^h 30^m$. I thought I could discern very faintly the dark limb of the moon beyond that of the sun; and at this time, making allowance for the general cloudiness, I did not perceive any decrease of light on the landscape.

At 8^h 40^m. But ten minutes later the darkness was decided.

At 8^h 45^m. Thick clouds well broken up, still gathered most closely in the region of the sun. Light becoming lurid, and increase of darkness very apparent.

At 8^h 52^m. Cusps perfect (magnifying-power 27).

"Closely before totality a bright line of light appeared to shoot out at a tangent to the moon's limb at its centre, as if running across the bright crescent of the sun (though of course not visible against the superior light) and extended beyond each cusp to a distance nearly, if not quite, 15'.

[Note by Lieut. H. The sketch in the margin represents Lieut. Campbell's meaning, as ascertained orally.]



The corona became visible immediately after, between the dark limb of the moon and the bright line. The corona did not appear so bright as the line, the brilliance and whiteness of the light of which was most striking. This was seen through a highly smoked glass. At this period, probably not more than 3 to 5 seconds before totality ensued, a thick cloud shut out everything, and the rest of the phenomenon was only seen fitfully through openings in the clouds, for an aggregate period which I estimate at somewhat less than half that of totality.

"This alternate appearance and disappearance troubled me greatly, and gave rise to nervousness and excitement; for owing to the imperfect mounting of my telescope I was apt to lose my place whenever the light was cut off by clouds, and to waste the precious moments of clearness in finding it again.

"On the first opportunity after the commencement of the eclipse I turned on the double-image prism with the eyepiece of 27 magnifying-power, as recommended in the Instructions, which gave a field of about 45' diameter. A most decided difference of colour was at once apparent between the two images of the corona; but I could not make certain of any such difference in the case of a remarkable horn-like protuberance, of a bright-red colour, situated about 210 degrees from the vertex, reckoned (as I have done in all cases) with reference to the actual, not the inverted image, and with direct motion. I then removed the double-image prism and applied the Savart's polariscope, which gave bands at right angles to a tangent to the limb, distinct but not bright, and with little, if any, appearance of colour. On turning the polariscope in its cell the bands, instead of appearing to revolve on their own centre, passing through various phases of brightness, arrangement, &c., travelled bodily along the limb, always at right angles thereto, and without much change in intensity, or any at all in arrangement.

"The point at which they seemed strongest was about 140° from the vertex, and I recorded them as black centred.

"Believing that with a higher power and a smaller field I should find it easier to fix my attention on one point of the corona and observe the phases

of the bands at that point, I changed eyepieces applying that of 41 power. With this eyepiece the first clear instant showed the bands much brighter than before, coloured, and as tangents to the limb at a point about 200° from the vertex; but before I could determine anything further a cloud shut out the view, and a few seconds later a sudden rush of light told that the totality was over, though it was difficult to believe that five minutes had flown by since its commencement.

“I experienced a strong feeling of disappointment and want of success; the only points on which I can speak with any confidence being as follows:—(1) When using the double-image prism, the strong difference of colour of the two images of the corona, and the absence of such difference in the case of the most prominent red flame. (2) With the ‘Savart’s polariscope’ the bands from the corona were decided; with a low power they were wanting in intensity and colour; excepting alternate black and white, making it difficult to specify the nature of the centre; and their position was at right angles to the limb, extending over about 30° of the circumference. When the polariscope was turned the bands travelled bodily round the limb without other change in position or arrangement, as if indeed they were revolving round the centre of the sun as an axis. With a higher power, when a smaller portion of the corona was embraced, the bands were brighter, coloured, and seen in a different position, viz. tangents to the limb.

“The appearance observed with a low power seems exactly what might be expected, supposing the bands to be brightest at every point when at right angles to the limb, in which case the bands growing into brightness at each succeeding point of the limb would distract attention from those fading away at the points passed over as the analyzer revolved.

“After totality was over the clouds cleared away somewhat, and I watched the eclipse till its conclusion, noting the times of emersion of the spots and of last contact.

“A light shower fell at 9.30.

“During totality several stars and planets were seen by those who were with me; and a fowl which I had placed near me, out of curiosity, was observed to compose itself to sleep. It was at no time so dark as I had expected: after the total phase had commenced I read the chronometer and wrote notes in pencil without difficulty; and the light of a bull’s-eye lantern when thrown on my paper appeared somewhat dull.

“The brilliance of the light of the corona when it burst out through the openings in the clouds astonished me. Also the very gradual decrease of light before totality, and the wonderful flood of light which followed the instant of the sun’s reappearance (though behind a cloud) were very striking.

“I was too much occupied in watching the position of the sun, so as not to lose an instant of the precious intervals of clearness, to see much of the general effect. I had no opportunity of using the hand spectroscope.

There was no one in my neighbourhood (except those of my own party, who had been warned to keep silence), but when totality commenced a wailing shout was heard in the distance, apparently rising all round us, which was succeeded after a few seconds by silence.

"The distant features of the landscape disappeared, and I noticed one light (apparently a village fire) some miles distant.

"I give below the different times I observed as of possible interest. Local apparent time is used :—

	First contact.	Last contact.
	h m s	h m s
Sun and moon.....	7 45 13	10 21 59
Spot No. 1.....	7 57 39	9 7 5
Entire disappearance	7 59 5	
Spot No. 2.....	8 40 28	9 54 39
Spot No. 3.....	8 46 58	10 3 25

I cannot state with any approach to accuracy either the instant of commencement or [that of] termination of totality."

Latitude of station 16° 30' 10"
Longitude ,, 75 20

(Signed) "W. R. CAMPBELL, Lieut. R.E."
"Bangalore, August 31, 1868."

True copy.

J. HERSCHEL, Lieut. R.E.

Bangalore, September 15, 1868.

(Copy.)

No. 886.

From J. Geoghegan, Esq., Under Secretary to Government of India.

To The Superintendent of the Great Trigonometrical Survey of India.

Fort William, February 21, 1868.

SIR,—I am directed to acknowledge receipt of your letter No. $\frac{6}{8}$ of 4th instant ; requesting permission to employ certain officers of the Government Trigonometrical Survey in taking observations of the total solar eclipse of the 17th, 18th August, and asking sanction to the expenditure on this account estimated roughly not to exceed 2000 Rupees.

In reply, I am directed to state that the Governor-General in Council cordially approves of your proposed arrangements, and sanctions the necessary expenditure.

The Government of India, I am to state, will be prepared to do every-

1868.] Capt. D. Rennoldson *on the Solar Eclipse of 1868.* 125

thing in its power towards securing full and accurate observations on this rare and important occasion.

I have, &c.,

(Signed) J. GEOGHEGAN,
Under Secretary to Government of India.

True copy.

J. HERSCHEL, Lieut. R.E.

[Commander Rennoldson's letter, which was sent independently by the Secretary of the Peninsular and Oriental Steam Navigation Company appears below.]

*XI. "Observations of the Total Solar Eclipse of August 18, 1868."

By Captain CHARLES G. PERRINS. Communicated by Prof. STOKES. Received October 30, 1868.

(Abstract.)

These observations are contained in a letter dated "S.S. 'Carnatic,' Suez, 28th August, 1868," addressed to the Managing Directors, Peninsular and Oriental Steam Navigation Company. One of the hand spectroscopes sent out by the Royal Society had been entrusted to Captain Perrins; but as his ship at the time of the eclipse was about 20 miles north of the track of the total phase, he had no opportunity of using it for the observations contemplated. He thus describes the appearance at the time of greatest obscuration:—

"That portion of the sun remaining uneclipsed consisted of a narrow streak (in shape like a crescent) of its upper left limb, in size about $\frac{1}{8}$ part of its diameter. The light emitted from this was of a very peculiar description and difficult to describe, being at the same time extremely brilliant and yet most remarkably pale. The high sea running appeared like huge waves of liquid lead, and the ghastly paleness of the light thrown upon it and all around revealed a scene which, for its weird-like effect, it would be as impossible to depict as it is to describe."

The slender crescent showed in the spectroscope several dark lines, as was to be expected.

XII. "Observations of the Total Solar Eclipse of August 18, 1868."

By Captain D. RENNOLDSON. Communicated by Prof. STOKES. Received October 30, 1868.

(Copy.)

From Captain D. Rennoldson.

"Peninsular and Oriental Company,
Bombay, 22nd August 1868.

"DEAR SIR,—I enclose you a sketch of the eclipse seen on board the

* This and the following three communications were transmitted by the Directors of the Peninsular and Oriental Steam Navigation Company.

126 Capt. D. Rennoldson *on the Solar Eclipse of 1868.* [Nov. 19,

'Rangoon' on the morning of the 18th inst. The ship was at that time on the central line, viz. in lat. $15^{\circ} 42'$ N., long. $59^{\circ} 15'$ E.

"The total eclipse lasted 4' 8". The sketch shows what was seen by a large number of persons. In observing with the spectroscope, I saw what none of the others could see with their glasses, viz. two prominences on the right limb of the moon (showing in the spectroscope to the left) of a yellow flame-colour, immediately opposite to the red ones, the whole forming a square, with the moon in the centre, showing out like a mass of rock. The colour of the corona, as seen through the prism, was red, a yellowish green, blue, and violet,—the violet the brightest till the middle of the eclipse, when the red became lumpy and showed brighter.

"The spectrum from the moon cut through the centre of this, but very faint, the red thrown out with a curve.

"The motion of the ship was so great it was impossible to get minute observations; so much haze and flying cloud, only Venus and one other star could be seen.

"I return the spectroscope, and am only sorry I could not make more use of it.

"I am, &c.,

(Signed)

"D. RENNOLDSON,

"Commander S. S. 'Rangoon.'"

Capt. Henry, Superintendent

P. & O. S. N. C., Bombay.

[This letter was accompanied by four coloured sketches of the prominences and corona. Of these No. 1 shows a small low prominence extending from about azimuth 144° to 150° , azimuths being measured in the direction of the motion of the hands of a watch, round the centre of the moon's disk, from the highest point, and another low prominence from azimuth 160° to 180° . No. 2 shows a lofty prominence at azimuth 198° , curved in the upper part, with the concavity turned in the direction of increasing azimuth, and a low prominence from azimuth 332° to 345° . No. 3 shows the long prominence at azimuth 202° , and the upper prominence at azimuth 320° to 338° . No. 4 shows the long prominence, reduced in height, at azimuth 212° , and the upper prominence at azimuth 230° to 255° . The figures are thus described.]

No. 1. A small red flame or protuberance on the right-hand lower corner of the moon, visible for a few seconds before the sun was totally eclipsed; disappeared a few seconds after.

No. 2. $1\frac{1}{2}^m$ after commencement of total eclipse. A large red flame of about 5' of arc on lower left-hand corner, and a red flame or blotch on upper left hand—both visible from commencement of totality, and very bright.

No. 3. 3^m after commencement. The long red flame rather shorter, and the upper one increased in size.

No. 4. At reappearance of sun's upper limb the upper protuberance disappeared; the lower one was visible for about 10^s after, about half its former size.

1868.] Capt. H. Welchman King *on the Solar Eclipse of 1868.* 127

XIII. "Observations of the Total Solar Eclipse of August 18, 1868."

By Captain SOMERVILLE MURRAY. Communicated by Prof. STOKES. Received October 30, 1868.

(Abstract.)

In accordance with the instructions he had received from the Managing Directors of the Peninsular and Oriental Steam Navigation Company, Captain Murray made all observations that were possible of the eclipse of the 18th August; but the high northern latitude of the ship's ('Ellora') position at the time precluded the possibility of observing any remarkable phenomenon, the obscuration of the sun being comparatively slight.

XIV. "Observations of the Total Solar Eclipse of August 18, 1868."

By Captain HENRY WELCHMAN KING. Communicated by Prof. STOKES. Received October 30, 1868.

(Abstract.)

The weather was cloudy throughout, but the clouds were thin, so much so as to allow two or three stars to be seen during the time of totality.

The corona exhibited itself quite suddenly on the instant of first totality. It presented the appearance of a golden-yellow brightness of no very intense brilliancy. It disappeared as suddenly as it appeared, on the first sign of the retiring sun. The flames or prominences became visible simultaneously with the corona.

The paper was accompanied by four coloured sketches, the first representing the positions of the sun and moon, with the spots on the former, at an early stage of the eclipse, as observed with a 5-foot telescope by Ross of three inches aperture; the remaining three representing different stages of the totality. The second figure shows a red prominence about 25° to the left or east of the lowest point, with a smaller green prominence, also in contact with the moon, a little distance to the east of it.

The third shows a red prominence about 30° to the right of the lowest point. The fourth figure shows a broad prominence a little to the left of the highest point. The figures 2-4 are thus described:—

Fig. 2. "First instant of totality. This flame or prominence was visible during the whole period of totality by ordinary glasses. The prismatic colours to the eastward of flame I did not see myself, and cannot vouch for them."

Fig. 3. "Middle of totality. This flame or prominence visible during the whole period of eclipse to ordinary glasses."

Fig. 4. "First reappearance of sun. I did not observe this flame in early stages of totality, though it may have been visible. It was observed by the above-mentioned Ross, and was not so brilliant as the others, though more extended. Entire power of the totality extended over 2 minutes 48 seconds."

The observations were made on board the steamer 'Rangoon,' approximate latitude $16^{\circ} 44' N.$, longitude $83^{\circ} 55' E.$

XV. "Supplementary Note on a Spectrum of a Solar Prominence."
By J. NORMAN LOCKYER, F.R.A.S., in a Letter to the Secretary.
Communicated by Dr. SHARPEY, Sec. R.S. Received November
5, 1868.

SIR,—I have the honour, in continuation of my letter of the 20th ultimo, to inform you that I have this morning obtained evidence that the solar prominences are merely the expansion, in certain regions, of an envelope which surrounds the sun on all sides. I may add that other facts observed seem to point out that we may shortly be in a position to determine the temperature of these circumsolar regions.

J. NORMAN LOCKYER.

XVI. "Spectroscopic Observations of the Sun."—No. II. By J. NORMAN LOCKYER, F.R.A.S. Communicated by Dr. SHARPEY, Sec. R.S. Received November 19, 1868.

The reading of this Paper was commenced.

November 26, 1868.

Lieut.-General SABINE, President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President.—Lieut.-General Edward Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., LL.D.

Secretaries.— { William Sharpey, M.D., LL.D.
 { George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Prof. William Hallows Miller, M.A., LL.D.

Other Members of the Council.—Frederick Augustus Abel, Esq.; Sir Benjamin Collins Brodie, Bart., M.A.; William Benjamin Carpenter, M.D.; J. Lockhart Clarke, Esq.; Frederick Currey, Esq., M.A.; Warren De La Rue, Esq., Ph.D.; Sir William Fergusson, Bart.; William Henry Flower, Esq.; Capt. Douglas Galton, C.B.; John Peter Gassiot, Esq.; John Hawkshaw, Esq.; John Marshall, Esq.; Joseph Prestwich, Esq.; George Henry Richards, Capt. R.N.; Archibald Smith, Esq., M.A.; Lieut.-Col. Alexander Strange.

Lieut.-Col. Cameron, Mr. Crofton, Mr. Griess, and the Rev. Dr. Tristram were admitted into the Society.

The following communications were read:—

I. "Account of Explorations by the Swedish Arctic Expedition at the close of the Season 1868, in a Letter to the President." By Professor A. NORDENSKIÖLD. Communicated by the President. Received November 20, 1868. .

Tromsö, October 23, 1868.

SIR,—The second geographical part of our expedition anchored a few days ago in the harbour of Tromsö, after a difficult and adventurous autumn cruise of a month in the polar basin north of 80° lat.; and as these regions were never before visited in such a late season, I hope that our observations will be of interest for the arctic men of Great Britain, as contributing to settle some points of the polar question recently much debated.

According to the plan adopted for the Swedish Expedition, five of its naturalists returned, in the middle of September, to Tromsö with one of the small ships that brought coal to our depot at Amsterdam Island, and the same day the 'Sofia,' with the remaining part of the expedition (consisting of v. Otter, Berggren, Nyström, Palander, Lemström, and myself), steamed northward for Seven Islands, where it was our intention to wait for a favourable occasion to go further. But finding these islands so surrounded by ice that no anchorage was accessible, we were compelled to abandon this plan and go directly northward, following a tolerably large opening in the pack. After a cruise of some days among the ice we, on the 19th of September, at $17\frac{1}{2}^{\circ}$ long. east of Greenwich, reached $81^{\circ} 42'$ N. Lat.; but, as may be seen by the adjoined photograph, the ice further northward was so closed that it was impossible even for a boat to advance. We turned westward, in vain looking for another practicable opening. Following the border of the pack, we were, on the 24th September, at a longitude of 2° W. already south of 79° lat., after often having passed fields of drift-ice covered with particles of earth, which seems to indicate that land is to be met with further northward. Despairing of finding the ice westward more favourable, and anxious to make a new survey later in the autumn of the position of the ice-field between 0° and 20° long., we returned to our coal-depot.

North of $80^{\circ} 30'$ the season was already far more advanced than one would presume from the observations at Spitzbergen during the first part of September. The temperature of the air being -6° to -8° (Centigrade) below zero, the surface of the sea was, when calm, covered by a layer of new ice more than an inch thick; and after sunset the obscurity, increased by constant intense frost-rime, made the sailing or steaming among the ice both uncertain and dangerous. As the salt water has no maximum of density, the freezing of the surface over a depth of 1000 to 2000 fathoms would be difficult to explain, were it not that the sea-water in the polar regions is by the melting of the ice and the heavy autumnal snowfalls *less salt, and accordingly lighter, even when at a temperature lower than that of the layers beneath.*

The last week of September was employed in filling our coal-boxes and refitting our steamer for a new struggle with the ice. During these days a strong easterly snow-storm prevailed, which made us hope to find the newly-formed ice broken and the pack more dispersed than before. O'

intention was to employ this favourable circumstance for making a last attempt to go northward, and if this should prove to be unsuccessful to winter at Seven Islands. This plan was frustrated by an accident similar to that which happened to the expeditions of Buchan and Ross in 1818.

The calm that during the summer prevails in the Arctic Sea gave way after September 23rd to almost uninterrupted stormy weather, which caused such a violent and irregular sea on the border of the pack that it was impossible to advance without exposing the ship to be instantly crushed by the large rolling hummocks. Consequently we were obliged to lay to under the 81st parallel, waiting for better weather and a calmer sea. However, everywhere on the surface of the sea large pieces of ice were scattered, dangerous by their rolling movement, their hardness (the temperature was $-14^{\circ} 5$ Centigrade), and the obscurity that prevailed at night. During a south-easterly storm on April 24 our steamer was so vehemently thrown against such a hummock that a large leak ensued, which forced us to make as soon as possible for land. After hard work in keeping the steamer afloat, we reached Amsterdam Island, where the leak was provisionally caulked so as to enable us to reach a safer harbour in Kings Bay the following day. Here we had the ship down, and the damage was repaired as well as possible.

October 12 we left this harbour, going through a large field of *new* ice. Evidently the season was too far advanced for further enterprises to the northward; besides, our steamer, having got two ribs broken, was no longer strong enough for a new encounter with the ice; and as a wintering only on Seven Islands could not be of an interest great enough to outweigh the loss of time, privations, and dangers unavoidably associated with it, we resolved to employ the yet tolerably open sea around the southern part of Spitzbergen to make an attempt to reach Giles Land. But being, at Thousand Islands, prevented by ice from penetrating further, we turned southward and reached Tromsø, April 19, after having at Beeren Eiland sustained a severe storm, during which our steamer was quite ice down by the waves that washed over.

During our cruize in the polar basin interesting observations were obtained on the temperature, currents, &c. of the sea, and a number of carefully examined deep soundings were made with an apparatus resembling the 'Bulldog' apparatus of M'Clintock, by the intelligent and intrepid commander of the 'Sofia,' Captain Baron v. Otter, and I hope soon to be able to present you a copy of his map on these subjects, the position of the ice, &c.

As you already know by the letter of Dr. Malmgren, the scientific results of the first part of our expedition have been very satisfactory, and I hope also that its second part will give important information about several arctic questions.

By the expeditions of Tschitschayoff (1765 & 1766), Phipps, Buchan, Franklin, Scoresby, Sabine, Clavering, Parry, Torell, &c., it was already long ago proved that in the summer compact masses of drift-ice prevented vessels from penetrating far into the polar basin. But during the most

favourable season, *i. e.* the time before the formation of new ice, no vessel had as yet made such an attempt. This was the aim of the Swedish Expedition, and it found—

(1) That the polar sea is far more open in the autumn than at any other season of the year, but that even then the passage is soon stopped by dense and impenetrable masses of broken ice.

(2) That during the winter the polar basin is covered by an unbroken ice, and that the freezing of the surface begins as early as the end of September. From September 23 to October 12 we had almost every day, either with the steamer or with boats, to cross new-formed ice.

(3) That an autumn cruise north of $80\frac{1}{2}^{\circ}$ lat. is attended with unusual dangers, owing to the darkness and storms then prevailing, no ships being able a long time to sustain a night storm among large rolling pieces of ice and a cold of -15° Cent. If the ship has the good luck not to be more or less damaged by the constant unavoidable encounters with the ice mounts, it will soon by the immediate freezing of the washing waves be itself quite covered and pressed down by ice.

(4) The idea of an open and comparatively milder polar basin is quite chimerical; on the contrary, $20'$ – $30'$ north of Spitzbergen a region of cold seems to begin which no doubt stretches far around the pole.

(5) The only plan to attain the pole, from which success can be expected, is that adopted by most English arctic men, namely of going northward by sledges in the winter either from Smith Sound or Seven Islands.

I remain, Sir,

Your obedient humble Servant,

A. E. NORDENSKIÖLD.

P.S. As soon as the magnetical observations of Dr. Lemström shall be duly worked out I will send you a copy of them.

Should you think it worth communicating this letter to the Royal Geographical Society, I beg you especially to inform its celebrated President, Sir R. Murchison, that besides other specimens interesting in a geological point of view (for instance, a mass of *Miocene* and *coal* plants, bones of *Ichthyosaurus*? &c.), we found a number of large fish fragments, probably belonging to the Devonian age, in the red slate of Liebbe Bay, constituting the overmost layer of what I in my 'Geology of Spitzbergen' called Hecla block-formation. Accordingly Sir Roderick probably is right in supposing that the deeper layers of this "formation" belong to the Silurian age. The underlying crystalline plates are evidently Laurentian.

II. The reading of Mr. Lockyer's Paper, "Spectroscopic Observation of the Sun, No. II.," was resumed and concluded.

(Abstract.)

THE author, after referring to his ineffectual attempts since 1866 to observe the spectrum of the prominences with an instrument of small dis-

persive powers, gave an account of the delays which had impeded the construction of a larger one (the funds for which were supplied by the Government-Grant Committee early in 1867), in order that the coincidence in time between his results and those obtained by the Indian observers might not be misinterpreted.

Details are given of the observations made by the new instrument, which was received incomplete on the 16th of October. These observations include the discovery, and exact determination of the lines, of the prominence-spectrum on the 20th of October, and of the fact that the prominences are merely local aggregations of a gaseous medium which entirely envelopes the sun. The term *Chromosphere* is suggested for this envelope, in order to distinguish it from the cool absorbing atmosphere on the one hand, and from the white light-giving photosphere on the other. The possibility of variations in the thickness of this envelope is suggested, and the phenomena presented by the star in Corona are referred to.

It is stated that, under proper instrumental and atmospheric conditions, the spectrum of the chromosphere is always visible in every part of the sun's periphery ; its height, and the dimensions and shapes of several prominences, observed at different times, are given in the paper. One prominence, 3' high, was observed on the 20th October.

Two of the lines correspond with Fraunhofer's C and F ; another lies 8° or 9° (of Kirchhoff's scale) from D towards E. There is another bright line, which occasionally makes its appearance near C, but slightly less refrangible than that line. It is remarked that the line near D has no corresponding line ordinarily visible in the solar spectrum. The author has been led by his observations to ascribe great variation of brilliancy to the lines. On the 5th of November a prominence was observed in which the action was evidently very intense ; and on this occasion the light and colour of the line at F were most vivid. This was not observed all along the line visible in the field of view of the instrument, but only at certain parts of the line which appeared to widen out.

The author points out that the line F invariably expands (that the band of light gets wider and wider) as the sun is approached, and that the C line and the D line do not ; and he enlarges upon the importance of this fact, taken in connexion with the researches of Plücker, Hittorf, and Frankland on the spectrum of hydrogen—stating at the same time that he is engaged in researches on gaseous spectra which, it is possible, will enable us to determine the temperature and pressure at the surfaces of the chromosphere, and to give a full explanation of the various colours of the prominences which have been observed at different times.

The paper also refers to certain bright regions in the solar spectrum itself.

Evidence is adduced to show that possibly a chromosphere is, under certain conditions, a regular part of star-economy ; and the outburst of the star in Corona is especially dwelt upon.

III. "Extract from a Letter addressed by CHAS. BABBAGE, Esq., F.R.S., to Dr. BACHE, of Washington, May 10, 1852. Communicated by Mr. BABBAGE. Received November 26, 1868.

"In reading the account of the great solar eclipse of last year (1851) I was much struck by the description of the pink excrescences apparently attached to the sun's disk, and connected with its spots (see Proceedings of Royal Astronomical Society). They are only visible during a few minutes in a total eclipse. It occurred to me that it might be possible to render them visible at other times by two different methods:—

"1st. By placing in the focus of an equatorial telescope moved by clockwork an opaque disk, equal to or a little larger than the sun's image. This would represent a continuous total eclipse; and if every known means of excluding light were adopted, it might be possible to see those faint pink objects, which are probably clouds raised by the eruption of solar volcanoes.

"2nd. If this fail, it might yet be possible to render them visible by taking daguerreotype or photographic images.

"It is really surprising that nobody has yet taken such images regularly, for the sake of recording the solar spots and their changes.

"I have no clock-moving equatorial myself fit for these observations, nor have I time to spare for them.

"I cannot persuade my countrymen that they are important, so you are at liberty to try them, or publish the plan on your side of the Atlantic.

"Mr. Gould will probably have explained to you an old plan of mine for mapping zones of stars without moving the eye from the telescope."

November 30, 1868.

ANNIVERSARY MEETING.

Lieut.-General SABINE, President, in the Chair.

Mr. Newmarch, on the part of the Auditors of the Treasurer's Accounts appointed by the Society, reported that the total receipts during the past year, including a balance of £495 10s. 3d. carried from the preceding year, amount to £4780 5s. 11d.; and that the total expenditure in the same period amounts to £4286 11s. 5d., leaving a balance of £479 16s. 1d. at the Bankers', and of £13 18s. 5d. in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

Royal.

His Imperial and Royal Highness the Archduke Louis of Austria (1864).

On the Home List.

Charles Dickson Archibald, Esq.
 Charles James Beverly, Esq.
 Capt. Benjamin Blake*.
 Rev. Miles Bland, D.D.
 Sir David Brewster, K.H., LL.D.,
 D.C.L.
 Henry, Lord Brougham and Vaux,
 M.A.
 Rev. Jonathan Cape.
 Robert John, Lord Carington.
 Antoine François Jean Claudet, Esq.
 The Right Hon. Sir George Clerk,
 Bart., D.C.L.
 John Crawford, Esq.
 Charles Giles Bridle Daubeney, M.D.,
 LL.D.
 John Davy, M.D.
 Rev. William Rutter Dawes.
 George Douglas, Esq. (1853).

Sir William Francis Elliott, Bart.
 (1864).
 John Elliotson, M.D.
 The Right Hon. Sir Edmund Walker
 Head, Bart.
 William Bird Herapath, M.D.
 Sir Charles Lemon Bart.
 Sir John Liddell, K.C.B., M.D.
 John Carnac Morris, Esq.*
 Rev. Henry Noel-Fearn, M.A.,
 D.C.L.
 Robert Porrett, Esq.
 Archibald John, Earl of Rosebery,
 K.T., M.A., LL.D.
 The Ven. Archdeacon Tattam, D.D.
 Thomas Pridgin Teale, Esq.
 Nathaniel Bagshaw Ward, Esq.
 Alexander Luard Wollaston, M.B*.

On the Foreign List.

Marie Jean Pierre Flourens.
 Jean Bernard Léon Foucault.

Julius Plücker.
 Jean Victor Poncelet.

Withdrawn.

Rear-Admiral Thomas Edward Lawes Moore.

Defaulters.

Sir John Macneill.
 Edward Solly, Esq.

The Right Hon. Charles Pelham
 Villiers.

Fellows elected since the last Anniversary.

John Ball, Esq., M.A.
 Henry Charlton Bastian, M.D.
 Lieut.-Col. John Cameron, R.E.
 Prof. Robert Bellamy Clifton, M.A.
 Morgan William Crofton, Esq., B.A.
 Joseph Barnard Davis, M.D.
 Peter Martin Duncan, M.B.
 John Peter Griess, Esq.

Augustus G. Vernon Harcourt, Esq.
 Rear Ad. Astley Cooper Key, C.B.
 Rear-Admiral E. Ommanney, C.B.
 James Bell Pettigrew, M.D.
 Laurence Parsons, Earl of Rosse.
 Edward James Stone, Esq., M.A.
 Rev. Henry Baker Tristram, M.A.
 Wm. S. Wright Vaux, Esq., M.A.

On the Foreign List.

Theodor Ludwig Wilhelm Bischoff.
 Rudolph Julius Emmanuel Clausius.

Hugo von Mohl.
 Samuel Heinrich Schwabe.

Readmitted.

Colonel John Le Couteur.

* Date of decease unknown.

The President then addressed the Society as follows :—

GENTLEMEN,

I HAVE the satisfaction of now laying before you the second volume of the Catalogue of Scientific Papers. The volume now completed carries on the list of titles in alphabetical order as far as G R A, inclusive. The Library Committee, under whose superintendence the Catalogue is published, had hoped that the printing of the work would have made greater progress than it has done during the time that has elapsed since the appearance of the first volume ; but notwithstanding their earnest endeavours to attain that object, they found that, with due regard to the careful revision of the press, the rate of printing could not be materially accelerated.

In fulfilment of the understanding with Her Majesty's Government, explained in my Address last year, copies have been presented to various Scientific Institutions and individuals, according to a list drawn up by the Council, and approved by the Treasury. It is gratifying to know that in the numerous letters of acknowledgment received in return, as well as more publicly through the press, the value of the work as an aid to scientific research has been warmly recognized. As a special instance of this favourable expression of opinion, I would refer to the ample notice of the book written by our Foreign Member, Hofrath W. Ritter v. Haidinger, of Vienna, and circulated by him in different parts of Europe.

Already of the remaining copies 120 have been sold.

My last year's Address contained an account of the proceedings of the Committee of the Royal Society, which, at the request of Her Majesty's Government, had undertaken the reorganization and superintendence of the meteorological department of the Board of Trade. The year that has since elapsed has been employed, 1°, In perfecting the instrumental arrangements, and the systematic working of the staff, at the seven British Observatories which have been supplied, under the Committee's direction, with continuously self-recording meteorological apparatus. For this purpose one or more of the staff of each Observatory has passed some days at the Central Observatory at Kew ; and the Observatories themselves have been visited, some by Mr. Scott, the Director of the Meteorological Office in London, and all by Mr. Stewart, the Superintendent of the Central Observatory, and also by Mr. Beckley, the Engineer of the Kew Establishment. By these means it is hoped that uniformity of action on thoroughly well considered principles has been secured, and a considerable advance made towards the systematic record of the meteorological phenomena over the British Islands. The monthly records are now beginning to be received at the Office in London with regularity from all the Observatories, but have scarcely yet quite attained in all instances to that uniform accuracy which it is hoped will be fully secured at the close of the present

year. The means and the methods by which the facts thus considerably and systematically obtained may be communicated to the public, in the form which may be at once suitable for the study of the weather phenomena over the very limited territorial area of the British Islands—and may at the same time contribute in the most satisfactory manner to the important investigations which are now in progress on the Continent of Europe regarding the periodic and non-periodic variations—will be the next point to which the careful attention of the Committee will be directed.

2°. In the branch of ocean meteorology the cooperation of several of our leading oceanic steam companies has been secured; and a large number of the commanders of their vessels are now actively engaged in the work of observing. Instruments have also been supplied to other masters of vessels of our mercantile marine, care being always taken that the recipients are both competent to observe and willing to do so regularly and accurately. The zeal and judgment displayed by Captain Henry Toynbee, the Marine Superintendent of the Office, in the selection of observers, has already begun to bear fruit in the marked improvement in the quality of the information in the registers which are now received compared with those which had previously accumulated in the office. The discussion of the material which has been thus collected and is still collecting is in progress; but some time must elapse before a significant portion of the immense arrear can be advanced to such a stage as to afford a prospect of its speedy publication. The staff of clerks is already fully occupied; so that the rate of progress cannot be much accelerated, unless the Committee find themselves in a position to devote more funds to this object than they are at present able to do. The special subject to which the attention of this department of the office has been first directed, is the discussion of information respecting the district of the Atlantic Ocean comprised between the parallels of 20° N. and 10° S., for which region it is in contemplation to ascertain the conditions of atmospheric pressure, temperature, and vapour tension, as well as the direction and force of wind, the character of the weather, and the surface temperature of the sea. These elements will be discussed for spaces of a single square degree in area for the different months.

As regards the temperature of the surface of the sea (a subject so much dwelt on by the President and Council of the Royal Society in their letter to the Board of Trade of February 22, 1855), a very valuable series of monthly charts has been published by the Royal Meteorological Institute of the Netherlands, exhibiting the temperature for each degree of latitude for the North and South Atlantic Oceans, and for the Indian Ocean. The Committee considered that a conversion of the data in these charts into British measures would be likely to be of immediate use to our own marine, and they have accordingly directed that a set of charts should be prepared in the first instance for the South Atlantic Ocean, exhibiting the Dutch results, as well as those obtained from the British registers received by the meteo-

rological department of the Board of Trade under its former management. These latter, however, were only calculated for spaces of five degrees square. In addition, some of the work left in an unfinished state by Admiral Fitz Roy has been undertaken by the office at extra hours, and a series of wind-tables for the Atlantic have been ordered to be printed. The discussion of general meteorological information for the Pacific seaboard of South America is in a state far advanced towards completion.

3°. The system of telegraphic weather-intelligence, described in my last year's anniversary address, has received a further development, and at present the Drum signal is hoisted at 97 British stations, to convey the intelligence of the existence of atmospherical disturbance in each case to such ports as may appear to the central office to be reasonably liable to be affected by it. Similar intelligence has been telegraphed to Hamburg since February 1868; and in the course of last month Herr von Freedon, the Director of the newly established meteorological office in that city (the Nord-deutsche Seewarte), has informed the London office that the harbour authorities on the Elbe have resolved to hoist the Drum signal at Hamburg and Cuxhaven whenever intelligence implying probable danger shall be received from London. In France also the ministry of the marine has adopted, for the present at least, the practice of telegraphing facts and not prophecies.

In addition to the telegraphic communications already referred to, the London Office sends, by special request, telegraphic intelligence of the existence of a certain amount of difference of barometric pressure between two stations within a defined area, to Mr. Rundell (Secretary of the Underwriters' Association at Liverpool), and to the Dutch authorities. The influence which the distribution of atmospheric pressure exerts on the motion of the air has been much dwelt upon by Dr. Buys Ballot, of Utrecht, and a rule has been propounded by him for inferring the coming direction of the wind from simultaneous readings of the barometer at different places. In order to lay the foundation of a systematic study of our weather, and, at the same time, to test the truth of this rule, it has been the practice of our meteorological office, for more than a year past, to prepare, and subject to systematic discussion, daily charts of the meteorological condition over the area embraced by the daily telegraphic reports which it receives, viz. the British Islands and a portion of the nearer continental coasts. The results of this investigation are on the whole encouraging, and favour the hope that with a more extended experience a real, if slight, advance will have been made in this most intricate but interesting inquiry.

The magnificent but rare phenomenon of a total solar eclipse is not more striking as a spectacle than interesting in a scientific point of view, from the precious opportunity it affords of gathering information, then only to be obtained, which bears on the constitution of our great luminary. The corona which surrounds the dark body of the moon must have

been seen from the earliest times; but what does it import? Has it its seat in our own atmosphere, or in an atmosphere of the moon, or in something surrounding the sun? and, in the latter case, is it self-luminous, or does it shine by reflected light? What, again, is the nature of those singular rose-coloured luminous objects seen just outside the dark disk of the moon, which were first brought prominently into notice by the observers who watched the eclipse of July 7, 1842, and have subsequently been seen on the occasion of total solar eclipses?

Evidence bearing in an important manner on the true answers to these questions had already been obtained on the occasion of former total eclipses. In that of July 18, 1860, M. Prazmowski ascertained that the light of the corona was strongly polarized in a plane passing through the centre of the sun, while that of the prominences was unpolarized. The fact of the polarization discarded the hypothesis, sufficiently improbable on other grounds, that the corona belongs either to our own atmosphere or to a lunar atmosphere (since in that case the light would be reflected or scattered at an almost grazing incidence), and proved it to belong to the sun, and to shine mainly, if not wholly, by reflected light. The absence of polarization in the light of the prominences proved that they are very probably self-luminous. The elaborate photographic observations of Mr. Warren De La Rue on the same eclipse proved, by the motion of the prominences relatively to the moon, that they belong to the sun, and showed that their light is remarkable for its actinic power.

In the interval between this eclipse and that of the present year, a new method of research had sprung up, in the application of the spectroscope to the celestial bodies, and already, in the hands of Mr. Huggins, had revealed in many of the nebulae a constitution hitherto unsuspected. It was important to apply this method of research to the red prominences. Should they give a continuous spectrum, the conclusion would be that the matter of which they consist is probably in a solid or liquid condition, such as clouds formed by precipitation; should the spectrum be one of bright lines, we must conclude that they are glowing gas.

To solve this important problem, independently of what might be done by other scientific bodies or by individuals, the Royal Society procured an equatorially mounted telescope, furnished with a spectroscope and clock-movement. With the sanction of Colonel Walker, R.E., Director of the Great Trigonometrical Survey of India, this instrument was entrusted to Lieut. John Herschel, R.E., who is attached to the Survey, and who, being at the time in England, had the advantage of instruction from so skilful an observer as Mr. Huggins before his return to India. After his return to India, Lieut. Herschel worked diligently at the spectra of the southern nebulae, thereby at the same time making an important addition to our knowledge, and practising for the approaching eclipse. Four direct-vision hand-spectroscopes, intended for distribution to observers at different stations, were also sent out,—partly that the occasion might not be

wholly lost in case clouds should prevent observations from being taken at the principal station; partly because a more rough and general view of the whole phenomenon might reveal features which would be missed in a more careful scrutiny of a particular part. Another telescope, furnished with analyzers for the examination of polarization, was also sent out; for from the shortness of the time at the disposal of an observer, it would be satisfactory that the results obtained, even by so skilful an observer as M. Prazmowski, should be confirmed.

The observations of the observers entrusted with these instruments were greatly impeded by flying clouds, notwithstanding which, however, important work was done. With the principal instrument, Lieut. Herschel ascertained that the spectrum of the prominences showed three isolated bright lines—red, orange, and blue. He had time to take a good measure of the position of the orange line, which proved to be coincident with D, as nearly as the instrument could measure. Clouds prevented the measure of the blue line from being equally good; it proved, however, to be nearly coincident with F, apparently a very little less refrangible. With one of the hand-spectroscopes Captain Haig, R.E., observed the spectrum of the red prominences to consist of two bands, “rose-madder” and “golden yellow,” corresponding, doubtless, to the “red” and “orange” of Herschel. But besides these, *just before* the emergence of the sun, Capt. Haig observed, “in the spectrum of the moon’s edge,” two well-defined bright bands, one green and one indigo. The seizing of this almost momentary phenomenon, establishing as it does the existence of a thin envelope of glowing gas (unless, indeed, the constitution thus revealed were merely local, and its occurrence just at the part of the sun first measured were a mere matter of chance), proves the advantage of not neglecting the use of a comparatively rough instrument intended for a general scrutiny of the phenomenon.

Of the remaining hand-spectroscopes, one was entrusted to Mr. Chambers, Director of the Bombay Observatory, but could not be used on account of clouds, and two were placed in the hands of the commanders of homeward-bound steamers, belonging to the Peninsular and Oriental Steam Navigation Company. Capt. Charles G. Perrins, of the ‘Carnatic,’ who had charge of one, was unable to apply it to the intended observations, as his ship was about 20 miles north of the track of the total phase; with the other, Capt. Rennoldson, of the ‘Rangoon,’ ascertained the discontinuous character of the red prominences, and his observation would have been very valuable had clouds prevented observations from being taken on shore.

The telescope furnished with analyzers was placed in the hands of Lieut. Campbell, R.E., who has fully confirmed the previous observation of M. Prazmowski relative to the strong polarization of the light of the corona.

A feature of the prominences, which is specially noticed in Capt. Haig’s

account, resting on the observations of Capt. Tanner and Mr. Kero Laxuman, who were of his party, is their streaked character. This had been noticed before, in the eclipse of 1860. Mr. Warren De La Rue, in speaking of the prominences, expressly mentions their structure; and M. Chacornac, who devoted himself to this object, has given a long description of their appearance*, which, however, is a little difficult to follow for want of a figure. The strong actinic power, the streaked character, and the bright-line spectrum of the prominences seem certainly to accord very well with the hypothesis in which they are regarded as gigantic auroræ—a view, however, which may be rendered less probable by the apparently general prevalence over the sun's surface of a lower stratum of similar nature, of which the prominences are merely elevated portions.

The great Melbourne Telescope was despatched to its destination in an Australian packet ('The Empress of the Seas'), which sailed from Liverpool on the 18th of July last; and M. Le Sueur proceeded overland to await its arrival. The micrometer and spectroscope which are to follow are quite ready, and the photographic apparatus is also nearly ready, to be despatched to Melbourne.

In June last the President and Council received from Dr. Carpenter and Professor Wyville Thomson letters strongly recommending that the Zoology of the *Deep Sea*, especially in the North Atlantic Ocean, should be more thoroughly and systematically examined than has hitherto been accomplished, and requesting the intervention of the Royal Society with the Admiralty for the purpose of obtaining the services of a vessel, with proper means and appliances for deep-sea sounding and dredging, to carry on a systematic research, in the seas immediately north of our own island, for a month or six weeks in the approaching autumn—and tendering their own services to accompany the vessel.

With the thoroughly efficient aid of the Hydrographer, Capt. Richards, R.N., the 'Lightning,' surveying-ship, Staff-Commander May, was selected and equipped expressly for this service; and Dr. Carpenter and Professor Thomson embarked in her on the 10th of August, at Stornoway. After examining the seas between Scotland and the Færoe Islands, the 'Lightning' returned on the 9th of September to Stornoway, to land Professor Thomson (whose presence was required elsewhere), and sailed again (this time accompanied by Dr. Carpenter only) for a second, more westerly cruise, which lasted until the 26th of September.

A preliminary report of the results has been received from Dr. Carpenter, and will be read to the Society at an early evening meeting in the present session; I will only venture to anticipate the contents of this very valuable report so far as to say that it will be found of very high interest both in respect to the temperature of the sea at great

* Le Verrier's 'Bulletin' for Sept. 4-8, 1860.

depths, and to the nature of the sea-bottom, and the life existing in its vicinity.

The report strongly recommends the continuation and extension of these researches—a recommendation which in due time will require and receive the attention of your Council, who may confidently anticipate that should a further application to the Admiralty be deemed desirable it will receive favourable consideration, and, if approved, will be secure of the same cordial and invaluable cooperation on the part of the Hydrographer as that which has been enjoyed on this occasion.

We have to rejoice in the safe return of the Swedish and North-German Expeditions, engaged in the past summer in the endeavour to extend the domain of Arctic Exploration to the north and to the west. Though the limits previously attained have not been passed in either direction, much valuable information has been obtained regarding the Natural History of Northern Lands, as well as many important facts bearing on the Hydrography of the Arctic Seas; while an experience has been gained in Arctic navigation, and habits acquired of surmounting the difficulties which it presents, that may yield good fruit hereafter.

The Arctic explorations of the Swedes included, from their commencement, the design of accomplishing such a preliminary survey of Spitzbergen as might solve the question of the practicability of the measurement of a degree of the meridian in that high latitude. The idea of such an undertaking having originated in this country and in this Society more than forty years ago, it is natural that we should regard the steps taken towards its accomplishment with a lively sympathy. A sketch of what was effected in 1861 and 1864 by MM. Chydenius, Düner, and Nordenskiöld, communicated to the Royal Society by Captain Skogman, of the Royal Swedish Navy, was printed in the Proceedings of December 1864. An official and elaborate Report has since been published (in Sept. 1866) by the Royal Swedish Academy, entitled “Förberedande Undersökningar rörande Utförbarheten af en Gradmätning på Spetsbergen” (preliminary researches touching the facilities for a measurement of a degree at Spitzbergen), by MM. Düner and Nordenskiöld (Chydenius having unfortunately died). In the Map accompanying the Report the triangles are laid down which connect the extremes of land, and comprehend an arc of about $4^{\circ} 11'$. One of the objects contemplated by the expedition which has just returned was, to examine the possibility of the extension of the arc to lands existing to the north of the north-easternmost part of Spitzbergen—a question, however, which cannot be regarded as yet perfectly solved, the northern progress of the ‘Sophia’ having been stopped by ice, which is described by M. Nordenskiöld as “consisting in part of fields of drift-ice, covered with particles of earth, which seems to indicate that land is to be met with further north.”

Should these preliminary researches and surveys eventuate in a Scandi-

navian arc-measurement at Spitzbergen, I need scarcely say with what interest such an undertaking would be regarded by this country and by its Royal Society.

With reference to the operations of the Committee, appointed at the Nottingham Meeting of the British Association, for the Exploration of the Tertiary Plant-beds of North Greenland, it was stated in my last Address that a large collection of fossil plant-remains had been brought from Greenland by Mr. Edward Whymper.

The entire collection has been sent, for examination and description, to Prof. Oswald Heer, of Zurich, who has already published a work, '*Flora Fossilis Arctica*,' containing the results of his examination of the fossils brought at various times from Greenland and other parts of the arctic regions and deposited in the museums of this country and of Denmark and Sweden.

The Committee, finding that their funds were exhausted, made a fresh application to the Government-Grant Committee, and received an additional sum to defray the expense of carriage of the specimens to and from Zurich.

The collection was forwarded to Switzerland at the end of last year; and within the last week Prof. Heer has sent the description of the fossils to London, with the view of submitting it to the Royal Society.

The localities which were examined by Mr. Whymper were situated on the shores of the Waigat, at two points on Disco Island, and at Atanekerdruk, on the mainland of Greenland.

From Disco, whence specimens had only once been obtained before (by Dr. Lyall), 14 species were procured. Among them the occurrence of two cones of *Magnolia* present the greatest interest, as they prove to us that an evergreen, such as *Magnolia*, could ripen its fruit at the high north latitude of 70°.

The collection from Atanekerdruk is especially rich, but this locality was well known before; the number of species from it in this collection is 73. Among the most important of these are the flowers and fruit of a *Chestnut*, proving to us that the deposits which contain them must have been formed at different seasons, corresponding to the times of flowering and fruit of the Chestnut.

The collection is not rich in animal remains; however, some insects have been noticed, as well as a freshwater bivalve, probably "*Cyclas*."

The results of this expedition have been eminently satisfactory, whether we look to the number of new species discovered, or to the additional facts, confirmatory of previous determinations, which have been ascertained. This latter remark is of special importance when we find that the identification of a tree by means of its leaves has been supported by the subsequent discovery of its flowers and fruit.

The number of fossil species of vegetable remains discovered in Green-

land has increased to 137, of which 46, or exactly one-third, belong to it in common with the Miocene deposits of Europe.

Four of these are found in our own Bovey Tracey beds, which have been already described by Prof. Heer in the 'Philosophical Transactions.' Among these is *Sequoia Couttsiae*, the commonest tree in the British locality. Accordingly the age of the Greenland deposits has been fixed beyond a doubt as Lower Miocene.

The collection itself is expected to arrive in London shortly, when a complete series of the specimens will be deposited in the British Museum, in accordance with the terms prescribed by the British Association and the Government-Grant Committee of the Royal Society.

The redaction of the great scientific work, the Magnetic Survey of the South Polar Regions—commenced in 1839, under the auspices and at the expense of Her Majesty's Government—has been completed in the present year by the presentation to the Royal Society, and the publication in the Philosophical Transactions, of Maps of the three Magnetic Elements in Southern Parallels, commencing in 30° south, and extending far beyond the limits of ordinary navigation. These Maps are accompanied by Tables containing the numerical coefficients to be employed in a revision of 'Gauss's General Theory,' at the intersection of every fifth degree of latitude and every tenth degree of longitude, between 30° south latitude and the south terrestrial pole. The magnetical determinations of the Survey correspond to the epoch $1842\frac{1}{2}$. Similar Maps for the corresponding latitudes of the Northern Hemisphere, from 30° north latitude to the north terrestrial pole, are in preparation, founded on a coordination of results obtained by magneticians of all countries in the fifteen years preceding and the fifteen years following the same mean epoch of $1842\frac{1}{2}$, and reduced to it. It is hoped that these Maps, with an accompanying Memoir, will be presented to the Royal Society before the close of the present session. There will then remain for subsequent completion the filling up (still for the same epoch) of the space between the parallels of 30° north and 30° south latitude, for which much preparation has been made in the assemblage of materials, requiring only, for their coordination, the allotment of the time needed for the due examination and treatment of so large a body of materials. Should I be so happy as to be able to complete this task also, (my occupation in Terrestrial Magnetism has now extended, more or less, over half a century,) I venture to express a hope that the great work of which the foundation will thus have been laid, viz. "the Revision of the Gaussian Theory, corresponding to a definite epoch in the great cycle of terrestrial magnetism," may, when a suitable time shall appear to have arrived, be taken up and completed under the auspices of the Royal Society.

Whilst on the subject of Terrestrial Magnetism, I may remark that, in a recent number of his 'Wochenberichte,' Dr. Lamont has called the

attention of magneticians to the probable occurrence of the epoch of maximum of the magnetic disturbances at the end of the present year 1868, in accordance with the hypothesis of a decennial period, and has noticed the already great increase in the number of days of unusual magnetic disturbance observed at Munich in the months of August, September, and October last. Coincidentally with Dr. Lamont's experience in this respect, the continuous records of the magnetometers at Kew have shown larger and more frequent magnetic disturbances than usual; and the Photoheliographs, taken there on all days when the sun is visible, have shown larger and more numerous groups of sun-spots.

It may be worthy of remark in this connexion, that 1868 is the fourth decennium since the occurrence of the first well-ascertained maximum of magnetic disturbance; I mean that which, resting on the authority of Arago's admirable and systematic series of observations (1821-1830), has been shown to have taken place in 1828*. It may be proper, however, to await the more decisive evidence which the years 1873 to 1879 may afford, as to the preference to be given to either of the periods assigned by different magneticians (respectively 10 and $11\frac{1}{4}$ years) as the duration of this remarkable phenomenon, which appears to attest the simultaneity of physical affections of the sun and of the earth. If the *decennial* hypothesis be correct, 1873 will be the year of minimum, and 1878 that of maximum; if, on the other hand, the period be one of 11 years and a small fraction, 1873 should be the year of maximum, and 1878 the year of minimum; and the order of progression and sequence be reversed.

I mentioned in my last year's Address that the operations of the Bombay Observatory were delayed by the non-reception of the necessary self-recording magnetical and meteorological instruments of the best modern construction. I am glad to be able to state on this occasion that a communication which I ventured to make to Sir Stafford Northcote, Secretary of State for India, had the immediate effect of removing the difficulty which had intervened, and that advice has recently been received of the safe arrival of these instruments at Bombay. We may now confidently anticipate that, under the able and zealous superintendence of Mr. Chambers, the Bombay Observatory will speedily take a place in the first rank of institutions specially devoted to these two branches of Physical Science.

A paper of considerable interest and importance, entitled "Scientific Exploration of Central Australia," was presented to the Society in April last. The geographical and scientific researches of its author, Dr. Neumayer, published under the authority of the Victorian Government, attest his competency to discuss a subject of this magnitude in its various points of view. The paper itself is an able and interesting one; it contains the

* Arago, Meteor. Essays, English Translation. Longman, 1855. Editor's Note, pp. 355-357.

outline of a large and apparently well-considered scheme, with estimates and other essential details ; it contemplates an expedition to last three or four years, starting from the eastern shores of Queensland, and terminating in an exploration of the western portion of the Australian continent ; and he offers his own services for the conduct of such an undertaking. Should the plan find favour with the different Australian Colonies who would bear its expense and reap the chief material advantage of its results, there can be no doubt of its producing a rich harvest in physical geography and natural history, and as little doubt of the warm interest it would command in the Scientific Societies of the mother country, especially in the Royal Society, and of the pleasure with which they would give to it every assistance in their power.

I proceed to the award of the Medals.

The Copley Medal has been awarded to Sir Charles Wheatstone, F.R.S., for his researches in Acoustics, Optics, Electricity, and Magnetism.

The researches of Sir Charles Wheatstone in acoustics, optics, electricity, and magnetism, numerous and important as they are, have already taken their place as integral parts of science, and have become so completely incorporated into its teaching that it will be hardly necessary on the present occasion to do more than enumerate the leading ones, in recognition of which the Copley Medal has this year been awarded.

The earliest of these researches in point of time were those connected with acoustics ; and among these we may mention a paper on the transmission of sound through solid conductors, which (in 1828) describes the means discovered by the author of transmitting musical performances to distant places, where they are made audible by sounding-boards through the intervention of wires or wooden rods.

His paper on the acoustic figures of vibrating surfaces was published in the ' Philosophical Transactions ' for 1832. In this the laws of the formation of the varied and beautiful figures discovered by Chladni were first traced.

His subsequent invention of the Kaleidophone furnished him with an elegant means of showing optically the coexistence of different forms of vibrations in sounding bodies.

His wave-machine furnished a still more complete method of demonstrating the composition of undulations by mechanical means.

In optics his contrivance of the Stereoscope and the Pseudoscope, and his discussion of the modes in which binocular vision is effected, described in the ' Philosophical Transactions ' for 1838 and 1852, were even more ingenious and important, as showing us how we obtain a perception of solidity or relief, or of its reverse, by the simultaneous observation of two plane images.

Another ingenious optical invention was the Polar Clock, described to the British Association at their Meeting in 1849. This is an instrument

which indicates the time by means of the changes of polarization of the blue light of the sky in the direction of the pole, founded on the discoveries of Arago and Quetelet.

In 1835 he communicated to the British Association a paper on the Prismatic Analysis of the Electric Light, proving that the electric spark from different metals presents for each a different spectrum, exhibiting a definite series of lines, differing in position and colour from each other, and thus enabling very small fragments of one metal to be distinguished with certainty from all the others. This was a starting-point in a new and fertile field of physical inquiry which has abundantly rewarded the labours of subsequent investigators.

But no series of his researches have shown more originality and ingenuity than those by which he succeeded in measuring the velocity of the electric current and the duration of the spark. The principle of the rotating mirror employed in these experiments, and by which he was enabled to measure time to the millionth part of a second, admits of application in ways so varied and important that it may be regarded as having placed a new instrument of research in the hands of those employed in delicate physical inquiries of this order.

Scarcely less valuable are the instruments and processes which Sir Ch. Wheatstone devised for determining the constants of a voltaic circuit, including, among others, the rheostat and the differential resistance measurer (or Wheatstone's bridge, as it is usually called), which, in one or other of its modifications, is become an indispensable means of measuring the resistance of telegraphic wires and cables, as well as for determining electromotive forces. The description of these methods is contained in a paper in the 'Philosophical Transactions' for 1843.

But it is with the Electric Telegraph that the name of Sir C. Wheatstone is in the public mind most completely identified; and ever since the first messages were transmitted along the Great Western Railway by insulated copper wires enclosed in iron tubes, to the present day—when a network of copper wires insulated by means of caoutchouc is suspended across our public thoroughfares for the instantaneous transmission of intelligence, not merely from one district to another in our large towns, but from one continent and capital to another—Sir C. Wheatstone has not ceased to contribute the most important aid towards perfecting the means of electro-telegraphic communication.

A bare enumeration of these various inventions would carry us beyond our limits on the present occasion. In 1840 he devised a cable adapted for transmitting intelligence under the sea; and it is to him that we are indebted for the Alphabetic Dial Telegraph working without any clock-power, and in which a magneto-electric machine supplies the place of a voltaic battery. These instruments were first used in the Paris and Versailles Railway in 1846.

A more recent invention is his High-Speed Telegraph, in which the

messages, previously prepared on slips of paper, are, by passing through a very small machine constructed somewhat on the principle of the Jacquard Loom, made to print the messages at the remote station, in the ordinary telegraphic characters, with a rapidity unattainable by the hand of an operator.

Allied to these inventions are others where electro-magnetism is the motive power, as, for example, the electro-magnetic clock for telegraphing time, a modification of which has since been employed to aid in determining the longitude of distant places; also the Chronoscope, for measuring the velocity of projectiles or falling bodies.

In this enumeration of his discoveries, inventions, and researches, we have passed over many, such as his speaking machine, the investigation of Fessel's Gyroscope, his experiments in illustration of Foucault's proof of the rotation of the earth, and others.

More than enough, however, has been stated to justify the presentation of the Copley Medal on this occasion to our eminent fellow-countryman.

SIR CHARLES WHEATSTONE,

I have the very agreeable duty of presenting to you this Medal, which you will receive as a testimony of the sense so universally entertained by your countrymen, and specially by the Fellows of the Royal Society, of the high scientific merit and practical value of your many discoveries and inventions, and of their varied applications.

The Council has awarded a Royal Medal to the Rev. Dr. George Salmon, Regius Professor of Divinity in the University of Dublin, for his original investigations on Analytical Geometry, published in the Transactions of the Royal Irish Academy and in the Philosophical Transactions,—and, specially, for his solution of the problem of the degree of a surface reciprocal to a given surface—and for his researches in connexion with surfaces subject to given conditions, analogous to those of Chasles in plane curves.

Besides the original investigations thus referred to, Dr. Salmon is the author of a series of works on Conic Sections, on higher Plane Curves, on Geometry of Three Dimensions, and on higher Algebra (the modern Analysis), full of original matter of great value to the advanced mathematician, and at the same time adapted to the requirements of the student. These works have become widely spread as text-books throughout Europe; and the estimation in which they are held is attested by the fact that they have already been translated into French, German, Italian, and Russian.

DR. SALMON,

I have the pleasure of presenting you this Medal in testimony of the

high estimation in which your attainments and labours in the higher branches of mathematics are held by the Royal Society.

A Royal Medal has been awarded to Mr. Alfred Russell Wallace, in recognition of the value of his many contributions to theoretical and practical zoology, among which his discussion of the conditions which have determined the distribution of animals in the Malay archipelago (in a paper on the zoological geography of that region, published in the Proceedings of the Linnean Society for 1859) occupies a prominent place.

The case may be briefly stated thus:—The strait separating the islands of Baly and Lembok is only fifteen miles wide; nevertheless the animal inhabitants of the islands are widely different, the fauna of the western island being substantially Indian, that of the eastern as distinctly Australian.

Mr. Wallace has described, in a far more definite and complete manner than any previous observer, the physical and biological characters of the two regions which come into contact in the Malay archipelago; he has given an exceedingly ingenious and probable solution of the difficulties of the problem, while his method of discussing it may serve as a model to future workers in the same field.

Another remarkable essay, "On the tendency of Varieties to depart indefinitely from the Original Types," published in the Proceedings of the Linnean Society for 1858, contains an excellent statement of the doctrine of Natural Selection, which the author, then travelling in the Malay archipelago, had developed independently of Mr. Darwin; and, apart from its intrinsic merits, this paper will always possess an especial interest in the history of science, as having been the immediate cause of the publication of the 'Origin of Species.'

Mr. Wallace's ability as an observer and describer of animal forms is shown in his numerous and valuable contributions to our knowledge of the animals, and especially the Pigeons, Parrots, and Butterflies, of the Malayan region.

It must not be forgotten that a knowledge of the circumstances under which the majority of these contributions to the higher branches of zoological science were made must greatly enhance our respect for the author. Mr. Wallace has spent the greater part of his life amidst the exhausting and often dangerous fatigues of a traveller in tropical countries rarely explored by Europeans; and some of his most valuable papers are dated from places which some might consider so little favourable to study as Ternate and Sarawak.

MR. WALLACE,

I have the pleasure of presenting to you this Medal in recognition of the great merit of your researches both in practical and theoretical Zoology, carried out in countries where such pursuits are necessarily attended with more than usual difficulties and dangers.

The Rumford Medal has been awarded to Mr. Balfour Stewart, for his researches on the qualitative as well as quantitative relations between the powers of emission and absorption of bodies for heat and light, published originally in the Transactions of the Royal Society of Edinburgh and in the Proceedings of the Royal Society of London, and now made more generally accessible by the publication, in 1866, of his treatise on heat.

When a body is placed within an opaque envelope which is kept at a constant temperature, it soon acquires the temperature of the envelope—and that, whatever be the nature or form of the envelope or of the body. The same is true if any number of bodies of different kinds be placed within the envelope; in the permanent state each of the bodies attains a fixed temperature, the same as that of the walls of the envelope. The equilibrium of temperature is not, however, of the nature of statical equilibrium; according to the theory by which Prevost so beautifully explained the apparent radiation of cold, each body radiates heat all the while, at a rate depending only on its nature and temperature, and not at all on its environment; and it is because the other bodies and the envelope are also radiating heat, and the first body absorbs a portion of the radiant heat thus falling upon it, that its temperature remains unchanged. The equality of radiation and absorption follows as a simple corollary.

It had long been known that rock-salt is remarkable for its transparency for obscure radiant heat. According to Melloni, a plate of rock-salt of the thickness of three or four millimetres transmits 92 per cent. of heat-rays from whatever source. Now, on measuring by the thermopile the radiation from thin and thick plates of rock-salt, as well as from two or more plates placed one behind the other, all being heated up to a definite temperature, Mr. Stewart found that the radiation from a thick plate, or from many plates, was, indeed, greater than from a thin plate or from a single plate, but that the difference was not by any means so great as it ought to have been on the supposition that the heat radiated by the hinder portion of a thick plate, or by the hinder plates of a group, passed through the front portion of a thick plate, or through the front plate of a group, as freely as obscure heat would have passed which was radiated by lampblack or most other substances. It thus appeared that rock-salt at any temperature is by no means transparent to heat radiated by rock-salt of the same temperature—that it exerts a preferential absorption on rays of the quality of those which it emits. This conclusion was confirmed by using a plate of cold rock-salt as a screen by which to sift the heat-rays falling on the thermopile. It was found that a much larger proportion of the heat was stopped by the screen when the source of heat was a plate of heated rock-salt than when it was a body coated with lampblack. The proportion stopped was also sensibly greater when the source of heat was a thin than when it was a thick plate of rock-salt, the reason being that the heat radiated from the hinder portion of a thick plate was partially sifted, in passing across the front portion, before it reached the rock-salt

screen, and therefore was transmitted by it in greater proportion than the heat which radiated from the front portion.

Similar conclusions were obtained from experiments on glass and mica, though the numerical results were not so striking, in consequence of the comparatively great opacity of those substances for obscure radiant heat.

It thus appeared, 1st, that the heat radiated by a body is not confined to that which comes from the immediate neighbourhood of the surface, but emanates from various, in the case of rock-salt considerable, depths; 2ndly, that there is a relation between the quality of the heat radiated and that absorbed by any given element of a body, and consequently by a sufficiently thin plate of a body, of such a nature that the kind of heat most freely radiated is also most freely absorbed.

These results and others were comprehended by Mr. Stewart in a definite theory, by means of his extension of Prevost's theory of exchanges. According to this extension, the stream of radiant heat within a uniformly heated enclosure is the same throughout in *quality* as well as quantity; *i. e.* the uniformity of radiation exists for *each kind of heat in particular* of which the total flux is made up.

Few now can doubt the identity of nature of radiant heat and light; and, accordingly, the application to light of the extension of Prevost's theory was an obvious step. This step was taken by Mr. Stewart, who verified by experiment that which theory predicted—that a coloured glass when heated, as compared with an opaque body glowing at the same temperature, gives out by preference rays of the kind which it absorbs, and consequently tends to glow with a colour complementary to its own. For a similar reason a plate of tourmaline cut parallel to the axis, when heated, and viewed in a direction perpendicular to the axis, is seen to glow with light which is partially polarized in a plane parallel to the axis.

It is right to mention that, in regard to the extension of Prevost's theory in its application to light, Mr. Stewart was slightly anticipated by Professor Kirchhoff, whose brilliant application of the theory to the lines of the spectrum has attracted general attention, whose researches, however, had hardly, if at all, reached this country when Mr. Stewart's papers were presented. As regards Radiation, however, without specifying of what kind, the priority in the extension of Prevost's theory belongs to Mr. Stewart, whose papers on Heat were published before those of Professor Kirchhoff, to whom, however, they were not known when he published his earlier papers.

MR. STEWART,

I have particular pleasure in presenting to you this Medal, because it will testify to you that all that really conduces to the advance of our knowledge meets sooner or later with its due recognition—and because I hope that this tribute to your earlier labours will be especially agreeable to you now that you are engaged in work of high public value, but which necessarily leave you little leisure for such original researches.

On the motion of Sir Charles Lyell, seconded by Sir Thomas Watson, it was resolved,—“That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed.”

The Statutes relating to the election of Council and Officers having been read, and Sir Edwin Pearson and Mr. Erasmus Wilson having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected, and the following were declared duly elected as Council and Officers for the ensuing year :—

President.—Lieut.-General Sabine, R.A., D.C.L., LL.D.

Treasurer.—William Allen Miller, M.D., D.C.L., LL.D.

Secretaries.— { William Sharpey, M.D., LL.D.
 { George Gabriel Stokes, Esq., M.A., D.C.L., LL.D.

Foreign Secretary.—Prof. William Hallows Miller, M.A., LL.D.

Other Members of the Council.—Frederick Augustus Abel, Esq. ; Sir Benjamin Collins Brodie, Bart., M.A. ; William Benjamin Carpenter, M.D. ; J. Lockhart Clarke, Esq. ; Frederick Currey, Esq., M.A. ; Warren De La Rue, Esq., Ph.D. ; Sir William Fergusson, Bart. ; William Henry Flower, Esq. ; Capt. Douglas Galton, C.B. ; John Peter Gassiot, Esq. ; John Hawkshaw, Esq. ; John Marshall, Esq. ; Joseph Prestwich, Esq. ; George Henry Richards, Capt. R.N. ; Archibald Smith, Esq., M.A. ; Lieut.-Col. Alexander Strange.

Receipts and Payments of the Royal Society between December 1, 1867, and November 30, 1868.

	£	s.	d.		£	s.	d.
Balance at Bank and on hand	495	10	3	Salaries, Wages, and Pension	1037	6	0
Annual Subscriptions, Admissions Fees, and Compositions ..	1585	4	0	The Scientific Catalogue	342	15	0
Rents	251	5	0	Instruments for India and freight	293	5	0
Dividends	1455	6	7	Books for the Library and Binding	219	9	0
Ditto, Trust Funds	280	12	4	Printing Transactions and Proceedings, Paper, Binding, ..	1651	13	10
Sale of Transactions, Proceedings, &c.	371	0	1	Engraving, and Lithography	356	2	2
Repayments	341	7	8	General Expenses (as per Table subjoined) ..			
				Donation Fund ..	335	0	0
				Wintringham Fund	35	5	0
				Copley Medal Fund ..	4	14	10
				Prof. Roscoe, Bakerian Lecture ..	4	0	0
				Rev. Dr. Stebbing, Fairchild Lecture ..	2	19	0
				Croonian Lecture, Poor of St. James' Parish ..	2	19	0
				Mabletborpe Schools, Donation	2	2	0
				Balance at Bank	4286	11	5
				Balance of Catalogue Account	479	16	1
				" Petty Cash Account	12	2	3
					1	16	2
					£4780	5	11

WILLIAM ALLEN MILLER,
Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mabletborpe, Lincolnshire (55 A. 2 R. 3 P.), £126 per annum.
 Estate at Acton, Middlesex (34 A. 3 R. 27½ P.), £100 per annum
 Fee Farm near Lewes, Sussex, rent £19 4s. per annum.
 One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
 £14,000 Reduced 3 per Cent. Annuities.
 £29,569 15s. 7d. Consolidated Bank Annuities.
 £513 9s. 8d. New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.

Scientific Relief Fund.

Investments up to July 1865, New 3 per Cent. Annuities..... £3052 17 8

Cr.

Dr.	£	s.	d.
Balance	197	6	0
Donation	5	0	0
Dividends	177	1	0
	<hr/>		
	£379	7	0

Statement of Income and Expenditure (apart from Trust Funds) during the Year ending November 30, 1868.

	£	s.	d.		£	s.	d.
Annual Subscriptions	1049	4	0	Salaries, Wages, and Pension	1087	6	0
Admission Fees	160	0	0	The Scientific Catalogue.....	342	15	0
Compositions	376	0	0	Instruments for India and freight	283	5	0
Rents	251	5	0	Books for the Library.....	122	11	3
Dividends on Stock (exclusive of Trust Funds)	1005	6	2	Binding ditto	96	17	9
" on Stevenson Bequest	449	0	5	Printing Transactions, Part II. 1867, and } 409 8 6			
Sale of Transactions, Proceedings, &c.	371	0	1	Part I. 1868	289	18	6
Cost of Instruments, repaid	224	18	6	Ditto Proceedings, Nos. 98-104	68	6	5
Chemical Society, Tea Expenses	12	0	0	Ditto Miscellaneous	1651	13	10
Linnean Society, Tea Expenses				Paper for Transactions and Proceedings	311	4	6
Geographical Society, Gas at Evening } 8 4 6	11	16	8	Binding and Stitching ditto.....	112	10	9
Meetings				Engraving and Lithography	480	5	2
Cambridge Local Examination Committee, Gas 3 13 0				Fittings, Cleaning, and Repairs.....			
Laundry Petty Receipts	12	12	6	Miscellaneous Expenses	45	4	4
				Coal, Lighting, &c.	37	10	6
Income available for the Year ending Nov. 30, 1868.....	4004	3	4	Tea Expenses	180	7	3
Expenditure in the Year ending Nov. 30, 1868	3869	11	7	Fire Insurance	54	14	11
				Taxes	28	11	6
Excess of Income over Expenditure in the Year ending } £104 11 9				Advertising	10	11	3
Nov. 30, 1868				Postage, Parcels, and Petty Charges.....	12	10	6
					35	12	6
					<hr/>		
					£3890	11	7

WILLIAM ALLEN MILLER, Treasurer.

154 *Number of Fellows, changes and present state of.* [Nov. 30,

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal.	Foreign.	Com- pounders.	£2 12s. yearly.	£4 yearly.	Total.
November 30, 1867.	5	48	298	2	264	617
Since elected		+4	+7		+9	+20
Since re-admitted ..					+1	+1
Since compounded..			+1		—1	
Since deceased	—1	—4	—17		—12	—34
Since withdrawn ..					—1	—1
Defaulters					—3	
November 30, 1868.	4	48	289	2	257	600

December 10, 1868.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President,
in the Chair.

It was announced from the Chair that the President had appointed the following Members of Council to be Vice-Presidents:—

The Treasurer.
Dr. Carpenter.
Mr. Gassiot.
Mr. Prestwich.
Capt. Richards.

Pursuant to notice given at the last Meeting, General Sabine proposed and Sir Roderick Murchison seconded the Right Honourable Lord Houghton for election and immediate ballot.

The ballot having been taken, Lord Houghton was declared duly elected.

The following communications were read:—

- I. "On the Phenomena of Light, Heat, and Sound accompanying the fall of Meteorites." By W. RITTER v. HAIDINGER, For. Mem. R.S. &c. Received October 6, 1868.

A particular incident caused me to return to some portions of my earlier studies in regard to meteors and meteorites.

It was the fall of a meteorite at Kakowa on the 19th of May 1858 that first induced me to bestow some more attention on this department of physical science. A report on the subject I laid before our Imperial Academy of Vienna on the 7th of January, 1859. On the same day also I gave the first list of the meteorites forming the meteorite collection in our Imperial Mineralogical Museum. A series of reports on meteorites followed, as well as a number of catalogues of meteorites, in accordance with the growing riches of the collection, embracing from 137 to 236 numbers of localities preserved up to the date of July 1, 1867.

But the studies relating to the recent fall of Ausson on the 9th of December 1858, and the ancient fall of the meteoric iron of Hraschina, near Agram, on the 26th of May 1751, others on the Cape meteorites of 1838, on those of Shalka, 1850, Allahabad, 1822, Quenggouk (Pegu), 1857, Assam, found 1846, Segowlee, 1853, St. Denis-Westrem, 1855, Nebraska, found 1856, but particularly some studies relating to meteorites of Stannern, 1858, and of that most remarkable meteoric iron from Tula, discovered in 1856 by Auerbach, all of them within the period of 1851 to 1860, and then the fall of New Concord, 1860, and of Parnallee, 1857, had forcibly called upon me to draw up, as it were, a general rule of the nature and succession of events which probably might have taken place in the history of their existence, though in each particular case only fragments of that history came to our notice.

A general survey of this kind I had the honour to lay before our Imperial Academy on the 14th of March 1861, "On the nature of Meteorites, relating to their composition and the phenomena of their fall"*. I felt, it is true, that I had rather too boldly ventured to transgress the limits of my former studies; but at the same time, led on by the high interest connected with the subject, I wished to gain some more publicity for it. As to England, I was most kindly and effectively patronized by that energetic promoter of meteoritic science my most honoured friend Mr. R. P. Greg. He laid a notice of mine before the British Association for the Advancement of Science, held that year at Manchester, and accompanied it with several considerations of his own †; then, also, he kindly had the pages of the Philosophical Magazine opened for me, and presented me with an edition of separate copies of a memoir on the subject—nearly a translation, by my honoured friend Count A. F. Marschall, of my original communication to our Academy ‡.

At the Meeting of German naturalists and physicians at Speyer, my honoured friend Dr. Otto Buchner kindly called the attention of the friends of this department of natural science to my memoir, which had been favourably mentioned in the reports.

A note of mine, containing the leading views of my papers, was likewise laid by my honoured friend M. Elie de Beaumont before the Paris Académie des Sciences in their Meeting of September 9th, 1861, while I also sent a French translation by my excellent friend Count Marschall, together with a copy of my original memoir §.

Since that time, up to this day, I had frequently, in several communications on meteoritic subjects, had an opportunity to refer to these leading papers, and to support the views which they contained. Therefore I had every reason to be astonished when I read, in a recent work on meteorites by M. Stanislas Meunier||, the following assertion:—"We may observe that a great number of particular phenomena occurring in the fall of meteorites have hitherto remained without explanation. Thus the reason of the ex-

* "Ueber die Natur der Meteoriten in ihrer Zusammensetzung und Erscheinung," Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften, der Mathem.-naturw. Classe, 1861, Band xliii. Abth. ii. S. 389-425.

† "An attempt to account for the Physical Condition and the Fall of Meteorites upon our Planet, by W. Haidinger, Hon. Memb. R.S.L. & E. &c.," Report, 1861, Transactions of the Sections, p. 15. "Some Considerations on M. Haidinger's Communication on the Origin and Fall of Aërolites, by R. P. Greg, F.G.S.," *ibid.* p. 13.

‡ Considerations on the Phenomena attending the Fall of Meteorites on the Earth, by W. Haidinger, For. Memb. R.S.L. & E.; and Philosophical Magazine for November and December 1861.

§ "De la nature des bolides et de leur mode de formation. Lettre de M. Haidinger," Comptes Rendus hebdomadaires des séances de l'Académie des Sciences etc. t. liii. Juillet-Décembre 1861, pp. 456-461.

|| Étude, descriptive, théorique, et expérimentale sur les Météorites, par M. Stanislas Meunier. Paris, 1857, p. 18.

plosions, and particularly of the repeated explosions, that of the rumblings, that of the incandescence, are still absolutely unknown"*.

But I had still more reason to be astonished when I found M. Daubrée himself nearly upon the same level in his views respecting the origin of light, heat, and sound in the fall of meteorites.

I certainly heartily appreciate the high merit of my honoured friend M. Daubrée, in regard both to his deep studies on meteorites and his eminent success in forwarding the interests of the Paris Museum of Meteorites; but I at the same time may be permitted to consider my own views, as given in the memoirs quoted, as representing a scientific advance compared with the statements of M. Meunier and those of M. Daubrée himself in his last memoir on the Orgueil fall†. Neither M. Daubrée nor M. Meunier had refuted or even objected to my views; they had only passed them over in silence, doubtless because they had escaped their notice.

But I believe I am fulfilling a duty to scientific progress if I endeavour to place the discrepant and even contradictory views on these subjects together, with the view once more to excite attention and recommend them to further study on the part of the votaries of natural science; and it was with this view that I prepared a new memoir, to be laid before our Imperial Academy in their approaching period of session, on the light, heat, and sound accompanying the fall of meteorites. I begin with some of the statements put forth by M. Daubrée, as taken from his memoir on Orgueil:—

“Things go on as if the greater part of the mass of the meteor got out of our atmosphere, in order to continue its course, after having left us some particles, the velocity of which, in consequence of the explosion, was reduced”‡. M. Daubrée does not admit the arrival of groups or swarms of meteorites as has been asserted§. “The carbonaceous meteorites contradict the hypothesis that the heat of the meteorites is due to the loss of their *vis viva*”||. The sounds, detonations under the name of explosions, remain without explanation¶.

M. Daubrée attributes to mere chance the situation of what he calls “scales,” or “*écailles de météorites*,” at the moment of an explosion, if they present certain particular seams of crust surrounding their most ex-

* “Remarquons qu’un grand nombre de particularités offertes par la chute des météorites sont restées jusqu’à présent sans explication. Ainsi, la cause des explosions et surtout des explosions multiples, celle des roulements, celle de l’incandescence, sont absolument inconnues.”

† “Complément d’Observations sur la chute de météorites qui a eu lieu le 14 Mai 1864 aux environs d’Orgueil,” *Nouvelles Archives du Muséum d’Histoire Naturelle*, t. iii. pp. 1–19.

‡ “Les choses se passent donc comme si la plus grande partie de la masse météorique ressortait de l’atmosphère pour continuer sa trajectoire, n’abandonnant que quelques parcelles dont la vitesse, à la suite de l’explosion, se trouvait amortie.”—*Op. cit.* p. 15.

§ Comme on l’a dit.

|| “Les météorites charbonneuses contredisent l’hypothèse que la chaleur des météorites est due à la perte de leur force vive.”—*Op. cit.* p. 8.

¶ “Sans explication.”—*Op. cit.* p. 16.

tended surface, by which, being foremost, they forced their way through the opposing atmosphere. In regard to this position I had long ago advanced that it must have been a necessary result, while the rectilinear movement of the meteorite was in the way of being checked, of part of the force having been expended in producing a rotatory motion, perpendicular to the direction of the course. This I did in particular, in a paper on the meteoric iron of Hraschina, on the 14th of April 1859, then on an aërolite from Stannern on the 22nd of May 1862, and in other instances.

The above-mentioned quotations of M. Danbrée's views are now compared with the successive periods of progress in the fall of meteorites, nearly in the same words as I proposed them in 1861.

In the arrival of meteorites on our earth:—

1. Single or agglomerated fragments, in their cosmical course, come into contact with our globe.

2. The fragments are arrested by the resistance of atmospheric air.

3. Pressure, in their progress through the atmospheric air, elicits light and heat; rotation ensues, and a melted crust is formed.

4. The white-hot compressed air is spread out in the form of a fireball, closed up behind, and enclosing the fragment, or fragments, and a vacuum-space.

5. The cosmic course is at an end when the fragment, or the fragments, have been arrested by air.

6. Light and heat are no longer generated; the vacuum-ball will collapse with a loud report, or several reports following each other.

7. The cosmic cold within the aërolite assists in reducing the heat of the melted crust.

8. The meteorite falls down upon the earth like any other ponderous body, the hotter the better conducting material it consists of.

In this way I believe it was my duty again to lay before the public the differences of the views newly taken by M. Daubrée from those which I hitherto had advocated.

But while I was engaged in contrasting them I found myself conspicuously supported by a number of recent publications relative to the subject in question. In one of his own papers M. Daubrée had to register the statement of M. Leymerie, of Toulouse, who considered the fall of Orgueil as presenting not one meteoric mass exploded, but a swarm of aërolites arrived at the same moment.

But above all, two reports of the fall of 30th January 1868, near Pultusk, both of them kindly presented to me by their respective authors, bore ample testimony in favour of a number of my theses, and enlarged them by deeper and more accurate investigation beyond what I formerly proposed.

These are the memoir “On the Course of the Pultusk Meteorite” *, by

* Ueber die Bahn des am 30. Januar 1868 beobachteten und bei Pultusk im Königreiche Polen als Steinregen niedergefallenen Meteors durch die Atmosphäre. Vom Professor Dr. C. G. Galle, Direktor der Sternwarte zu Breslau. Vorgetragen am 4.

Professor J. G. Galle in Breslau, and another, "On the Meteorites of Pultusk" *, by Professor G. vom Rath, in Bonn.

In both of them the most evident proofs are given of the actuality of a swarm, consisting of a very great number of distinct aërolites, having entered our atmosphere.

The course of the Pultusk meteor, according to M. Galle, met the horizontal line under an angle of 44 degrees at the place of dispersion, at a height of 25·25 English miles, or $5\frac{1}{2}$ German miles. After its movement was checked, and the force of it expended in the development of light and heat, how would it have been possible that, as it would follow from M. Daubrée's supposition, the great mass of the meteor should have risen again and left our atmosphere to continue its cosmical orbit? Nor could such be the case with the Knyahinya meteor, which pounced upon our earth almost from the zenith of the place, the course making an angle only of 6 degrees with the perpendicular. But even the Orgueil meteor moved in a direction meeting the horizontal line at the point of dispersion under an angle of about $11^{\circ} 26'$, from which position it certainly could not rise again higher up into the atmosphere, and still less leave it altogether.

I availed myself of the circumstance that I had been gratified by several honoured friends with a number of important publications closely connected with the subject, to quote some appropriate passages. I would refer especially to that grand 'Atlas of Charts of Meteor-tracks,' by Messrs. R. P. Greg and A. S. Herschel †, together with the "Reports of Luminous Meteors for the years 1865 and 1866-1867" ‡, and to the recent memoir by M. G. V. Schiaparelli on the astronomical theory of falling stars §, kindly sent to me by the late lamented Matteucci. Schiaparelli holds forth that in shooting-stars "the *vis viva*, while the meteoric matter is dispersed in the atmosphere, is completely destroyed by being transformed into heat and light" ||. From

März u. s. w. Besonderer Abdruck aus den Abhandlungen der Schlesischen Gesellschaft für vaterländische Cultur. Breslau, 1868.

* Ueber die Meteoriten von Pultusk im Königreiche Polen gefallen am 30. Januar 1868. Von Dr. G. vom Rath. Mit einer Tafel. Besonders abgedruckt aus der Festschrift der Niederrheinischen Gesellschaft für Natur- und Heilkunde zum 50jährigen Jubiläum der Universität Bonn.

† Atlas of Charts of the Meteor-tracks contained in the British Association Catalogue of Observations of Luminous Meteors, extending over the years from 1845 to 1866, &c.' Prepared for the Luminous-Meteors Committee of the British Association by R. P. Greg and A. S. Herschel.

‡ Report on Observations of Luminous Meteors, 1865-66, by a Committee consisting of James Glaisher, F.R.S., of the Royal Observatory, Greenwich, Secretary to the British Meteorological Society; Robert P. Greg, F.G.S.; E. W. Brayley, F.R.S.; and Alexander Herschel, B.A. From the Report of the British Association for the Advancement of Science for 1866. The same for 1866-1867.

§ 'Note e riflessioni intorno alla teoria astronomica delle Stelle Cadenti.' From the work 'Memorie di Matematica e di Fisica della Società Italiana delle Scienze fondata da Anton Mario Lorgna,' ser. 3, tomo i. parte 1. p. 153, Firenze, 1867.

|| "Questa forza viva, dileguandosi la materia meteorica nell' atmosfera, viene completamente distrutta trasformandosi in calore ed in luce."—*Op. cit.* p. 198.

Mr. A. S. Herschel's observations with the spectroscope, we learn that the condition of the August meteors is exactly that of a flame of gas in a Bunsen's burner freely charged with the vapour of burning sodium, or of the flame of a spirit-lamp newly trimmed and largely dosed with a supply of moistened salt (*op. cit.* p. 146). The idea of a diminutive fireball containing the solid mass, although diminutive itself, surrounded by a luminous gaseous case, including a vacuum, till the force of the movement is spent in heat and light, may not be considered inadequate to the subject.

In a most interesting memoir entitled "Contributions to the Knowledge of Falling Stars" *, by Dr. Edmond Weiss, of Vienna, that able astronomer (the representative, together with Dr. Oppolzer and Lieut. Reziha, of the Austrian Navy, of our Austrian expedition for the eclipse of 18th of August at Aden, where they were so hospitably welcomed and kindly supported by the Governor-General, J. Russell, in behalf of the British Government, along with the North-German expedition, composed of Drs. Vogel, Fritsche, Zenker, and Thiele) considers among other subjects the influence of the earth's attraction upon shower-meteors, independently of Schiaparelli's disquisitions relative to the same subject, and points out also the circumstance that some of them may receive such a direction as to leave our solar system altogether, while Dr. Galle insists upon the fact that the Pultusk swarm must have entered it with an independent force of at least from $4\frac{1}{2}$ to 7 English miles (1 to $1\frac{1}{2}$ geographical miles).

My original design was only to offer some appropriate remarks on the subject of the phenomena of light, heat, and sound generated in and accompanying the arrival of meteorites on the earth through our terrestrial atmosphere; but the different departments of natural science referring to meteors and meteorites are of so manifold a nature, that I frequently was obliged to advert to some of them in regard to which I should rather have kept more on the reserve. But the whole range of meteor- and meteorite-science, continually enlarging, more and more clearly presents itself in these four grand sections:—1st, the original formation of meteorites; 2nd, their movement through cosmic space; 3rd, their arrival through the atmosphere upon our earth; and, 4th, the studies instituted on the objects themselves, which fall into our hands and are preserved in our museums. To the third of these sections it is that my particular attention was directed.

* "Beiträge zur Kenntniss der Sternschnuppen, von dem c. M. Dr. Edmund Weiss. Vorgelegt in der Sitzung am 16. Jänner 1868," Sitzungsberichte der Math.-nat. Classe der Kais. Akademie der Wissenschaften, lvii. Band ii. Abth. 5. pp. 281-342.

II. "On the Solar and Lunar Variations of Magnetic Declination at Bombay."—Part I. By CHARLES CHAMBERS, Esq., Superintendent of the Colaba Observatory. Communicated by B. STEWART, LL.D. Received June 30, 1868.

(Abstract.)

The hourly observations of magnetic declination at the Government Observatory, Bombay, have extended over a period of nearly a quarter of a century, but the present discussion is confined to the observations made in the seven years 1859 to 1865. After describing the instrument with which the observations were made and the method of reducing them, the writer exhibits, by means of Tables and curves, the following results:—

1st. The agreement of the diurnal variation of the aggregate of easterly disturbances when different *separating values* are adopted.

2nd. The same for the aggregate of westerly disturbances.

3rd. The diurnal variation of the aggregate of easterly disturbances, exceeding 1'·4 in amount, in the period of seven years.

4th. The same for westerly disturbances.

5th. The disturbance-diurnal variation, or the excess at each hour of the aggregate of easterly over the aggregate of westerly disturbances.

6th. The aggregates of easterly, westerly, and easterly and westerly disturbances, and the numbers of disturbed observations in each month of the year.

7th. The aggregates of easterly, westerly, and easterly and westerly disturbances, and the numbers of disturbed observations, in each of the years 1859 to 1865, and in the period of seven years.

8th. The solar-diurnal variation of declination in each month of the year, and for the whole year.

9th. The excess of the diurnal variation of declination for each month over the mean diurnal variation for the year.

10th. The mean diurnal variation of declination for the half-years April to September and October to March, in each of the years 1859 to 1865.

11th. The semiannual inequality in the mean diurnal variation of declination.

12th. the mean diurnal variation of declination for each of the years 1859 to 1865.

13th. The calculated values of the coefficients A_1 , B_1 , A_2 , B_2 , A_3 , and B_3 in the equation

$\delta_\lambda = A_1 \cos n + B_1 \sin n + A_2 \cos 2n + B_2 \sin 2n + A_3 \cos 3n + B_3 \sin 3n + \&c...$, which expresses the mean diurnal variation of declination for each month of the year, for the whole year, and for different half-years.

14th. The same for the half-years April to September and October to March, in each of the years 1859 to 1863.

15th. The solar-diurnal inequality of declination, in the calculation of which all disturbances are included.

16th. The variation from year to year in the range of the mean diurnal variation of declination.

17th. The secular change and semiannual inequality of absolute declination.

The diurnal variations of disturbance, both easterly and westerly, are found to be of definite and systematic character, and to be comparable with the same variations for other places; the annual variation is not very regular, but the progression in the amounts of disturbance in different years accords well (with exception as to the incomplete year 1861) with the known character of the decennial variation. The mean diurnal variation of declination, as well as its semiannual inequality, is of the general character due to the latitude in which Bombay lies; the progression from month to month in the annual variation of the diurnal variation is also distinctly marked in all months except July. A semiannual inequality is shown to exist in the diurnal variation of declination whose times of opposition are the equinoxes. It is found that this inequality not only exists, but has the same general character at five widely separated stations in the northern magnetic hemisphere, and also, with some modifications as to character, at two stations having south magnetic latitude. Its special characteristics are:—

1st. That, as in the typical mean diurnal variation of declination, there is scarcely any change during the night hours, and that the main variation occurs during half the day, in this case between 18 hours and 6 hours, local astronomical time.

2nd. That the range of variation differs from about half a minute to nearly a minute of arc.

3rd. That the hour of noon is that about which the deviations due to this variation pass through zero, and on each side of which the inflexions of the representative curve are inversely, but, in respect to north latitude stations, symmetrically disposed.

4th. The turning-points are 21 hours and 3 hours, the former being a maximum, and the latter a minimum for north latitude stations from January to June, and for south latitude stations from July to December; and *vice versa*, for north latitude stations from July to December, and for south latitude stations from January to June. The solar-diurnal inequality of declination, in the calculation of which all disturbances are included, differs at no hour of the day by more than $0'061$ from the mean diurnal variation, which is calculated after the rejection of all observations disturbed to the extent of more than $1'4$.

The range of the diurnal variation of declination in different years is shown to be subject to a periodical variation, whose times of maxima and minima approach nearly to those of the maxima and minima of the decennial period in the amount of yearly disturbance.

The secular change of absolute declination is found for the years 1859 to 1865 to be an annual increase of easterly declination of $3'017$; the semi-

annual inequality to be an excess of $0'227$ of easterly declination in the months October to March over its value in the months April to September.

III. "On the Diurnal and Annual Inequalities of Terrestrial Magnetism, as deduced from observations made at the Royal Observatory, Greenwich, from 1858 to 1863; being a continuation of a communication on the Diurnal Inequalities from 1841 to 1857, printed in the Philosophical Transactions, 1863. With a Note on the Luno-diurnal and other Lunar Inequalities, as deduced from observations extending from 1848 to 1863." By GEORGE BIDDELL AIRY, Astronomer Royal. Received July 27, 1868.

(Abstract.)

The author states that the instruments employed are precisely the same which were used in the second part of the former investigation, from 1848 to 1857, mounted in the same place, and treated in the same manner. In describing the treatment of the photographic curves, he first gives the number of days which have been omitted in different years; because the character of the observations or curves was too disturbed to permit the usual treatment of the observations, or the drawing by hand of a pencil curve that would fairly represent the general course of the curve.

The greatest numbers of omitted days occur in the years 1846, 1847, 1848; 1851, 1852, 1853, 1854; 1859, 1860. As the estimate of the amount of irregularity has been made throughout by the same person, he considers that these years may be accepted as those in which the disturbances were the greatest. If they point to any cycle at all, it is one of 6 or $6\frac{1}{2}$ years. These days being omitted, the ordinates of the pencilled curves on the other days were used as basis of all the following investigations. For the solar inequalities, they were treated by collecting the measures for every complete solar day, or for every solar hour bearing the same ordinal number, according as the annual or diurnal inequalities were the subject of inquiry; but in all cases these quantities were next grouped by months, and the monthly means were taken.

In the further treatment, the means of the monthly means of every complete day for all the months of the same name in the different years were taken and corrected for secular change; the corrected numbers do not appear to indicate any sensible annual equation. Then the means of the monthly means of every solar hour for all the months of the same name in the different years were taken, giving the diurnal inequalities on the mean of years for the twelve separate months; and these present, for the declination (north to west) and horizontal force, for the period 1858 to 1863, sensibly the same differences between the summer months and the winter months as those for the period 1848 to 1857. For the vertical-force

curves also, the nodal passage in both periods is earliest in the summer months; but it is not quite certain whether the curves in autumn, in the period 1858 to 1863, are quite so bold as those in 1848 to 1857; the difference, however, if any, is inconsiderable. After this, the monthly means of every solar hour are taken through each year, giving the mean diurnal inequality of each year; and here a very remarkable change is observable. To explain this, it is necessary to refer to the former paper, where it is shown that the curves for diurnal inequality of the horizontal forces had very slightly increased from 1841 to 1847, but had rapidly diminished from 1848 to 1857, giving the smallest and most winter-like curves in 1856 and 1857. Now it is found that from 1858 to 1863 the curves have increased, with a little irregularity in 1861, till they are sensibly as large as they were at first. Thus—

1858	nearly resembles	1856
1859	„ „	1851
1860	„ „	1850
1861	„ „	1851
1862	„ „	1847
1863	„ „	1841

With regard to the diurnal inequality of vertical force, it appears that the curves gradually increased in boldness to 1855, and have gradually diminished to 1862. The nodal passages, it was remarked in the former paper, had been much accelerated in the hour of the day, from 1842 to 1857. Now, from 1858 to 1863, the hours of nodal passages have been retarded, till in 1863 they are again nearly the same as in 1848. In all these remarkable changes there is no appearance of cycle.

The author then proceeds to the treatment of lunar inequalities from 1848 to 1863. The bases of their treatment were thus obtained: the exact time of moon's transit was laid down on the time-scales of the photographic sheet, and the intervals were divided into lunar hours, and a new system of ordinates, corresponding to the lunar hours, was measured to the pencil curves. The system of grouping was precisely similar, *mutatis mutandis*, to that for the solar inequalities. First, for the menstrual inequalities. The declination seems to exhibit a distinct lunar menstrual inequality, with + maximum about the fifth day of lunation; the horizontal force seems to show a lunar semimenstrual equation with — maximum about the second day; the vertical force shows nothing certain, proving only that, if there is anything, it is very small. Secondly, for the luno-diurnal inequalities. The luno-diurnal inequalities in declination and horizontal force on the mean of 1858 to 1863 agree so closely with those on the mean of 1848 to 1857, as to leave no doubt of their existence and law as luno-semidiurnal inequalities, with no trace of luno-diurnal or other inequality.

Remarking the singular difference for different years which has presented

itself in the discussion of the solar inequalities, it appeared to the author very desirable to examine whether there is any discoverable difference in the lunar inequalities for the same years. The years were accordingly thus divided :—

Large solar curves.. 1848 to 1852, 1859, 1860, 1862, 1863.

Small solar curves.. 1853 to 1858, 1861.

On discussing these, it was found that in all cases the lunar horary epoch for the inequality was sensibly the same for years of large solar curves and for years of small solar curves ; but the coefficient was different. The value of the fraction

$$\frac{\text{lunar semidiurnal inequality in years of large solar curves}}{\text{lunar semidiurnal inequality in years of small solar curves}}$$
is

For declination 1·35

For horizontal force 1·25

The author remarks that it would seem possible to suggest two conjectural reasons for this remarkable association in the time-law of changes of solar effect and lunar effect. One is, that the moon's magnetic action is really produced by the sun's magnetic action ; and a failure in the sun's magnetic power will make itself sensible, both in its direct effect on our magnets and in its indirect effect through the intermediation of the moon's excited magnetism. The other is that, assuming both actions (solar and lunar) to act on our magnets indirectly by exciting magnetic powers in the earth, which alone or principally are felt by the magnets, the earth itself may have gone through different stages of magnetic excitability, increasing or diminishing its competency to receive both the solar and the lunar action.

The epochs of lunar inequality in western declination from north and in horizontal force to magnetic north are sensibly the same ; and the coefficients expressed in terms of horizontal force on the mean of all the years are sensibly the same, and equal to 0·000061. The direction of the composite disturbing force is therefore sensibly N.W. and S.E. magnetic, or (roughly) in the direction of a line from the Red Sea to the south of Hudson's Bay. It may be remarked in opposition to this that the solar diurnal action is mainly in the S.W. direction.

The luno-diurnal inequality of vertical force on the mean of all the years appears to consist of a luno-diurnal and a luno-semidiurnal term.

December 17, 1868.

Capt. RICHARDS, R.N., Vice-President, in the Chair.

The following communications were read :—

I. "On the Measurement of the Luminous Intensity of Light." By
WILLIAM CROOKES, F.R.S. &c. Received June 27, 1868.

(Abstract.)

The measurement of the luminous intensity of a ray of light is a problem the solution of which has been repeatedly attempted, but with less satisfactory results than the endeavours to measure the other radiant forces. The problem is susceptible of two divisions, the absolute and the relative measurement of light.

A relative photometer is one in which the observer has only to ascertain the relative illuminating powers of two sources of light, one of which is kept as uniform as possible, the other being the light whose intensity is to be determined. It is therefore evident that one great thing to be aimed at is an absolutely uniform source of light. In the ordinary process of photometry the standard used is a candle, defined by Act of Parliament as a "sperm of six to the pound, burning at the rate of 120 grains per hour." This, however, is found to be very variable, and many observers have altogether condemned the employment of test-candles as light-measures.

The author has taken some pains to devise a source of light which should be at the same time fairly uniform in its results, would not vary by keeping, and would be capable of accurate imitation at any time and in any part of the world by mere description. The absence of these conditions seems to be one of the greatest objections to the sperm-candle. It would be impossible for an observer on the continent, ten or twenty years hence, from a written description of the sperm-candle now in use, to make a standard which would bring his photometric results into relation with those obtained here. Without presuming to say that he has satisfactorily solved all difficulties, the writer believes that he has advanced some distance in the right direction, and pointed out the road for further improvement.

A glass lamp is taken of about 2 ounces capacity, the aperture in the neck being 0.25 inch in diameter; another aperture at the side allows the liquid fuel to be introduced; this consists of alcohol of sp. gr. 0.805, and pure benzol boiling at 81° C., which are mixed together in the proportion of five volumes of the former and one of the latter. The wick-holder consists of a platinum tube, and the wick is made of fifty-two pieces of platinum wire, each 0.01 inch in diameter. The flame of this lamp forms a perfectly shaped cone, the extremity being sharp, and having no tendency to smoke; without flicker or movements of any kind, it burns when protected from currents of air at a uniform rate of 136 grains per hour.

There is no doubt that this flame is very much more uniform than that of the sperm-candle sold for photometric purposes. Tested against a candle, considerable variations in relative illuminating power have been observed; but on placing two of these lamps in opposition, no such variations have been detected.

The instrument devised for measuring the relative intensities of the standard and other lights is next described ; it has this in common with that of Arago described in 1833, as well as with those described in 1853 by Bernard, and in 1854 by Babinet, that the phenomena of polarized light are used for effecting the desired end*. But it is believed that the present arrangement is quite new, and it certainly appears to answer the purpose in a way which leaves little to be desired. The instrument cannot be described without the aid of drawings which accompany the original paper, but its mode of action may be understood by the following description.

The standard lamp being placed on one of the supporting pillars which slide along a graduated stem, it is moved along the bar to a convenient distance, depending on the intensity of the light to be measured. The light to be compared is then fixed in a similar way on the other side of the instrument. On looking through the eyepiece two brightly luminous disks will be seen, of different colours. One of the lights must now be slid along the scale until the two disks of light, as seen in the eyepiece, are equal in tint. Equality of illumination is easily obtained ; for, as the eye is observing two adjacent disks of light which pass rapidly from *red-green* to *green-red*, through a neutral point of no colour, there is no difficulty in hitting this point with great precision. Squaring the distance between the flames and the centre will give inversely their relative intensities.

The delicacy of this instrument is very great. With two lamps, each about 24 inches from the centre, it is easy to distinguish a movement of one of them to the extent of one-tenth of an inch to or fro, and by using the polarimeter an accuracy exceeding this can be attained.

The employment of a photometer of this kind enables us to compare lights of different colours with one another. So long as the observer, by the eyepiece alone, has to compare the relative intensities of two surfaces respectively illuminated by the lights under trial, it is evident that, unless they are of the same tint, it is impossible to obtain that absolute equality of illumination in the instrument which is requisite for a comparison. By the unaided eye one cannot tell which is the brighter half of a paper disk illuminated on one side with a reddish, and on the other with a yellowish light ; but by using the photometer here described the problem becomes practicable. When the contrasts of colour are very strong (when, for instance, one is a bright green and the other scarlet) there is difficulty in estimating the exact point of neutrality ; but this only diminishes the accuracy of the comparison, and does not render it impossible, as it would be according to other systems.

* Since writing the above, I have ascertained that M. Jamin had previously devised a photometer in which the principle adopted in the one here described is employed, although it is carried out in a different and, as I believe, a less perfect manner.—W. C., Dec. 16, 1868.

II. "Preliminary Report," by Dr. WILLIAM B. CARPENTER, V.P.R.S.,
"of Dredging Operations in the Seas to the North of the British
Islands, carried on in Her Majesty's Steam-vessel 'Lightning,'
by Dr. CARPENTER and Dr. WYVILLE THOMSON, Professor of
Natural History in Queen's College, Belfast." Received October
22, 1868.

In accordance with the request of the President and Council of the Royal Society, conveyed in the Letter written by their direction to the Secretary to the Admiralty on the 18th of June (Appendix), the Lords Commissioners of the Admiralty were pleased to give their sanction to the scheme for Deep-sea Dredging therein proposed, and to furnish the means of carrying it out as effectively as the advanced period of the season might permit.

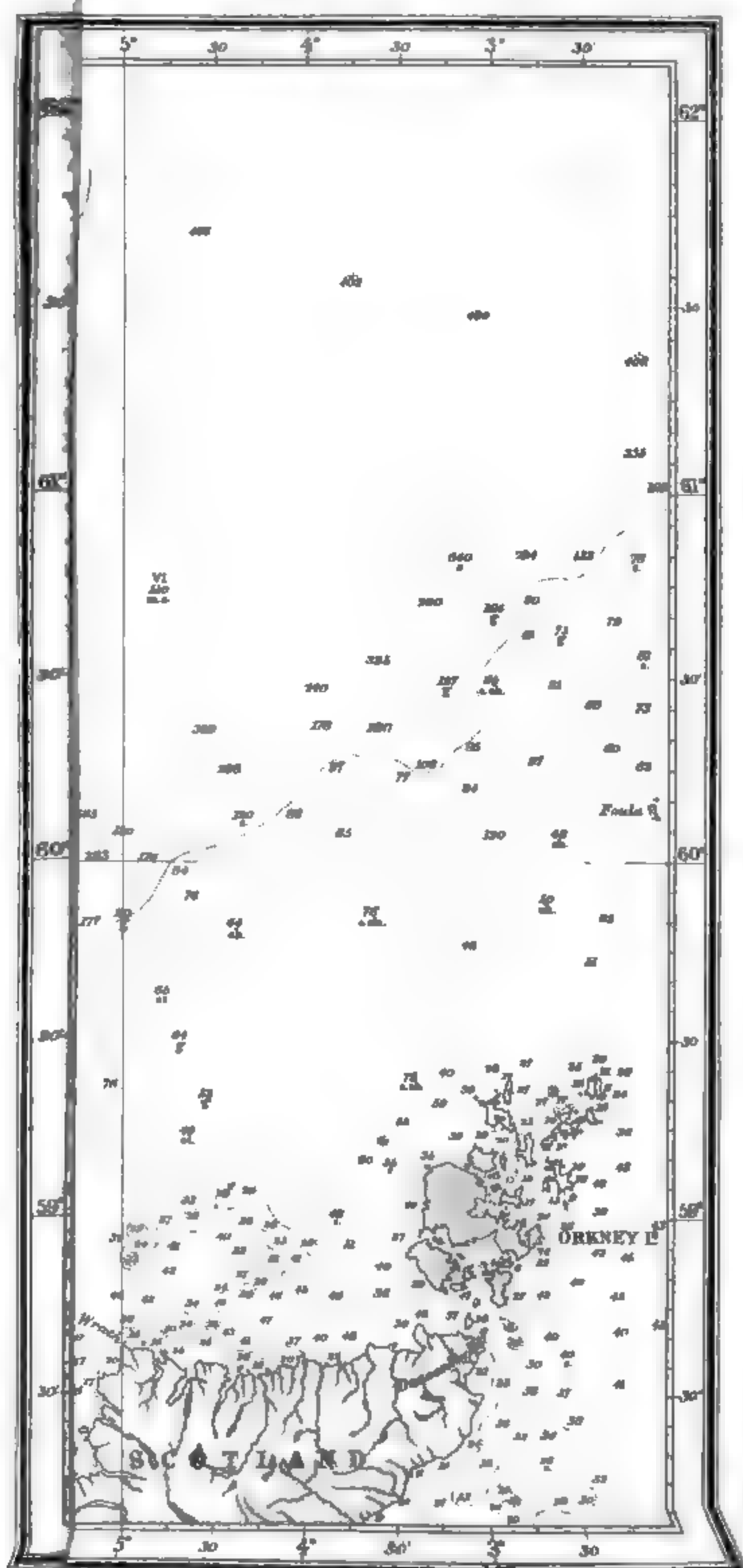
2. The Surveying-ship 'Lightning' was assigned for the service, and was furnished with a "donkey-engine," and with all other appliances required for the work, together with the most approved Sounding-apparatus* and Thermometers. The vessel was placed under the charge of Staff-Commander May, who had been much engaged in exploratory service elsewhere; and the instructions given to him were so framed as to enable him to carry out my wishes in every practicable way.

3. I was accompanied by my friend Professor Wyville Thomson, with whom the idea of this inquiry had originated †, and to whose zealous and efficient cooperation I have been greatly indebted in the prosecution of it. His large previous experience in Dredging-operations, and his extensive knowledge of the Marine Fauna, not merely of Great Britain, but of the Scandinavian and Boreal provinces, have supplied much that would otherwise have been deficient on my own part; and he has shown himself ever ready to relieve me of the more laborious part of the work we had jointly undertaken. Although it has been deemed fitting that, as it was by me that the proposal for this inquiry was brought before the Royal Society, and as I was entrusted by the Admiralty with the direction of it, this Report of its proceedings should proceed from myself, I have the satisfaction of saying that it has the full concurrence of my able Coadjutor.—I was permitted to take with me one of my sons as an Assistant; and we were all three considered as in the Public Service, and liberally provided for accordingly.

4. It is with great pleasure that I am able to state that the results of

* The Sounding-apparatus which we employed was that known as "Fitzgerald's Sinker," and we found it to answer perfectly. It carries down a weight of either 56 lbs. or 112 lbs., which detaches itself on reaching the bottom, so that the sinker (the weight of which is itself small) can be brought up by a small line; and this sinker is provided with a scoop, which brings up a sample of the bottom in a wedge-shaped box furnished with a cover that falls down and closes it when it has struck the ground.

† See his Letter of May 30 in the Appendix.



Continued on Sheet 6.5a



our inquiries have been, in all essential particulars, fully as satisfactory as we had ventured to anticipate. The lateness of the date at which the Expedition started (its departure from Stornoway having been necessarily delayed until August 11th), and the consequent limitation of the time during which deep-sea dredging would be likely to be practicable, precluded the idea that the present inquiry could be more than tentative, anything like a systematic exploration of the Marine Zoology of the area we proposed to traverse being scarcely to be expected. In point of fact, during the *four weeks* which elapsed between our first departure from Stornoway and our return to it on September 9th, only *nine days* were available for dredging in the open ocean ; and on only *four* of these were we on a bottom exceeding 500 fathoms [914 mètres] in depth ; and in our second cruise of a week's duration, we only dredged once. Yet, as will hereafter appear, we have been enabled, by this very limited amount of work, not only to add many new and interesting facts to science, in regard both to the Physics and the Animal Life of the Ocean ; but also to correct serious errors which have been sanctioned by high authority, and to lay a definite foundation for more extended inquiries directed towards the solution of various general questions of the highest importance.

5. On the day after our first departure from Stornoway (August 12) we were met by a breeze from the N.E., so strong that, although a sounding was obtained in lat. $59^{\circ} 20' N.$ and long. $7^{\circ} 5' W.$, which indicated a depth of at least 500 fathoms [914 mètres], with a *minimum* temperature of 49° [$9^{\circ} \cdot 4$ Cent.], the temperature of the surface-water being $54\frac{1}{2}^{\circ}$ [$12^{\circ} \cdot 5$ Cent.], any attempt to dredge was out of the question.

6. This breeze lasted with considerable force for three days, during which, being compelled to lie-to under canvas, we drifted to the northward of the deep water ; our first soundings after its abatement (August 15th) giving depths of 229 and 164 fathoms [419 and 300 mètres] respectively, with a *minimum* temperature of 48° [$8^{\circ} \cdot 9$ Cent.], the temperature of the surface-water being 54° [$12^{\circ} \cdot 2$ Cent.]. As we were then approaching the Faroe Banks, we considered it expedient to devote a couple of days to the examination of the distribution of Animal Life at these comparatively moderate depths, and then to proceed to the Faroe Islands, reserving the deeper water for our return voyage.

7. The average depth of the Faroe Banks is about 60 fathoms [110 mètres], and their *minimum* temperature was found to be about 50° [$10^{\circ} \cdot 0$ Cent.] when the temperature of the surface was 53° [$11^{\circ} \cdot 6$ Cent.]. The character of the Marine Invertebrate Fauna of this region exhibited the admixture of British and of Boreal types, which might be expected from its temperature and geographical position, the former decidedly predominating. The common *Ophiocoma rosula* of our own shores (*Ophiothrix fragilis* of Müller and Troschel) presents itself in very great abundance, and probably furnishes an important part of the food of the Cod which frequent these banks.

8. We reached Thorsaven on the morning of August 17th ; and, as the weather was then fine, we applied ourselves without delay to the exploration of the Fiords in its vicinity, using for this purpose the boats of the country, with native boatmen, whose knowledge of the tides and currents was indispensable to us. Unfortunately the weather again became so unfavourable as to prevent us from extending our inquiries to more distant localities, at the same time that the low state of the barometer rendered it inexpedient to put to sea again for the prosecution of our special object *. We found, however, that the Shells of the straits and fiords of the Faroes had been carefully collected by Sysellman Müller, who has long been in the habit of availing himself of the opportunities for dredging afforded by his official visits to different parts of the group ; and that a List of the Mollusca found in them has been recently published by Dr. O. A. L. Mörch †. The result of our own dredgings, taken in connexion with the information obtained from these sources, leads us to believe that further exploration in this locality is not likely to bring out facts of any special interest. The tides and currents in the Straits between the islands are so strong as to render the deepest parts of the mid-channels (in which alone could any novelty be anticipated) an unsuitable habitation for Marine Invertebrata ; and in the long narrow fiords which extend from these between the elevated ridges of Trap that traverse the interior of the islands the water is seldom of any considerable depth, and probably contains a large admixture of fresh water from the almost continuous rainfall which here prevails. The general character of the Marine Zoology of the Faroes, as of their adjacent banks, seems to be just what might be expected from their position on the border between the British, Scandinavian, and Boreal provinces.

9. At the first indication of improvement in the weather, we left Thorsaven on the 26th of August, with the intention of reaching the deep channel which we expected to find lying E. and W., between the North of Scotland and the Faroe Banks, as soon as possible, and of exploring this channel as completely as we might be able. At the end of our first day of steaming southwards, however, we encountered a gale from the S.W., in the course of which the barometer fell to 29 inches, and which was severe enough to do much damage to our ship ; and it was not until the afternoon of August 29th that, after lying-to for nearly three days under canvas and drifting to the N.E., we were able to obtain a Sounding in lat. $60^{\circ} 45'$ and long. $4^{\circ} 49'$. This gave us a depth of 510 fathoms [933 mètres] ; and the two thermometers sent down with the

* We learned on our return home that heavy gales had been experienced at this date in British seas.

† *Faunula Molluscorum Insularum Færoënsium*. Beretning om de hidtil fra Færoerne bekjendte Bløddyr : Af O. A. L. Mörch (Aftryk af Naturhistorisk Forenings Vidensk. Meddel. Nos. 4-7, 1867. Kjöbenhavn).—Dr. Mörch gives a very elaborate comparative Table of the distribution of the Mollusca in Greenland, Iceland, the Faroes, Scotland, England, and Denmark.

lead gave a *minimum* of 33° [$0^{\circ}\cdot5$ Cent.] and $34\frac{1}{2}^{\circ}$ [$1^{\circ}\cdot4$ Cent.] respectively, the temperature of the surface-water being 52° [$11^{\circ}\cdot1$ Cent.].

10. This very remarkable indication was fully confirmed the next morning, when we sounded again in lat. $60^{\circ} 7'$ and long. $5^{\circ} 21'$, and found the depth to be 500 fathoms [914 mètres], and the *minimum* temperature, as given by the mean of three thermometers* (showing $31\frac{1}{2}^{\circ}$ [$-0^{\circ}\cdot2$ Cent.], 32° [0° Cent.], and 33° [$0^{\circ}\cdot5$ Cent.] respectively), to be $32^{\circ}\cdot2$ [$0^{\circ}\cdot1$ Cent.], the temperature of the surface-water being 51° [$10^{\circ}\cdot5$ Cent.].

11. We here for the first time had an opportunity of working our Dredge at this great depth, and found no difficulty in doing so. The bottom consisted of sand and stones; and it is important to remark that the same kind of bottom was met with in all our subsequent soundings and dredgings in the "cold area" (§§ 12–14).—As might have been anticipated from the extraordinary reduction of the Temperature, there proved to be a comparative scantiness of Animal life; and of the forms which did present themselves, several belonged to the Boreal Fauna. Still there were examples of several different groups; and there was not that predominance of low forms which some have supposed to characterize the Fauna of great depths. Indeed the *Rhizopoda*, of which we afterwards encountered an extraordinary development at the like depth, but in a much warmer temperature, were almost entirely absent. It is worthy of note that a specimen of *Astropecten* of a bright red colour came up adherent to the dredge-line at a distance of 250 fathoms [457 mètres] from the dredge, about 1200 fathoms [2195 mètres] of line being out. As this animal is entirely unprovided with swimming-organs, and was found to be of such specific gravity as to sink immediately when placed in a jar of sea-water, it can scarcely have been taken up anywhere else than from the sea-bottom; and if this be admitted, it is obvious that at least 250 fathoms [457 mètres] of the dredge-line must have been lying on that bottom. Not only on many subsequent occasions did *Ophiurida* come up on the like part of the dredge-line, but in our last dredging (§ 19), from a depth of 650 fathoms [1189 mètres], there came up attached to it, at a distance of about 50 fathoms [92 mètres] from the dredge, two pieces of a *Siliceous Sponge*, which most assuredly could not have been drawn from any other source than the sea-bottom†, and which included many small living *Ophiurida*.

* It had been our intention to make a careful comparison of each of these Thermometers with an accurate standard on our return, and thus to have determined with greater precision the temperatures they respectively indicated; but two of them were unfortunately lost in a subsequent Sounding (§ 19).

† From this it is obvious that the Dredge-rope, so far from buoying up the Dredge, must effectually assist in sinking it, especially when the rope has been solidified by previous repeated immersions at great depths. I find the specific gravity of a portion of our dredge-rope, which has been thus subjected to a pressure of 118 atmospheres, to be 1347, that of Sea-water being about 1029. In our earlier dredgings, we attached one or two couples of 12-lb. shot to the dredge-line at a short distance from the dredge, so as

12. The weather again interfered with the prosecution of our inquiry, which had now become of most unexpected interest; but we were able on the morning of September 1st to obtain a Sounding, in lat. $60^{\circ} 10'$ and long. $5^{\circ} 59'$, which fully confirmed our previous observations. The depth was here 550 fathoms [1006 mètres], and the *minimum* temperature indicated by the mean of two thermometers* (which stood at $31^{\circ} \cdot 7$ and $32^{\circ} \cdot 5$ respectively) was 32° [0° Cent.], the surface-temperature being 53° [$11^{\circ} \cdot 6$ Cent.]. There was, however, too much wind for dredging on that day.

13. On the following day (Sept. 2), in lat. $60^{\circ} 24'$ and long. $6^{\circ} 38'$, our Sounding gave us a depth of only 170 fathoms [311 mètres]; but even at this depth we found, with a surface-temperature of 52° [$11^{\circ} \cdot 1$ Cent.], a *minimum* temperature, indicated by the mean of two thermometers† (which stood at $41\frac{1}{2}^{\circ}$ and 42° respectively), of $41\frac{3}{4}^{\circ}$ [$5^{\circ} \cdot 4$ Cent.],—that is, about 6° [$3^{\circ} \cdot 3$ Cent.] lower than the minimum temperature we had found at a like depth when approaching the Faroe Banks (§ 6), and 8° [$4^{\circ} \cdot 4$ Cent.] lower than that we subsequently encountered at the like depth when approaching the north coast of Scotland (§ 17). Our Dredgings here afforded evidence of a great abundance and variety of Animal life, Norwegian forms being mingled in a very marked manner with British. In particular we obtained a large number of specimens of *Terebratula cranium* of unusual size, a beautiful delicately moulded arenaceous triradiate Foraminifer‡, and very large examples of a coarsely arenaceous Rhizopod closely corresponding with the *Lituola Soldanii* of the Silurian Tertiaries.

14. On the following day (Sept. 3) we again found ourselves in deep water, our Sounding, taken in lat. $60^{\circ} 28'$ and long. $6^{\circ} 55'$, giving a depth of 500 fathoms [914 mètres]. The *minimum* indicated by the mean of three thermometers (which registered $31\frac{3}{4}^{\circ}$, $33\frac{1}{2}^{\circ}$, and 34° respectively) was 33° [$0^{\circ} \cdot 5$ Cent.], the temperature of the surface being 51° [$10^{\circ} \cdot 5$ Cent.]. Here, again, our Dredgings gave the same general results as those of previous dredgings at the like depth and temperature (§ 11); and not only was our previous conclusion confirmed, that a pressure of 100 atmospheres is not incompatible with the existence of numerous and varied forms of Animal life, but we had the gratification of obtaining a specimen of the remarkable Echinoderm *Brisinga* (one of the Norwegian types specially mentioned in Prof. Wyville Thomson's letter), part of the arms of which

to ensure its handles being kept down upon the ground, in the position requisite for the 'biting' of its edge; but we soon became satisfied that this is effectually done by the weight of the dredge-rope itself, when it has once been deeply submerged.

* A third Thermometer had been sent down; but as it registered a *minimum* of $36^{\circ} \cdot 2$ [$2^{\circ} \cdot 3$ Cent.], we thought it fair to presume that its index had not been carried down as far as the real minimum—a circumstance of frequent occurrence.

† Our third Thermometer stood on this occasion at 45° [$7^{\circ} \cdot 2$ C.]; and its reading has not been taken into account, for the reason stated in the preceding note.

‡ This we believe to be the *Rhabdammina abyssorum* of Sars; but as no description of the type has yet (so far as we can learn) been published by him, we are unable to identify it with certainty.

came up on the dredge-rope, whilst other portions, with the body (apparently belonging to one and the same individual), were found in the dredge.

15. The weather again occasioned for two days an interruption in our dredging; and it did not even permit the use of the proper deep-sea sounding-apparatus. But a sounding was taken on Sept. 5th, in lat. $60^{\circ} 30'$ and long. $7^{\circ} 16'$, with the ordinary deep-sea lead, which showed that there was no bottom at 450 fathoms [822 mètres], and gave a *minimum* temperature, indicated by the mean of two thermometers (which marked 33° and $35\frac{1}{2}^{\circ}$ respectively), of $33\frac{1}{4}^{\circ}$ [$0^{\circ} 7$ Cent.], the surface-temperature being 50° [$10^{\circ} 0$ Cent.].

16. It was then considered expedient to shape our course in a southerly direction; and on the morning of September 6th we found ourselves in lat. $59^{\circ} 36'$ and long. $7^{\circ} 20'$. Here a very careful Sounding gave a depth of 530 fathoms [969 mètres]; and the *minimum* temperature indicated by the mean of three thermometers (which registered 47° , $47\frac{1}{2}^{\circ}$, and $47\frac{1}{2}^{\circ}$ respectively) was $47\frac{1}{3}^{\circ}$ [$8^{\circ} 5$ Cent.], the surface-temperature being $52\frac{1}{2}^{\circ}$ [$11^{\circ} 4$ Cent.]. This result fully confirmed that obtained by our first less satisfactory sounding in nearly the same locality (§ 5), which the low temperatures subsequently obtained with such uniformity in like depths elsewhere had led us to doubt.—We were able on this day to obtain several good casts of the Dredge, the results of which proved of extraordinary interest. The bottom consisted of a bluish-white tenacious mud, containing but a small admixture of the *Globigerinæ* so abundantly obtained by previous soundings from various parts of the sea-bottom of the North Atlantic. Imbedded in this mud there came up an extraordinary collection of *Siliceous Sponges*, of new and most remarkable forms; and with these was associated the *Hyalonema Sieboldii*, which appeared to us clearly referable to that Family. The *Rhizopods* found in this mud were scarcely less interesting; for besides numerous specimens of the typically triradiate *Rhabdammina abyssorum* (?), presenting a varied range of forms, another large group of gigantic coarsely arenaceous bodies presented themselves, of the most varied shapes, apparently referable to the *Astrorhiza limicola** as their fundamental type, together with a large and perfect living specimen of *Cristellaria*, closely resembling that common in the Sicilian Tertiaries, and a *Cornuspira* of extraordinary size. With these lower forms, our dredgings on this bottom brought up a considerable variety of higher types, *Zoophytes*, *Echinoderms*, *Mollusks*, and *Crustaceans*; among which may be mentioned, as of special interest, two specimens of *Rhizocrinus*, the small Apiocrinoid whose recent discovery by M. Sars on the coast of Norway (see Appendix) may be considered as having furnished a principal “motive” of our expedition, and a living *Oculina prolifera*, of which we had on previous occasions brought up only dead and worn specimens.—We thus obtained evidence of

* See Dr. Sandahl in ‘*Öfversigt af Vet. Akad. Förhandl.*’ 1857, p. 299.

the existence, not of a degraded or starved-out *residuum* of Animal life, but of a rich and varied Fauna, including elevated as well as humble types, at a depth of 530 fathoms [969 mètres]. This Fauna was essentially British in its general character, but included several types hitherto found only near the coast of Norway. Since it presented itself on the southern border of the deep channel intervening between the North of Scotland and the Faroe Banks, these types must henceforth be considered to appertain equally to the British province.

17. As it was necessary for us to continue our course towards Stornoway, we were not able to prosecute further inquiries in this interesting locality, as we should otherwise have been most glad to do ; and on the morning of September 7th, in lat. $59^{\circ} 5'$ and long. $7^{\circ} 29'$, a Sounding gave the comparatively small depth of 189 fathoms [345 mètres]. We found the *minimum* temperature, indicated by the mean of three thermometers (respectively marking $49\frac{1}{2}^{\circ}$, $49\frac{3}{4}^{\circ}$, and $49\frac{3}{4}^{\circ}$), to be here $49\frac{2}{3}^{\circ}$ [$9^{\circ} \cdot 8$ Cent.], the surface-temperature being 52° [$11^{\circ} \cdot 1$ Cent.]. Here our Dredge brought up almost exclusively the ordinary types of the northern shores of Scotland, the chief features of interest being the great abundance of *Cidaris papillata*, and the occurrence of *Antedon celticus* (*Comatula celtica* of Barrett), numerous specimens of which had been previously obtained off the coast of Ross-shire by Mr. J. Gwyn Jeffreys. As we approached the land, the contents of the dredge became altogether barren of animal life, probably on account of the "scour" of the strong currents and tides of this locality, and the stony character of its bottom. In the Minch (the channel between the Island of Lewis and the mainland) the dredge again brought up a considerable number of well-known North British forms ; and at one of our casts it came up full of mud, sticking in which was an extraordinary number of living specimens of *Pennatula*.

18. We arrived at Stornoway on the afternoon of September 9 ; and here Prof. Wyville Thomson was obliged to leave us, in order to attend the Meetings of the Commission on Science and Art Instruction, of which he is a member. As, however, the weather presented an unusually settled aspect, and as the results we had already obtained led me strongly to desire an opportunity of examining both the Temperature and the Animal life of waters still deeper than any we had hitherto sounded, it was thought by Captain May and myself that, notwithstanding the lateness of the season, it would be worth while to venture another short cruise in a westerly direction, where we knew, from soundings previously taken, that a depth exceeding 1000 fathoms (1829 mètres) is to be met with.—After refitting our ship and our dredging-apparatus at Stornoway, we left that harbour for a second time on September 14, and proceeded in a N.W. course, with the view of finding, in the latitude of the region which had given us a temperature of 32° [0° Cent.] at a depth of 500 fathoms [914 mètres], but at some distance to the westward, still deeper water, and possibly a still lower temperature (the freezing-point of sea-water being

27°·4 [—2°·55 Cent.]), and of then running southwards until we should find ourselves over the deep valley between the Western Hebrides and the Rockall Bank. In this valley we hoped, from our previous success in working the Dredge at upwards of 500 fathoms, to be able, if weather should permit, to demonstrate the practicability of examining by its means the distribution of Animal life at twice that depth.

19. After a very fine run of 140 miles in a N.W. direction from the Butt of Lewis, we took a Sounding on the morning of Sept. 15 in lat. 59° 59', long. 9° 15', and found at 650 fathoms [1189 mètres] a bottom of bluish-white unctuous mud, very like that from which we had brought up the Siliceous Sponges (§ 16). The *minimum* temperature here indicated by the mean of three thermometers (registering 45°, 46°, and 47½° respectively) was 46° [7°·7 Cent.], the surface-temperature being 53° [11°·6 Cent.]. As it was thus evident that we were in the warm, not in the cold area of bottom-temperature, we proceeded about 60 miles still further to the N.W., and on the morning of Sept. 16 we sounded in lat. 60° 38' and long. 11° 7'. The depth was here 570 fathoms [1043 mètres]; and the scoop of the Sounding-apparatus brought up an almost pure *Globigerina* sand. The *minimum* temperature indicated by two thermometers (registering 46½° and 47½° respectively) was 47° [8°·3 Cent.], the surface-temperature being 52°.—Still looking for deeper water and a lower temperature, we proceeded about 50 miles further in the same direction; and on the afternoon of that day took another Sounding in lat. 61° 2' and long. 12° 4', which gave a depth of 650 fathoms [1189 mètres]. On this occasion our Sinker and three Thermometers were unfortunately lost by the parting of the line in winding-up, so that we did not ascertain either the nature of the bottom or the *minimum* temperature; but as we had now reached a latitude far north of that of the cold depths we had previously traversed (being nearly that of the southern end of the Faroe group), we deemed it inexpedient to proceed further in this direction; and a cast of the Dredge was therefore taken at this point, the depth being greater by 120 fathoms than any at which we had previously worked it. We found no difficulty in this operation, notwithstanding that the dredge was loaded with about 2½ cwt. [127 kilog.] of whitish grey mud, of peculiar viscosity, brought up from a depth (3900 feet) nearly equal to the height of the highest mountains in Great Britain. At some 50 fathoms [92 mètres] from the dredge, two whitish tufts were seen on the dredge-rope; and these proved to consist of portions of a Siliceous Sponge, quite free from the mud with which all the specimens previously obtained had been infiltrated. As it is obvious that these specimens must have been detached by the dredge-rope in its passage over the surface of the mud (§ 11), it seems clear that these Sponges, in part at least, project above that surface, which the infiltrated condition of those previously obtained had caused us to doubt. On separating the different parts of the large mass of mud brought up by the dredge, we found it to be *everywhere* traversed by fibres, which

proved to be long siliceous Sponge-spicules; and our subsequent examination of these has shown them to be the *root-fibres* of Sponges, the *bodies* of which have a siliceous framework of very different structure. As it thus appears that these Siliceous Sponges, when growing on the surface of the mud, send root-fibres (so to speak) far and wide into its substance, the idea previously suggested by Prof. Lovén*, that the elongated flint-rope of *Hyalonema Sieboldii* is in reality the mud-imbedded stem, *supporting* the Sponge with which it is connected, instead of being *implanted* in the Sponge and supported by it (which is the commonly received opinion), seems the more likely. This idea is thought probable by Prof. Wyville Thomson, who has already paid great attention to the whole group†, and by whom all the new forms we have obtained will hereafter be fully described.—Entangled among the fibres of the Sponge were found several small *Ophiocomæ*, *Polyzoa*, *Crustacea*, and tubicolar *Annelida*, the tubes of the last being for the most part composed of *Globigerinæ* cemented together, frequently in a most regular and beautiful manner. The only living testaceous Mollusk that presented itself was a small specimen of *Terebratula cranium*. Imbedded in the mud were found a specimen of *Kophobelemnon Mulleri* (a type allied to *Pennatula*) in full life, and two headless stems of *Rhizocrinus*, the perfectly fresh aspect of which leads me to believe that they must have grown on the spot, and have been mutilated in the sifting of the mud in which they were imbedded. This mud contained a considerable proportion (about 60 per cent.) of *Globigerinæ*, together with some remarkably large *Biloculinæ* and other *Milioline* forms.—The general character of this Fauna obviously bore a close relation to that of our previous dredging in a similar bottom; and though we cannot positively affirm the Temperature of that bottom to be the same, yet we have not merely the evidence of a previous Sounding in a locality not far removed from it, but also that of a Sounding subsequently taken in another locality further to the south, but nearly in the same longitude (§ 20), to this effect.

20. Being anxious now to proceed as quickly as possible to the region in which we knew that we should find much deeper water, we steered nearly due south, and on the morning of Sept. 17 reached lat. 59° 49' and long. 12° 36'. Here a Sounding gave us a depth of 620 fathoms [1134 mètres], with a bottom of white mud very similar to that of our last dredging. The *minimum* temperature, as shown by the mean of two ther-

* See his description of *Hyalonema boreale* in 'Öfversigt af K. Vetenskaps Akademiens Förhandlingar,' 1868, p. 105; translated in 'Annals of Natural History,' Fourth Series (1868), vol. ii. p. 81.—Dr. J. E. Gray, whilst still maintaining that the "flint-rope" is a Zoophytic product, and that the Sponge with which it is connected is parasitic, has also come to the conclusion that the brush-like termination serves as the root implanted in mud, above which the Sponge is borne. (See Ann. of Nat. Hist., Fourth Series, vol. ii. p. 272.)

† See his Paper on the Vitreous Sponges, in 'Annals of Natural History,' Fourth Series, vol. i. (1868), p. 114.

mometers (registering $45\frac{1}{2}^{\circ}$ and $46\frac{1}{2}^{\circ}$ respectively), was 46° [$7^{\circ}\cdot 7$ Cent.], the temperature of the surface being 52° [$11^{\circ}\cdot 1$ Cent.].

21. Still proceeding southwards, we reached in lat. $58\frac{1}{2}^{\circ}$ the locality in which we hoped, from soundings previously made and recorded, to be able to extend our inquiries to greater depths; but unfortunately a breeze had now set in from the N.E., which was strong enough to prevent us not only from dredging but even from sounding; and this breeze freshened on the night of Sept. 19 to a gale, which made it prudent to seek the shelter of the land by running to the eastward. Notwithstanding a partial abatement on the afternoon of the next day, it was considered by Capt. May that, having due regard to the uncertain aspect of the weather, to the state of the barometer, and to the season of the year, as well as to the fact that the time assigned by the Admiralty for our remaining at sea was on the point of expiring, it would not be prudent to hold on as we were, for the slight chance of being able to accomplish our object. Our course was therefore directed to Oban, which we reached on the afternoon of Sept. 21 *.

General Results.

Before proceeding to sum up the general results of our inquiries, and to indicate the conclusions to which these seem to point, I think it desirable to give a brief notice of the researches of those who had preceded us in the same line of inquiry.

The earliest instance I have been able to find in which living Animals were brought up from great depths in the Ocean, occurred in the Arctic Expedition (1818) of Captain (afterwards Sir John) Ross, and is mentioned in the narrative of his 'Voyage of Discovery' †. General Sabine, who was a member of that Expedition, has been kind enough to furnish me with the following more ample particulars of this occurrence:—"The ship sounded in 1000 fathoms, mud, between one and two miles off shore (lat. $73^{\circ} 37'$ N., long. $75^{\circ} 25'$ W.); a magnificent *Asterias caput-medusæ* was entangled by the line and brought up with very little damage. The mud was soft and greenish, and contained specimens of *Lumbricus tubicola*.' So far my written journal; but I can add, from a very distinct recollection, that the heavy deep-sea weight had sunk, drawing the line with it, *several feet* into the very soft greenish mud, which still adhered to the line when brought to the surface of the water. The Starfish had been entangled in the line so little above the mud, that fragments of its arms, which had been broken off in the ascent of the line, were picked out from amongst the mud."

It hence seems indubitable that the *Asterias* (*Astrophyton*) and the Tubicular Annelids were brought up *from the bottom*; and the only doubt

* This gale, being from the East, was but little felt on the West coast of Scotland; but we afterwards learned that it had done much damage on the East coast.

† Vol. i. p. 251, and Appendix, vol. ii. p. 178.

that can fairly be thrown upon the value of this observation has reference to the precise depth indicated by the Sounding, this having been made according to the old method now abandoned as unreliable. The circumstances under which this sounding was taken, however, render it probable that the actual depth was not much less than that recorded.

In another Sounding, in calm water, and with a smooth sea (lat. $72^{\circ} 23'$ N., long. $73^{\circ} 7'$ W.), a depth of 1050 fathoms was obtained with great precision; and a small Starfish was found attached to the line below the point marking 800 fathoms.

The subsequent explorations of Prof. Edward Forbes*, on which he founded the opinion that a *zero* of animal life would be found at 300 fathoms [548 mètres], did not themselves go deeper than 230 fathoms [420 mètres]; yet his high authority on questions of this nature caused his opinion to be very generally adopted, alike by Zoologists, Physical Geographers, and Geologists.

The fallacy of Prof. E. Forbes's assumption, however, was demonstrated by the results of Dredgings carried on in Sir James Ross's Antarctic Expedition, at depths of from 270 to 400 fathoms, which yielded evidence of great abundance and variety of Animal life between those depths. Dr. J. D. Hooker has kindly placed in my hands some extracts from his Journal, which give much fuller particulars of these results than are to be found in Sir James Ross's Narrative†.

On the 28th of June, 1845, the ill-fated Mr. Harry Goodsir, who was a member of Sir John Franklin's expedition, obtained in Davis's Straits, from a depth of 300 fathoms, "a capital haul,—*Mollusca, Crustacea, Asterida, Spatangii, Corallines, &c.*†" The bottom was composed of very fine green mud, apparently corresponding to that mentioned by General Sabine.

I am not aware that between this date and that at which the researches of MM. Sars commenced, any Dredging was carried on at depths exceeding those now specified; and the additions to our knowledge of the Life of the deep sea, with one remarkable exception to be presently noticed (p. 182), were made through the instrumentality of the improved Sounding-apparatus, which brings up a specimen of the superficial deposit (of whatever nature this may be) covering the sea-bottom, with such Animals as it may meet

* "Report on the Mollusca and Radiata of the Egean Sea, and on their distribution considered as bearing on Geology;" in Report of the British Association, 1843, p. 130.

† 'Voyage of Discovery and Research in the Southern and Antarctic Regions, during the Years 1839–1843,' vol. i. p. 207, and Appendix, p. 334.—It is much to be regretted that the specimens obtained should never have been systematically catalogued, and that the many novelties which presented themselves (among them a *Pycnogonid* twelve inches across) should not have been described. The specimens, with drawings made at the time by Dr. Hooker, were kept by Sir James Ross, with a view to their publication; but he died without carrying that intention into effect; and neither specimens nor drawings are now recoverable.

† See the 'Natural History of the European Seas,' by Prof. E. Forbes and R. Godwin-Austen 1859, p. 51.

with on the spot on which it drops. This method of examination must obviously be very inferior to Dredging in collecting-power; nevertheless it has yielded some very important results.

In the year 1855, Prof. Bailey (of West Point, U.S.) published a "Microscopic Examination of Deep Soundings from the Atlantic Ocean"*, between lat. $42^{\circ} 4'$ and $54^{\circ} 17'$ North, and long. $9^{\circ} 8'$ and $29^{\circ} 0'$ West, and at depths of from 1080 to 2000 fathoms. He stated that "none of these soundings contain a particle of gravel, sand, or other recognizable Mineral matter; and that they are all made up of the shells of *Globigerinæ* and *Orbulinæ*, with a fine calcareous mud derived from the disintegration of those shells, containing a few siliceous skeletons of *Polycystina* and spicules of *Sponges*." Connecting these results with those furnished by previous Soundings in the western portions of the Atlantic, Prof. Bailey inferred that with the exception of a spot near the bank of Newfoundland, in which the bottom at 175 fathoms was found to be made up of quartzose sand without any traces of organic forms, "the bottom of the North Atlantic Ocean, so far as examined, from the depth of about 60 fathoms to that of 2000 fathoms, is literally nothing but a mass of microscopic shells;" and he explicitly likened this deposit to the Chalk of England and the Calcareous Marls of the Upper Missouri. After stating that examination of samples of ocean-water, taken at different depths in situations in close proximity to the places where the soundings were made, yielded no trace of *Foraminifera*, he concludes with the following questions:—"Do they live on the bottom at the immense depths where they are found, or are they borne by submarine currents from their real habitat? Has the Gulf-stream any connexion, by means of its temperature or its current, with their distribution?" Upon these questions Prof. Bailey does not seem ever to have given a decided opinion; although he inclined to the belief that the *Globigerinæ* and *Orbulinæ* had *not* lived on the bottom where they were found, but had either been transported thither by currents, or had lived nearer the surface of the sea, and had fallen to the bottom after death. On the other hand, Prof. Ehrenberg, to whom specimens of these Soundings were forwarded, expressed his conviction (based on the condition of the organic substance contained in the cavities of the shells) that these *Foraminifera* *had* lived on the bottom from which they were brought up.

Similar conclusions regarding the extensive diffusion of *Globigerinæ* over the deep-sea bottom of the North Atlantic were drawn by Prof. Huxley from his examination of the Soundings brought up by Lieut.-Commander Dayman, from depths of from 1700 to 2400 fathoms†. Of the whole mass of the fine muddy sediment of which these soundings consisted, it is estimated by Prof. Huxley that 85 per cent. consisted of *Globigerinæ*; 5 per cent. of other *Foraminifera*, of, at most, not more than four or five

* Quarterly Journal of Microscopical Science, vol. iii. (1855) p. 89.

† Deep-sea Soundings in the North Atlantic Ocean, between Ireland and Newfoundland, made in H.M.S. 'Cyclops,' in June and July 1857.

species; and the remaining 10 per cent. partly of Siliceous organisms (*Diatoms* and *Polycystina*), partly of mineral fragments, and partly of the very minute granular bodies designated by Prof. Huxley *Coccoliths*. These granules he described as apparently consisting of several concentric layers surrounding a minute clear centre, and looking at first sight somewhat like single cells of the plant *Protococcus*; but as they are rapidly and completely dissolved by dilute acids, their composition cannot be organic. With reference to the question whether the *Globigerinæ* actually live at these depths, Prof. Huxley says, "The balance of probabilities seems to me to incline in that direction. And there is one circumstance which weighs strongly in my mind. It may be taken as a law that any genus of animals which is found far back in time is capable of living under a great variety of circumstances as regards light, temperature, and pressure. Now the genus *Globigerina* is abundantly represented in the Cretaceous epoch, and perhaps earlier" (*op. cit.* p. 67).

The results obtained by Prof. Bailey and Prof. Huxley, in regard to the prevalence of *Globigerinæ* over a large part of the sea-bottom in the North Atlantic Ocean, were confirmed and extended by the observations of Dr. Wallich, made during the voyage of the 'Bull-dog' in 1860; and as he was able to examine the condition of the *Globigerinæ* when freshly brought up, his testimony furnishes an important corroboration of Prof. Ehrenberg's conclusion. "The *Globigerinæ*," he says*, "have never been detected free-floating in any number in deep, or forming deposits in shallow waters; a considerable proportion of those met with in deep-sea deposits exhibit every appearance of vitality; and their maximum development is associated with the presence of the Gulf-stream, but only through the operation of collateral conditions prevailing at great depths below the current itself." But in addition, the 'Bull-dog' sounding-line brought up a cluster of *Ophiocomæ* attached to a portion of it which had lain on the bottom at a depth of 1260 fathoms; and *Globigerinæ* were found, with other matters, in their stomachs. Further, in various localities, at depths ranging from 871 to 1913 fathoms, tubes of small *Tubicular Annelids* were brought up; and some of these were found to be composed of *Globigerina*-shells cemented together, whilst others were made up of an admixture of Sponge-spicules and minute Calcareous débris. Lastly a living *Serpula*, *Spirorbis*, and a group of *Polyzoa* were brought up from a depth of 680 fathoms, and a couple of living *Amphipod Crustacea* from a depth of 445 fathoms. "Taking into consideration the arguments adduced to prove that the conditions which prevail on the deep-sea bed are not incompatible with the maintenance of animal life, and the extreme improbability that the creatures heretofore discovered at great depths are merely exceptional or accidental examples, it will, I think, be conceded that the presence of a living Fauna in the deeper abysses of the ocean has been fully established" †.

Dr. Wallich's just conclusions have not by any means commanded the

* The North-Atlantic Sea-Bed, p. 147.

† Ibid. p. 148.

universal assent of Naturalists. It is still urged* that the *Globigerinæ* lived at or near the surface, and that they only fell to the bottom after death. And it has been thought by many to be more probable that the *Ophiocomæ* had been entangled by the Sounding-line during either its descent or its ascent through the water, than that they had lived on the bottom. Our Dredge, however, having brought up, from depths of 530 and 650 fathoms, abundance of living *Globigerinæ* and *Ophiocomæ* entangled in the recesses of *Sponges*, with *Rotaliæ* attached by shell-substance to the spicules of these *Sponges*, the statements of Dr. Wallich with regard to these animals, which I had always myself regarded as probable, may now be considered as put beyond reasonable question †.

The general bearings of the facts thus brought to light, together with those furnished by the earlier observations of Sir John Ross and others, are fully and ably discussed by Dr. Wallich; but I must content myself with the following citation of his conclusions, referring to his Treatise for the arguments on which they rest:—

“Basing my arguments, then, on two facts which I venture to hope are unequivocally proved in the preceding pages, namely that highly organized creatures have been captured in a living condition at depths vastly exceeding those to which animal life had previously been supposed to extend, and that their presence, when captured, cannot be regarded as an accidental or exceptional phenomenon, it has been my endeavour to establish the following important propositions:—

“I. The conditions prevailing at great depths, although differing materially from those which prevail near the surface of the ocean, are not incompatible with the maintenance of animal life.

“II. Assuming the doctrine of single specific centres to be correct, the occurrence of the same species in shallow water and at great depths proves that it must have undergone the transition from one set of conditions to the other with impunity.

“III. There is nothing in the nature of the conditions prevailing at great depths to render it impossible that creatures originally, or through acclimatization, adapted to live under them should become capable of living in shallow water, provided the transition be sufficiently gradual; and hence it is possible that species now inhabiting shallow water may at some anterior period have been inhabitants of great depths.

* See Mr. Gwyn Jeffreys, in ‘Annals of Natural History,’ 4th series, vol. ii. (October 1868), p. 305.

† I had myself accepted Dr. Wallich’s inference in regard to the *Ophiocomæ* on the following grounds:—*first*, because, having often kept *Ophiocomæ* in an aquarium for several weeks together, I never saw them swim, and do not believe that they are capable of moving in any other way than by crawling over a solid surface; and *second*, because I know it to be their habit to cluster round a rope lying along the bottom they frequent,—the first I ever saw alive having been obtained for me by the Harbour-master of Plymouth, who sank a rope in a part of the Sound which he knew to be frequented by them, and drew it up again after some hours, covered with *Ophiocomæ*.

"IV. On the one hand the conditions prevailing near the surface of the ocean render it possible for organisms to subside after death to the greatest depths, provided every portion of their structure is freely pervious to fluid. On the other hand, the conditions prevailing at great depths render it impossible for organisms still constituted to live under them to rise to the surface, or for the remains of these organisms after death to make their appearance in shallow water.

"V. The discovery of even a single species living normally at great depths warrants the inference that the deep sea has its own special fauna, and that it has always had it in ages past; and hence that many fossiliferous strata, heretofore regarded as having been deposited in comparatively shallow water, have been deposited at great depth" *.

In 1861 the very important fact was made public by M. Alphonse Milne-Edwards†, that when the Submarine Telegraph-cable between Sardinia and Algiers was taken up for repair, several living Polyparies and Mollusks were attached to portions of it which had been submerged to a depth of from 2000 to 2800 mètres, or from 1093 to 1577 fathoms. Of these, some had been previously considered very rare, or had been altogether unknown; whilst others were only known in a fossil state as belonging to the Fauna of the later Tertiaries of the Mediterranean basin.

In the Swedish Expedition to Spitzbergen in 1861, a compact mass of clay was brought up from 1400 fathoms by the "M'Clintock apparatus," the temperature of the interior of which was found to be $32^{\circ}\cdot5$ [$0^{\circ}\cdot3$ Cent.], the temperature of the surface-water being $39^{\circ}\cdot2$ [4° Cent]. "Notwithstanding this low degree of warmth, there were found several marine animals of different types and classes—amongst others a moderately large Polyparium, probably belonging to the Hydroid class, a bivalved Mussel, some Tunicata attached to the Polyparium, and one Crustacean of bright colours" ‡.

Of the very important researches which have been subsequently carried on by Prof. Sars of Christiania and his Son, we knew little more, when we proceeded on our own cruise, than is stated in Prof. Wyville Thomson's letter (Appendix). But I have since learned from the recently published Report §, which Prof. Sars has been good enough to transmit to me, that their Dredgings have ranged between 200 and 450 fathoms, and that no fewer than 427 species have been collected within this range, which he classifies as follows:—

* North-Atlantic Sea-Bed, p. 155.

† Annales des Sciences Naturelles, sér. 4, Zool. tom. xv. p. 149.

‡ See a letter from Christiania, signed M. R. B., in the 'Athenæum' for December 7, 1861.—I have not been able to meet with further information in regard to this interesting occurrence.

§ Fortsatte Bemærkninger over det dyriske Livs Udbredning i Havets Dybder, af M. Sars. (Særskilt aftrykt af Vidensk.-Selsk. Forhandlinger for 1868.)

Protozoa	{	Rhizopoda	68
		Spongiæ	5
Cœlenterata	{	Anthozoa	20
		Hydrozoa	2
Echinodermata	{	Crinoidea	2
		Asterida	21
		Echinida	5
		Holothurida	8
Vermes	{	Gephyrea	6
		Annelida	51
Mollusca	{	Polyzoa	35
		Tunicata	4
		Brachiopoda	4
		Conchifera	37
		Cephalophora	53
Arthropoda	{	Arachnida	1
		Crustacea	105
			<hr/>
			427

Of these, 20 species of Rhizopoda, 3 of Echinodermata, 8 of Conchifera, 3 of Cephalophora, and 4 of Crustacea—in all 42—are recorded as having been found at 450 fathoms.

Shortly after our return, I learned that an exploration of the deep sea by means of the Dredge had been very successfully commenced by Count Pourtales, in connexion with the United States Coast Survey; and I have since received from Mr. Alexander Agassiz the following account of its results:—"He has dredged to 500 fathoms along quite a line of sections between Florida and Cuba; and under this pressure of nearly 100 atmospheres he has found *Echini*, *Starfishes*, *Ophiuridans*, *Crinoids*, *Corals*, many kinds of *Crustacea*, *Annelids*, *Mollusca*, *Molluscoids*, and, in fact, a Fauna as plentifully represented as along the most populous of our marine shore-fauna. It has been decided to send Mr. Pourtales again this winter; and with his former experience and additional equipment, we may look for grand results. The facilities placed at his disposal are very great; as his dredging-work is done in connexion with regular soundings carried on by the Survey of the Gulf-stream commenced by Mr. Bache and prosecuted by his successor Prof. Pierce"*.

Our own Dredgings, which have extended to a depth of 650 fathoms, are still the deepest of which I have any knowledge. They were accomplished without any serious difficulty, and with results fully as satisfactory as those of ordinary shore-dredging. And I have no doubt that similar dredges, worked by adequate engine-power, would answer equally well at those far greater depths, our knowledge of the living inhabitants of which has been hitherto limited (with the notable exception of the Mediterranean

* A fuller notice of these results will be found in Silliman's Journal for November 1868, and Annals of Nat. Hist. Jan. 1869.

cable, p. 182) to the few forms that have been brought up by the Sounding-apparatus*.

I. The collective results of these recent Dredgings have conclusively established the justice of the inference formerly drawn by Dr. Wallich from the more restricted data he had collected, as to the existence of a varied and abundant submarine Fauna, at depths which have been generally supposed to be either altogether *azoic*, or occupied only by Animals of very low type. And a complete disproof has thus been furnished of the doctrine, against which Dr. Wallich argued with great force, that a certain amount of bathymetric pressure must be prejudicial, if not absolutely fatal, to higher forms of Animal life.

In much that has been put forward upon this subject, two important considerations have been altogether ignored :—*first*, that pressure will not act upon an Animal whose body entirely consists of solid and liquid parts, in the same manner as it acts upon one that includes air-cavities ; and *second*, that as fluids press equally in *all* directions, an Animal immersed at any depth is just as free to move one part upon another, as it would be if living near the surface. The right point from which to look at this subject has long appeared to me to be the condition of a *drop of water*, conceived as carried down from the surface to a depth (say) of 1100 fathoms [2012 mètres], at which the pressure will be about 200 atmospheres, or 3000 lbs. [1360 kilogr.] upon the square inch. Let it be conceived that this drop is inclosed in a pellicle of the thinnest possible membrane, fitted only to separate it from the surrounding medium, but having in itself no power of resistance. Now it is obvious that this drop would maintain its *form*, whatever this may have originally been, entirely unchanged, being neither flattened-out into a plane, nor reduced to a sphere, by pressure to any amount which acts upon it equally in all directions ; while its *bulk* will only undergo reduction, under a pressure of 200 atmospheres, to the extent of less than one-hundredth. Next, let us suppose, instead of a drop of water contained within a pellicle, a particle of the semifluid “sarcodé” of which the body of a *Rhizopod* is composed ; in which the more liquid interior (*endosarc*) is contained by a more tenacious external layer (*ectosarc*), the contractility of which gives rise to continual changes of form, that are subservient to the movement of the creature from place to place, and also to the ingestion of its food. Now, it will be obvious to any one who follows out the law of fluid pressure in its application to an Animal of this simple constitution, that so long as these changes of *form* do not involve a change of *bulk*, pressure to any amount exerts no antagonizing influence ; so that its movements can be performed with the same freedom on the ocean-bottom as they can be near the surface. And, further, even

* It is reported that the Swedish Expedition, which has recently returned from Spitzbergen, has brought up a considerable number and variety of animals from depths of 2000 fathoms and upwards ; but whether these were obtained by the Dredge or by the ‘Bulldogsmaskinen,’ I have not yet learned.

when the bulk of the body is augmented by the ingestion of solid or liquid particles (say the reception of a zoospore of a Protophyte as food, or the filling of the "contractile vesicle" with water from without, which seems to be a sort of respiratory process), just as much pressure will be exerted by the superincumbent liquid in forcing those particles into the body as is exerted upon the exterior of the body in resisting its distension; so that here, again, the influence of that pressure will be practically *nil*.—If the actions of any purely aquatic Animal of more complex structure be looked at from the same point of view, I am persuaded that it will be found that they are not practically interfered with by fluid pressure to any amount,—such pressure not having any tendency to alter either the general form of the body, or the shape of its softest and most delicate parts, and not interfering in the least either with the movements of these parts one upon the other, or with the circulation of fluid in their interior, or with those molecular changes which are concerned in their nutrition.

II. The results we have obtained fully justify the confident expectation we had formed and expressed (see Appendix), on the basis afforded by the observations of M. Alphonse Milne-Edwards on the Mediterranean Cable, and by the results of the dredgings of M. Sars, jun., that the systematic exploration of the Ocean-bottom, at depths much greater than are usually to be found near land, would bring to light many forms of Animal life, either altogether new to science, or hitherto supposed to be limited to particular localities, or known only as belonging to a Geological epoch supposed to have terminated. For *one and the same cast of the dredge*, in the singularly productive locality specified in § 16, brought up specimens of the highest interest belonging to each of these categories; so that if we had been able, by remaining there even for a few days, to work this ground thoroughly, a much larger addition might have been fairly expected from this one spot,—still more, therefore, if the inquiry should be extended over that much wider area in which, as will presently appear, the like conditions prevail. For it must have been a strangely fortunate accident that brought together into our dredge so remarkable a collection of *Vitreous Sponges* and gigantic *Rhizopods* (many of them altogether new, and the rest known only as inhabitants of very distant localities,—with the *Rhizocrinus* previously obtained only in one spot more than 600 miles off), if these were not diffused tolerably abundantly as well as widely; and the probability that they are so rises almost to a certainty, when it is borne in mind that the next dredgefull that was obtained from a bottom similar both in character and in temperature, though at a depth of 120 fathoms greater, and at a distance of 200 miles in a straight line, showed distinct evidence of the prevalence of similar types (§ 19).

III. Our researches have conclusively established the existence of a *minimum Temperature** at least as low as 32° [0° Cent.] over a considerable

* It is obvious that any error in our Thermometers, arising from the pressure of

area, where the depth was 500 fathoms [914 mètres] and upwards; notwithstanding that the *surface*-temperature varied little from 52° [$11^{\circ}\cdot 1$ Cent.], alike in this region and in neighbouring areas of similar depth, in which the *minimum* temperature was only a few degrees beneath that of the surface. The current doctrine in regard to deep-sea temperatures may be considered to be that expressed by Sir J. Herschel (*Physical Geography*, 1861, p. 45) in the following terms:—"In very deep water all over the globe a uniform temperature of 39° Fahr. [4° Cent.] is found to prevail, while above the level, when that temperature is first reached, the ocean may be considered as divided into three great regions or zones—an equatorial and two polar. In the former of these, warmer, in the latter colder, water is found at the surface. The lines of demarcation are of course the two isotherms of 39° mean annual temperature." This doctrine, which is more fully and explicitly set forth by Dr. Wallich ('*The North-Atlantic Sea-bed*,' 1862, pp. 98, 99), rests, I believe, chiefly on the temperature-observations made in Sir James Ross's Antarctic Expedition, which were not inconsistent with the prevalent belief that *sea*-water, like *fresh* water, has its maximum density at this temperature, and that consequently water at 32° or 33° cannot underlie water at 39° . Several instances, however, had been previously recorded, in which temperatures below 39° had been observed. Thus Lieut. S. P. Lee, of the United States Coast Survey, in August 1847 found 37° below the Gulf-stream, at the depth of 1000 fathoms [1829 mètres], in lat. $35^{\circ} 26'$ N. and long. $73^{\circ} 12'$ W.; and Lieut. Dayman found the temperature at 1000 fathoms [1829 mètres] in lat. 51° N. and long. 40° W. to be $32^{\circ} 7'$ [$0^{\circ}\cdot 4$ Cent.], the surface-temperature being $54^{\circ}\cdot 5$ [$12^{\circ}\cdot 5$ Cent.]*. "At the very bottom of the Gulf-stream," says Lieut. Maury (*Physical Geography of the Sea*, 1860, p. 58), "when its surface-temperature was 80° [$26^{\circ}\cdot 6$ Cent.], the deep-sea thermometer of the Coast Survey has recorded a temperature as low as 35° [$1^{\circ}\cdot 6$ Cent.]. These cold waters doubtless come down from the north to replace the warm waters sent through the Gulf-stream to moderate the cold of Spitzbergen; for within the Arctic Circle the temperature at corresponding depths off the shores of that island is said to be only one degree colder than in the Caribbean Sea, while on the shores of Labrador and in the Polar Sea the temperature of the water beneath the ice was invariably found by Lieut. De Haven at 28° [$-2^{\circ}\cdot 2$ Cent.], or 4° below the melting-point of freshwater ice. Capt. Scoresby relates that on the coast of Greenland, in latitude 72° , the temperature of the air was 42° [$5^{\circ}\cdot 5$ Cent.], of the water 34° [$1^{\circ}\cdot 1$ Cent.], and 29° [$-1^{\circ}\cdot 6$ Cent.] at the depth of 118 fathoms"†. That there is no Physical improbability in the

100 atmospheres to which their bulbs were subjected, would prevent them from recording a *minimum* as low as the *actual* minimum; and it seems to us not at all improbable that the actual minimum may have been from 2° to 4° *lower* than the recorded minimum.—In any renewal of the inquiry, it will be of course desirable that the Thermometric apparatus used should be specially protected from this source of error.

* See Purdy on the Northern Atlantic Ocean, 12th edit., 1865, pp. 330 and 338.

† General Sabine has been kind enough to send me the following extract from his

existence of a stratum of sea-water at a temperature of 32° or even 28° below a stratum at 39° , is evident from the fact (which has been experimentally established beyond question *) that Sea-water, in virtue of its saline impregnation, *contracts continuously down to its ordinary freezing-point*, which is below 28° Fahr. And the existence of such strata, even in Equatorial regions, has been regarded by high scientific authorities † as proving the existence of deep currents bringing cold water from Polar Regions to replace the warmer water that is continually flowing, as (notably) in the Gulf-stream, from the Equatorial towards the Polar Regions, as well as to make good the immense loss which is constantly taking place by evaporation from the surface of Tropical seas ‡. To such an under-current, probably proceeding from the North or North-east, the low temperatures we

Journal of Capt. Ross's Voyage, which, if there was no error in the instrument employed, gives a lower temperature than any yet recorded :—" Having sounded, on Sept. 19, 1818, in 750 fathoms, the registering Thermometer was sent down to 680 fathoms; and on coming up, the index of greatest cold was at $25^{\circ}\cdot75$. Never having known it lower than 28° in former instances (even at a depth of 1000 fathoms, and at other times when close to the bottom), I was very careful in examining the Thermometer; but could discover no other reason for it than the actual coldness of the water."

* It is stated by M. Despretz, as the result of a series of carefully conducted experiments, that the *maximum* density of Sea-water cooled down continuously without agitation is at $-3^{\circ}\cdot67$ Cent., or $25^{\circ}\cdot4$ Fahr.; the freezing-point of Sea-water which is agitated being $-2^{\circ}\cdot55$ Cent., or $27^{\circ}\cdot4$ Fahr. See his "Recherches sur le Maximum de Densité des Dissolutions Aqueuses," in *Annales de Chimie*, 1833, tom. lxx. p. 54.

† This doctrine, long since explicitly stated by Humboldt (*Cosmos*, vol. i. p. 296), is thus set forth by Prof. Buff in his 'Physics of the Earth' (p. 194) :—" The water of the ocean at great depths has a temperature, even under the equator, nearly approaching to the freezing-point. This low temperature cannot depend on any influence of the sea-bottom. . . . The fact, however, is explained by a continual current of cold water flowing from the Polar regions towards the Equator. The following well-known experiment clearly illustrates the manner of this movement. A glass vessel is to be filled with water with which some powder has been mixed, and is then to be heated at bottom. It will soon be seen, from the motion of the particles of powder, that currents are set up in opposite directions through the water. Warm water rises from the bottom, up through the middle of the vessel, and spreads over the surface, while the colder and therefore heavier liquid falls down at the sides of the glass. Currents like these must arise in all water-basins, and even in the oceans, if different parts of their surface are unequally heated. The water that is cooled in the polar regions sinks and travels from the poles towards the equator, pushing away the warmer and lighter liquid from the bottom of the sea; itself to give way in turn, as it gets warm, to the colder water that follows after it. This continual flow of the water from the cold zones is replaced in a twofold manner. The warm water of the tropical seas, since it is the lightest, must spread itself north and south over the surface of the ocean, and thus gradually losing its heat, be carried to the polar regions. Between the tropics, too, evaporation goes on most vigorously, and a great part of the vapours formed fall again in rain and snow only in higher latitudes."

‡ A set of deep soundings, taken across the Arabian Sea, between Aden and Bombay, by Capt. Shortland, in H.M.S. 'Hydra,' has lately been received by the Hydrographer to the Admiralty, which give a line of bottom-temperature of $33\frac{1}{2}^{\circ}$ [$0^{\circ}\cdot8$ Cent.] at depths

observed between lat. 60° 45' and 60° 7', as shown in the following Table and the accompanying Map (for which I am indebted to the kindness of the Hydrographer to the Admiralty), may be pretty certainly attributed.

TABLE OF PLACES, DEPTHS, AND TEMPERATURES OF SOUNDINGS.
Warm Area.

No.	Latitude, N.	Longi- tude, W.	Depth, in Fathoms.	Temperature	
				at surface.	at bottom.
1	59 20	7 5	At least 500	54.5	49
2	60 32	9 10	164	54	48.5
3	60 31	9 18	229	54	48
4	60 44	8 45	72	54	49
5	61 1	7 48	62	53	50
12	59 36	7 20	530	52.5	47.3
13	59 5	7 29	189	52	49.3
14	59 59	9 15	650	53	46
15	60 38	11 7	570	52	47
16	61 2	12 4	650	—	—
17	59 49	12 36	6 0	52	46

Cold Area.

No.	Latitude, N.	Longi- tude, W.	Depth, in Fathoms.	Temperature	
				at surface.	at bottom.
6	60 45	4 49	510	52	33.7
7	60 7	5 21	500	51	32.2
8	60 10	5 59	550	53	32
9	60 24	6 38	170	52	41.7
10	60 28	6 55	500	51	33
11	60 30	7 16	At least 450	50	33.2

Of its northern limit we are not able to give any account; but about 50 miles to the southward we found the temperature at the same depth to be 15° higher [8°.3 Cent.]; and since the like temperature showed itself at even greater depths to the westward, between lat. 59° 59' and 60° 38', and inferentially (§ 19) as far north as 61° 2', at a distance of 175 miles from the most westerly point to which we traced this cold area, it may be presumed that this area was as limited in a *westerly* as we found it to be in a *southerly* direction. Here, therefore, within a short distance of the Northern Coast of Scotland, an opportunity is presented for determining with great precision the physical conditions of two opposing currents, having a difference of temperature of at least 15°. In such determination it

of 1800 fathoms and upwards, the surface-temperature being 75°. It seems impossible to account for this fact on any other hypothesis than that of a deep current from the Antarctic Polar region, which must have maintained this extremely low temperature throughout the vast course it has had to traverse.

would be very desirable to ascertain whether the *minimum* temperature is that of the *bottom* (a point of fundamental importance as regards the distribution of Animal life), or whether it is that of some intermediate stratum. The deep-sea Sounding-apparatus with which we were provided only allowed the attachment of the Thermometers to the extremity of the line; and it is *possible*, of course, that their *minimum* may represent, not the temperature of the sea-bottom, but that of some higher stratum. Independently, however, of the physical improbability (for the reason already stated) that Sea-water at 32° should overlies water of any higher temperature, which would be specifically lighter than itself, we have the evidence afforded by our Sounding in 170 fathoms (§ 13) within the cold area, that the temperature descends progressively with the depth; at first (as elsewhere observed) more rapidly, afterwards more slowly. And as this shallow bank is of very limited extent, and the bottom in its neighbourhood must become rapidly deeper, a careful examination of the bottom-temperature of its inclined sides at different depths would furnish satisfactory data on this point.

IV. A general comparison of the Faunæ of the different localities which we had the opportunity of examining seems to warrant the conclusion that the distribution of the Animal life of the seas beyond the Littoral zone* is more closely related to the *temperature* of the water than to its *depth*. The predominance of North British types, not merely on the southern but on the northern side of the deep valley which separates the Faroe Banks from the coast of Scotland, and in the *warm* area of the valley itself, the slight admixture of *exclusively* Scandinavian or Boreal forms even as far north as the Faroe Islands, the larger admixture of these on the shallow bank in the *cold* current, the still greater proportion of Boreal forms in the deeper and yet colder waters of that current, and (in most striking contrast with this) the presence of forms hitherto known only as inhabitants of the warmer temperate seas at the like depth in the *warm* area not many miles off,—all indicate the intimacy of the relationship between Geographical distribution and Temperature. The existence of Boreal types in the midst of an area whose surface-temperature is 52° [11°·1 Cent.], and whose bottom-temperature, even at 500 fathoms' [914 mètres] depth, is generally 47° or 48° [8°·3 or 8°·8 Cent.], is obviously a phenomenon parallel to the occurrence of Alpine plants at a high elevation on mountains within the tropics; and as every Botanist would regard such occurrence as having no relation to elevation *per se*, but only to eleva-

* The distribution of marine Animal life in the Littoral zone is affected by a great number of conditions, which place it in altogether a different category from that of the deeper seas. I am very glad to find our views on this point in harmony with those of my friend Mr. J. Gwyn Jeffreys. "The bathymetrical zones have been too much divided by Risso and subsequent authors. There are two principal zones, *littoral* and *submarine*; the nature of the habitat and the supply of food influence the residence and migration of animals, not the comparative depth of water."—*Annals of Natural History*, 4th ser. vol. ii. (1868) p. 303.

tion as affecting Temperature, so it is obvious that, with the evidence we are enabled to present of an abundant and varied Fauna at a depth of even 650 fathoms [1189 mètres], the Zoologist is fully justified in attributing the far different character of the Fauna we encountered at 500 fathoms [914 mètres] with a Temperature of 32° [0° Cent.] to that remarkable reduction.—Further, although the *nature of the bottom* has doubtless an important influence on the Animal life which it sustains, yet this very condition, as will presently appear, is itself dominated in great degree by Temperature.

V. The results of our Dredgings fully confirm the indications afforded by the specimens of the bottom previously brought up by the Soundings already noticed, in regard to the existence, on the sea-bottom of large areas of the North Atlantic, of a stratum of "calcareous mud," partly composed of living *Globigerinæ*, partly of the disintegrated materials of the shells of former generations, and partly of the "coccoliths" of Prof. Huxley (*loc. cit.*) and the "coccospheres" of Dr. Wallich *, with a greater or less admixture of other constituents. And they further indicate that the prevalence of this deposit is connected with a bottom-temperature of 45° and upwards, which, in latitudes above 56° , can scarcely be attributed to any other influence than that of the Gulf-stream. The examination which Prof. Huxley has been good enough to make of the peculiarly viscid mud brought up in our last dredging at the depth of 650 fathoms [1189 mètres], has afforded him a remarkable confirmation of the conclusion he announced at the recent Meeting of the British Association, that the coccoliths and coccospheres are imbedded in a living expanse of protoplasmic substance, to which they bear the same relation as the spicules of Sponges or of Radiolaria do to the soft parts of those animals. Thus it would seem that the whole mass of this mud is penetrated by a living organism of a type even lower, because less definite, than that of Sponges and Rhizopods; and to this organism Professor Huxley has given the name of *Bathybius* †. In what manner the materials for its protoplasm, as for that of the *Globigerinæ* which usually accompany it in larger or smaller proportion, are obtained, is a most perplexing problem. All the evidence we at present possess in regard to the alimentation of *Rhizopods*, leads to the belief that, in common with higher Animals, they depend upon the Organic Compounds previously elaborated by Vegetative agency under the influence of the light and heat of the Sun. But every form of Vegetable life that is visible to the naked eye seems entirely wanting at great depths in the ocean: and although this deposit is found by the Microscope to contain the siliceous *loricæ* of *Diatoms*, yet these do not present themselves in anything like the abundance that would be required for the nutrition of so large a mass of Animal life as that

* "Remarks on some novel Phases of Organic Life at great depths in the Sea," in 'Ann. of Nat. Hist.' ser. 3, vol. viii. (1861) p. 52.

† "On some Organisms living at Great Depths in the North Atlantic Ocean;" in Quart. Journ. of Microsc. Society, vol. viii. N.S. p. 203.

represented by the *Globigerina*-shells; and there appears good reason to regard them as rather representing Diatoms which have lived at or near the surface, and have only subsided to the bottom after death, than organisms which habitually live and grow in the ocean-depths. It may be that the *Bathybius* (which bears a very striking resemblance to the Rhizopod-like *mycelium* of the Myxogastric Fungi) has so far the attributes of a Vegetable, that it is able to elaborate Organic Compounds out of the materials supplied by the medium in which it lives, and thus to provide sustenance for the Animals imbedded in its midst. But to whichever of these two Kingdoms we refer it, there seems adequate reason for regarding this *Bathybius* as one of the chief instruments whereby the solid material of the Calcareous mud which it pervades is separated from its solution in the ocean-waters*.

In connexion with this subject it may be suggested, as a subject well worthy of experimental inquiry, to what depth the *Actinic* rays penetrate Sea-water in sufficient intensity to produce an appreciable effect on a highly sensitive surface. Certain it is that among the Animals brought up from great depths, bright colours are not wanting. This was noticed by Dr. Wallich in the case of the *Ophiocomæ* brought up from 1260 fathoms. And not only did the *Astropecten*, which came up on our dredge-line from 500 fathoms, at once attract attention by its bright orange-red hue, but the small *Annelids* which inhabited the Siliceous Sponge brought up from 650 fathoms were distinguished by the vividness of their red or green coloration.

VI. Our researches have brought out with remarkable force the resemblance between this Calcareous deposit and the great Chalk-formation, which had been previously pointed out by Prof. Bailey, Prof. Huxley, and Dr. Wallich, but more particularly by Mr. Sorby†, who identified the

* The discovery of this indefinite plasmodium, covering a wide area of the existing Sea-bottom, should afford a remarkable confirmation, to such (at least) as still think confirmation necessary, of the doctrine of the Organic origin of the Serpentine-Limestone of the Laurentian Formation. For if *Bathybius*, like the testaceous Rhizopods, could form for itself a shelly envelope, that envelope would closely resemble *Eozoon*. Further, as Prof. Huxley has proved the existence of *Bathybius* through a great range not merely of *depth* but of *temperature*, I cannot but think it probable that it has existed continuously in the *deep seas of all Geological Epochs*. And so far, therefore, from considering that the discovery of *Eozoonal Rock* in the Liassic or even in *Tertiary Strata*, would (as asserted by Profs. King and Rowney in a Paper recently presented to the Geological Society) be a *conclusive disproof* of its Organic origin, I am fully prepared to believe that *Eozoon*, as well as *Bathybius*, may have maintained its existence through the whole duration of Geological Time, from its first appearance to the present Epoch; and should be not in the least surprised at bringing it up from 1000 or 2000 fathoms, if I should be enabled to dredge at those depths. There must have been *deep seas* at all periods; and the considerations stated in Par. IX. show that the *continuity of Organic types* is perfectly consistent with *great local changes*. Of such continuity there is now ample evidence.

† "On the Organic Origin of the so-called Crystalloids of the Chalk," in 'Ann. of Nat. Hist.' ser. 3, vol. viii. (1861) p. 52.

"coccoliths" of Prof. Huxley and the "coccospheres" of Dr. Wallich with bodies observed in Chalk. While the *soundings*, on the nature of which this conclusion was based, could not indicate more than the existence of a mere *surface-layer* of this material, the fact that our large dredges came up completely filled with it, and the manner in which massive Siliceous Sponges had obviously been imbedded in it, clearly prove it to possess considerable thickness. The existence of this deposit over a very large area was marked out by our Dredgings at the extreme distance of 200 miles, and by several intermediate Soundings; and the variations in its character corresponded closely with those which present themselves in different parts of the same stratum of Chalk.

VII. But besides confirming the views already promulgated, as to the complete dependence of this Calcareous deposit on the enormous development of low forms of Organic Life, our researches also show that the area over which this deposit is being formed is peopled by a variety of higher types of marine Animals, many of which carry us back in a most remarkable manner to the Cretaceous epoch. Thus among Mollusca we have two *Terebratulidæ*, of which one at least (*Terebratulina caput-serpentis*) may be certainly identified with a Cretaceous species, whilst the second (*Waldheimia cranium*) may be fairly regarded as representing, if not lineally descended from, another of the types of that family so abundant in the Chalk. Among *Echinoderms* we have the little *Rhizocrinus*, that carries us back to the *Apiocrinite* tribe which flourished in the Oolitic period, and was until lately supposed to have had its last representative in the *Bourgettocrinus* of the Chalk, to which the *Rhizocrinus* presents many points of remarkable correspondence*. Among *Zoophytes*, the *Oculina* we met with in a living state seems generically allied to a Cretaceous type (*O. explanata* of Michelin). And the remarkable abundance of *Sponges*, which not improbably derive their nutriment from the protoplasmic substance that enters largely into the composition of the calcareous mud wherein they are imbedded (p. 190), is a preeminently conspicuous feature of resemblance.—We can scarcely doubt that a more systematic examination of the remarkable Formation at present in progress would place in a still stronger light the relationship of its Fauna to that of the Cretaceous period, since the specimens which our few dredgefuls contained can only be considered as a mere *sample* of the varied forms of Animal life which this part of the Ocean-bottom sustains. And if our notion of the intimacy of this relationship should be confirmed by further inquiry, it would go far to prove, what seems on general grounds highly probable, that the deposit of *Globigerina*-mud has been going on, over some part or other of the North-Atlantic sea-bed, from the Cretaceous epoch to the present time (as there is much reason to think that it did elsewhere in *anterior* Geological periods), this mud being not merely a Chalk-formation, but a continuation of *the* Chalk-

* See the recently published "Mémoires pour servir à la connaissance des Crinoides vivants," by Prof. Sars (Christiania, 1868).

formation ; so that *we may be said to be still living in the Cretaceous Epoch* *.

VIII. It can be scarcely necessary to point out in detail those various important applications of the foregoing conclusions to Geological Science, which will at once occur to every Geologist who endeavours to interpret the past history of our globe by the light of the changes it is at present undergoing. But this Report would not be complete without some notice of these.—In the first place, it may, I think, be considered as proved that no valid inference can be drawn from either the absence or the scantiness of Organic Remains in any unmetamorphosed sedimentary rock, *as to the depth at which it was deposited*. So far from the deepest waters being *azoic*, it has been shown that they may be peculiarly rich in Animal life. On the other hand, comparatively shallow waters may be almost *azoic*, if their temperature be low or their currents be strong ; and thus even littoral formations may show but few traces of the life that might be abundant on a deeper bottom at no great distance.—Again, it has been shown that two deposits may be taking place within a few miles of each other, *at the same depth and on the same geological horizon* (the area of one penetrating, so to speak, the area of the other), of which the Mineral character and the Fauna are alike different,—that difference being due on the one hand to the *direction of the current* which has furnished their materials, and on the other to the *temperature of the water* brought by that current. If our “cold area” were to be raised above the surface, so that the deposit at present in progress upon its bottom should become the subject of examination by some Geologist of the future, he would find this to consist of a barren Sandstone, including fragments of older rocks, the scanty Fauna of which would in great degree bear a Boreal character (§ 11) ; whilst if a portion of our “warm area” were elevated at the same time with the “cold area,” the Geologist would be perplexed by the *stratigraphical continuity* of a Cretaceous formation, including not only an extraordinary abundance of Sponges, but a great variety of other Animal remains, several of them belonging to the warmer Temperate region, with the barren Sandstone whose scanty Fauna indicates a widely different climatic condition, which he would naturally suppose to have prevailed at a different period. And yet these two conditions have been shown to exist *simultaneously*, at *corresponding depths*, over *wide contiguous areas* of the sea-bottom ; in virtue solely of the fact that one area is traversed by an *Equatorial* and the other by a *Polar* current †. Further, in the midst of the land formed by the elevation of the

* I think it due to my valued Colleague to state that this hypothesis (which I myself fully accept) entirely originated with him, having been foreshadowed in his first communication to me on the subject (Appendix).

† It may be said that the asserted existence of these Currents is a mere hypothesis, until an actual movement of water in opposite directions has been substantiated. But, as Prof. Buff has pointed out (p. 187, *note*), the existence of such deep currents is a

"cold area," our Geologist would find a hill some 1800 feet high, covered with a Sandstone continuous with that of the land from which it rises, but rich in remains of Animals belonging to a more temperate province (§ 13); and might easily fall into the mistake of supposing that two such different Faunæ, occurring at different levels, must indicate two distinct climates separated in time, instead of indicating, as they have been shown to do, two contemporaneous but dissimilar climates, separated only by a few miles horizontally, and by 300 fathoms vertically.—It seems scarcely possible to exaggerate the importance of these facts, in their Geological and Palæontological relations, especially in regard to those more localized Formations which are especially characteristic of the later Geological epochs. But even in regard to those older Rocks, whose wide range in space and time would seem to indicate a general prevalence of similar conditions, it may be suggested whether a difference of bottom-temperature, depending upon deep oceanic currents, was not the chief determining cause of that remarkable contrast between the Faunæ of different areas in the same Formation, which is indicated by the abundance and variety of the Fossils of one locality, and their scantiness and limitation of type in another; as is seen, for example, when the "Primordial Zone" of Barrande is compared with its equivalent in North Wales.—Further, in the case of those Calcareous deposits which owe their very existence to the vast development of Organisms that possessed the power of separating Carbonate of Lime from the ocean-waters, *temperature* may be pretty certainly assumed to be the chief condition, not merely of the character of the Animal remains which those formations may include, but of the very production of their solid material.

IX. How important a light is thrown by the facts we have brought into view on those changes in the Marine Fauna of any particular area, which cannot be referred to changes in its own geological condition, need scarcely be pointed out. As there must have been *deep seas* in all Geological epochs, so there must have been *varieties in Submarine Climate* at least as great as those we have discovered, depending upon those Equatorial and Polar Currents whose existence has been shown to be a Physical necessity. Hence it is obvious that since changes in the direction of such opposing currents must have been produced by any upward or downward movement of the sea-bottom (as in the areas of elevation and subsidence marked out by Mr. Darwin in our existing seas), a considerable modification, or even a complete reversal, of the Submarine Climates of adjacent areas might have been consequent upon alterations in the contour of the land, or in the level of the sea-bottom, *at a great distance*. The effect of such a modification of Temperature upon the respective Faunæ of these areas would probably depend upon the rate and degree of the change. If

necessary consequence of the difference of surface-temperature between Equatorial and Polar waters; and those who raise the objection are consequently bound to offer some other conceivable hypothesis on which the facts above stated can be accounted for.

rapid and considerable, it might cause the extinction over those areas of a large proportion of the species which inhabited them; whilst others would migrate in the direction of the temperature most congenial to them, and transfer to new localities those types which could no longer exist in their previous habitats,—thus establishing the *Colonies* of M. Barrande. If, on the other hand, such a change of Temperature were more gradual, the greater part of the species constituting the Faunæ of the areas over which it occurred might adapt themselves to it, undergoing such modifications in their structure and habits as might be considered sufficient to differentiate them specifically, whilst retaining so many characters of general similarity as to constitute “representative species” *.

X. The ingenious suggestion of Dr. Wallich† that the nature of the Animal life found on the sea-bottom may not unfrequently afford some clue to the history of its changes of *level*,—his discovery at great depths of a type (the *Ophiocoma granulata*) which is essentially *littoral* being indicative of slow progressive subsidence,—may be extended with some probability to changes of submarine *climate*; for where any species is found abundantly as a *littoral* form, its presence at great depths in the same region would seem to indicate that the subsidence of the bottom has not been attended with any considerable alteration of temperature, whilst its absence on neighbouring parts of the same area may be fairly taken as evidence of such a change.

The preparation of a detailed list of the Species found in each locality, with the depths from which they were brought up, furnishing the justification of the general statements made in this Report, has been kindly undertaken by Professor Wyville Thomson, who will present it at the earliest practicable date; and he will also describe the new and very remarkable forms of *Vitreous Sponges* we have obtained, this being a group to which he has already given special attention.—I shall myself lose no time in preparing an account of the *Rhizopods* we have collected, availing myself of the kind assistance of Professor Huxley, who has undertaken to examine and describe the Organic components of our various specimens of Chalk-mud, and of Professor Frankland, who will determine their Chemical composition.

We cannot but hope that when our Report shall have been thus completed, it may be found not unworthy of the Royal Society by which our inquiry was promoted in the first instance, and of the Government which provided the means for its prosecution, and that the results we have obtained may be regarded as sufficiently important to justify its extension both in range

* It will be obvious to every one who is conversant with Sir Charles Lyell's ‘Principles,’ that in the views above stated I have simply *extended* the doctrines long since promulgated by that great Master of the Philosophy of Geology.

† The North-Atlantic Sea-Bed, pp. 149–155.

and objects. For we cannot but believe that Physicists, Physical Geographers, Naturalists, and Geologists will alike desire such a careful and detailed exploration of the Sea-bottom between the North of Scotland and the Faroe Islands; as may determine with precision,—(1) the *depth* in every part of that area; (2) the *temperature*, not merely of every part of the bottom, but also at various depths of the water that lies upon it, say, at every 50 fathoms vertically; (3) the precise boundaries of the *cold area* of bottom-temperature which separates the northern and southern portions of the *warm area*; (4) the *direction* and *rate* of any *current* that may be detected in either or each of these areas; (5) the relative *composition* of the water in these areas respectively; (6) the relative proportions of *gases* contained in the sea-water at different *depths*, and in the same depth at different *temperatures*; (7) the *penetrating power* of the *Actinic* rays in their passage through Sea-water; (8) the nature, composition, and sources of the *deposits* in progress over the several parts of the sea-bottom, especially distinguishing those of its *warm* and those of its *cold* tracts, as well as those along the line or band of demarcation between the two; and (8) the distribution of *Animal and Vegetable Life* throughout the whole region, as complete a collection as possible being made by repeated dredgings in every part of it, so as to furnish materials for valid inferences as to the relations of its several forms to the depth, temperature, and character of the sea-bottom on which they respectively occur.

The near proximity of this area to our own shores, and the consequent facility with which a vessel may be kept at sea during the whole of the season most suitable for work of this kind, by running for supplies to Stornoway, Lerwick, or Kirkwall (as may be most convenient), renders it peculiarly fitting for such an investigation; for just as the limited area of the British Islands presents an epitome of the whole Geological series, so does this limited Oceanic area present such varieties of depth and temperature, and probably of currents, as are only likely to be met with elsewhere at a far greater distance from land, and over a much wider Geographical range.—But it is also greatly to be desired that these inquiries should be prosecuted at still greater depths; and such may be reached with no less facility by proceeding westwards from the West of Scotland or the North-west of Ireland, a depth of at least 1300 fathoms being known to exist between these Coasts and Rockall Banks.

It only remains for me to tender the grateful acknowledgments of Professor Wyville Thomson and myself to Her Majesty's Government for the readiness with which they acceded to the recommendation of the President and Council of the Royal Society, and for the liberality with which the means of prosecuting our inquiries were furnished by the Admiralty; and we would in particular express our obligations to the Hydrographer to the Admiralty for the earnestness with which he took up the idea of this Ex-

pedition in the first instance, the perseverance with which he subsequently carried through every arrangement that could promote its scientific efficiency, and the considerate kindness with which he provided all that was needful for our welfare and comfort. Our cordial thanks are also due to Staff-Commander May for the heartiness with which he threw himself into the work, and the thoughtful consideration he uniformly showed, alike for the objects of the Expedition and for our personal convenience; and to Sub-Navigating-Lieutenant Tooker, by whom Captain May's exertions in both these respects were zealously and efficiently seconded.

We would also record our sense of the friendly reception which we met with on the part of His Excellency the Governor of the Faroe Islands, who, although we were not in any way accredited to him, did his utmost not only to promote the Scientific objects of our visit, but also (with the aid of his accomplished Lady) to render our stay at Thorshaven agreeable to us.

APPENDIX.

From the Minutes of the Council of the Royal Society, June 18, 1868.

From Dr. Carpenter, V.P.R.S., to the President of the Royal Society.

University of London, Burlington House, W.
June 18th, 1868.

DEAR GENERAL SABINE,—During a recent visit to Belfast, I had the opportunity of examining some of the specimens (transmitted by Prof. Sars of Christiania to Prof. Wyville Thomson) which have been obtained by M. Sars, jun., Inspector of Fisheries to the Swedish Government, by *deep-sea* dredgings off the coast of Norway. These specimens, for reasons stated in the enclosed letter from Prof. Wyville Thomson, are of singular interest alike to the Zoologist and to the Palæontologist; and the discovery of them can scarcely fail to excite, both among Naturalists and among Geologists, a very strong desire that the zoology of the *deep sea*, especially in the Northern Atlantic region, should be more thoroughly and systematically explored than it has ever yet been. From what I know of your own early labours in this field, I cannot entertain a doubt of your full concurrence in this desire.

Such an exploration cannot be undertaken by private individuals, even when aided by grants from Scientific Societies. For dredging at great depths, a vessel of considerable size is requisite, with a trained crew, such as is only to be found in the Government service. It was by the aid of such an equipment, furnished by the Swedish Government, that the researches of M. Sars were carried on.

Now as there are understood to be at the present time an unusual number of gun-boats and other cruisers on our northern and western coasts, which will probably remain on their stations until the end of the season, it has occurred to Prof. Wyville Thomson and myself, that the Admiralty, if moved thereto by the Council of the Royal Society, might be induced to place one of these vessels at the disposal of ourselves and of any other Naturalists who might be willing to accompany us, for the purpose of carrying on a systematic course of deep-sea

dredging for a month or six weeks of the present summer, commencing early in August.

Though we desire that this inquiry should be extended both in geographical range and in depth as far as is proposed in Prof. Wyville Thomson's letter, we think it preferable to limit ourselves on the present occasion to a request which will not, we believe, involve the extra expense of sending out a coaling-vessel. We should propose to make Kirkwall or Lerwick our port of departure, to explore the sea-bottom between the Shetland and the Faroe Islands, dredging around the shores and in the fiords of the latter (which have not yet, we believe, been scientifically examined), and then to proceed as far north-west into the deep water between the Faroe Islands and Iceland as may be found practicable.

It would be desirable that the vessel provided for such a service should be one capable of making way under canvas, as well as by steam-power; but as our operations must necessarily be slow, *speed* would not be required. Considerable labour would be spared to the crew if the vessel be provided with a "donkey-engine" that could be used for pulling up the dredge.

If the Council of the Royal Society should deem it expedient to prefer this request to the Admiralty, I trust that they may further be willing to place at the disposal of Prof. Wyville Thomson and myself, either from the Donation Fund or the Government-Grant Fund, a sum of £100 for the expenses we must incur in providing an ample supply of spirit and of jars for the preservation of specimens, with other scientific appliances. We would undertake that the choicest of such specimens should be deposited in the British Museum.

I shall be obliged by your bringing this subject before the Council of the Royal Society, and remain,

Dear General Sabine, yours faithfully,
WILLIAM B. CARPENTER.

The President of the Royal Society.

From Prof. Wyville Thomson, Belfast, to Dr. Carpenter, V.P.R.S.

May 30, 1868.

MY DEAR CARPENTER,—When I last saw you, I suggested how very important it would be to the advancement of science to determine with accuracy the conditions and distribution of Animal Life at great depths in the ocean; I now resume the facts and considerations which lead me to believe that researches in this direction promise valuable results.

All recent observations tend to negative Edward Forbes's opinion that a *zero* of animal life was to be reached at a depth of a few hundred fathoms. Two years ago, M. Sars, Swedish Government Inspector of Fisheries, had an opportunity in his official capacity of dredging off the Loffoden Islands at a depth of 300 fathoms. I visited Norway shortly after his return, and had an opportunity of studying with his father, Prof. Sars, some of his results. Animal forms were *abundant*; many of them were new to science; and among them was one of surpassing interest, the small Crinoid of which you have a specimen, and which we at once recognized as a degraded type of the *Apiocrinidæ*, an order hitherto regarded as extinct, which attained its maximum in the *Pear-encrinites* of the Jurassic Period, and whose latest representative hitherto known was the *Bourguetticrinus* of the Chalk. Some years previously, M. Absjornsen, dredging in 200 fathoms in the Hardangerfjord, procured several examples of a Starfish (*Brisinga*) which seems to find its nearest ally in the fossil genus *Protaster*. These observations place it beyond a doubt that animal life is abundant in the

ocean at depths varying from 200 to 300 fathoms, that the forms at these great depths differ greatly from those met with in ordinary dredgings, and that, at all events in some cases, these animals are closely allied to, and would seem to be directly descended from, the fauna of the early Tertiaries.

I think the latter result might almost have been anticipated; and probably further investigation will add largely to this class of data, and will give us an opportunity of testing our determination of the zoological position of some fossil types by an examination of the soft parts of their recent representatives. The main cause of the destruction, the migration, and the extreme modification of Animal types, appears to be change of climate, chiefly depending upon oscillations of the earth's crust. These oscillations do not appear to have ranged, in the northern portion of the Northern Hemisphere, much beyond 1000 feet since the commencement of the Tertiary epoch. The temperature of deep water seems to be constant for all latitudes at 39° ; so that an immense area of the North Atlantic must have had its conditions unaffected by Tertiary or Post-tertiary oscillations.

One or two other questions of the highest scientific interest are to be solved by the proposed investigations:—

1st. The effect of *pressure* upon Animal life at great depths. There is great misapprehension on this point. Probably a perfectly equal pressure to *any amount* would have little or no effect. Air being highly compressible, and water compressible only to a very slight degree, it is probable that under a pressure of 200 atmospheres, water may be even more aerated, and in that respect more capable of supporting life, than at the surface.

2nd. The effect of the great diminution of the stimulus of Light. From the condition of the Cave Fauna, this latter agent probably affects only the development of colour and of the organs of sight.

I have little doubt that it is quite practicable, with a small heavy dredge, and a couple of miles of stout Manilla rope, to dredge at a depth of 1000 fathoms. Such an undertaking would, however, owing to the distance, and the labour involved, be quite beyond the reach of private enterprise. What I am therefore anxious for is, that the Admiralty may be induced, perhaps at the instance of the Council of the Royal Society, to send a vessel (such as one of those which accompanied the Cable Expedition to take soundings) to carry out the research. I should be ready to go any time after July; and if you would take part in the investigation, I cannot but believe that it would give good results.

I would propose to start from Aberdeen, and to go first to the Rockall fishing-banks, where the depth is moderate, and thence north-westward, towards the coast of Greenland, rather to the north of Cape Farewell. We should thus keep pretty nearly along the isotherm of 39° , shortly reaching 1000 fathoms depth, where, allowing 1000 feet for oscillations in level, and 1000 feet for influence of surface-currents, summer heat, &c., we should still have 4000 feet of water whose conditions have probably not varied greatly since the commencement of the Eocene epoch.

Yours most truly,

WYVILLE THOMSON.

These letters having been considered, it was

Resolved,—That the proposal of Drs. Carpenter and Wyville Thomson be approved, and recommended to the favourable consideration of the authorities of

the Admiralty; and that a sum, of not exceeding £100, be advanced from the Donation Fund to meet the expenses referred to in Dr. Carpenter's letter.

The following draft of a letter to be written by the Secretary to the Secretary of the Admiralty was approved:—

MY LORD,—I am directed to acquaint you, for the information of the Lords Commissioners of the Admiralty, that the President and Council of the Royal Society have had under their consideration a proposal by Dr. Carpenter, Vice-President of the Royal Society, and Dr. Wyville Thomson, Professor of Natural History in Queen's College, Belfast, for conducting dredging operations at greater depths than have heretofore been attempted in the localities which they desire to explore—the main purpose of such researches being to obtain information as to the existence, mode of life, and zoological relations of marine animals living at great depths, with a view to the solution of various questions relating to animal life, and having an important bearing on Geology and Palæontology. The objects of the operations which they wish to undertake, and the course which they would propose to follow, as well as the aid they desire to obtain from the Admiralty, are more fully set forth in the letter of Dr. Carpenter to the President, and that of Professor Thomson, copies of which I herewith inclose.

The President and Council are of opinion that important advantages may be expected to accrue to science from the proposed undertaking; accordingly they strongly recommend it to the favourable consideration of Her Majesty's Government, and earnestly hope that the Lords Commissioners of the Admiralty may be disposed to grant the aid requested. In such case the scientific appliances required would be provided for from funds at the disposal of the Royal Society.

I am, &c.,

W. SHARPEY, Sec. R.S.

Lord H. Lennor, M.P., Secretary of the Admiralty.

From the Minutes of the Council of the Royal Society for October 20, 1868.

Admiralty, 14th July, 1868.

SIR,—In reply to your letter of the 22nd ultimo, submitting a proposition from Dr. Carpenter and Professor Thomson to investigate, by means of dredging, the bottom of the sea in certain localities, with a view to ascertain the existence and zoological relations of marine animals at great depths,—a research which you and the Council of the Royal Society strongly recommend in the interests of science to the favourable consideration of Her Majesty's Government, for aid in furtherance of the undertaking,—I am commanded by My Lords Commissioners of the Admiralty to acquaint you that they are pleased to meet your wishes so far as the Service will admit, and have given orders for Her Majesty's steam-vessel 'Lightning' to be prepared immediately, at Pembroke, for the purpose of carrying out such dredging operations.

I am, Sir,

Your obedient Servant,

W. G. ROMAINE.

To the President of the Royal Society.

The Society then adjourned over the Christmas Recess to Thursday, January 7, 1869.

January 7, 1869.

Lieut.-General SABINE, President, in the Chair.

The following communications were read :—

- I. "Description of the Cavern of Bruniquel, and its Organic Contents.—Part II. Equine Remains." By Professor OWEN, F.R.S.
Received August 20, 1868.

(Abstract.)

In this paper the author has selected the fossil remains of the Equine family as the subject of the second part of his Description of the Cave of Bruniquel and its contents, which Cave, with the human remains, was described in Part I. communicated to the Royal Society, June 9, 1864.

He premises a definition of the several parts of the grinding-surface of the upper and lower molars and premolars in the genus *Equus*, homologizing them with those in the corresponding teeth of *Hipparion*, *Paloplotherium*, and *Palæotherium*.

Next, referring to the want of figures of the natural size, or of any figures of the characteristic surface of the teeth of the molar series in the known species of the existing Equines, the author gives a description thereof in the Horse (*Equus caballus*), Ass (*E. asinus*), Kiang (*E. hemionus*), Quagga (*E. quagga*), Dauw (*E. Burchelli*), and Zebra (*E. Zebra*), indicating by comparison their respective characteristics. These descriptions are accompanied with drawings (of the natural size) of the working-surface of the dentition of each species, with lettered details of such surface in the teeth of both upper and under jaws.

The Equine fossils from the Cave of Bruniquel are then described and compared with each other, with the above-named existing species of *Equus*, and with previously defined fossil species of *Equidæ*. Two varieties in respect of size and some minor characters are pointed out in the Bruniquel series, of one of which figures (of the natural size) of the grinding-surface of the upper and lower molar series, and of the second variety, figures of the same surface of the upper molar series are given.

The author, remarking that such evidences of mature and full-grown animals are rare from the Bruniquel Cave-deposits, selects evidence of certain phases of dentition in the Cave Equines which lend aid in determining their affinities; these phases being illustrated by four drawings of the natural size.

Of the various fossil teeth of *Equidæ* with which those from Bruniquel have been compared, the author finds the closest resemblance, approaching to identity, in certain fossils from freshwater sedimentary deposits of Post-pliocene or "Quaternary" age in the Department of the Puy-de-Dôme, France. Of these, descriptions are given of the teeth of the upper and lower jaws from such deposits at a locality traversed by the river Allier,

near the "Tour de Juvillac." A figure of the working-surface of the teeth of the lower jaw from this locality is given (of the natural size), showing the characters of the canine and proportions of the diastema. The close conformity in the characters of the upper grinders of the Puy-de-Dôme fossils of deposit with those of the Bruniquel cavern enables the author to dispense with figures of them.

The sum of the several comparisons is to refer the above Equine fossils from sedimentary deposits and both varieties from the Bruniquel cave to one and the same species or well-marked race belonging to the true Horses, or restricted genus *Equus* of modern mammalogists; the individuals of which race, with a small range of size, probably due to sex, were less than the average-sized horse of the present period, but larger than known existing striped or unstriped species of *Asinus*, Gray.

Interesting testimony, confirmatory of the conclusion from the palæontological comparisons, is adduced from outlines of the heads of different individuals of the Cave Equine when alive, neatly cut on the smooth surface of a rib of the same species, discovered by the Vicomte de Lastic St. Jal in 1863, in his cavern at Bruniquel, under circumstances which indisputably showed the work to have been done by one of the tribe of men inhabiting the cavern and slaying the wild horses of that locality and period for food.

The author remarks that every bone of the Horse's skeleton (and such evidence had been obtained from about a hundred individuals that had been exhumed at the period of his second visit to Bruniquel, in February 1864) had been split or fractured to gain access to the marrow. The dental canal and roots of the teeth had been similarly exposed in every specimen of jaw.

II. "On the Mechanical Possibility of the Descent of Glaciers, by their Weight only." By the Rev. HENRY MOSELEY, M.A., Canon of Bristol, F.R.S., Instit. Imp. Sc. Paris, Corresp. Received October 21, 1868.

(Abstract.)

All the parts of a glacier do not descend with a common motion; it moves faster at its surface than deeper down, and at the centre of its surface than at its edges. It does not only come down bodily, but with different motions of its different parts; so that if a transverse section were made through it, the ice would be found to be moving differently at every point of that section.

This fact*, which appears first to have been made known by M. Rendu,

* The remains of the guides, lost in 1820 in Dr. Hamel's attempt to ascend Mont Blanc, were found imbedded in the ice of the Glacier des Bossons in 1863. "The men and their things were torn to pieces, and widely separated by many feet. All around them the ice was covered in every direction for twenty or thirty feet with the hair of one knapsack, spread over an area three or four hundred times greater than that of the knap-

Bishop of Annecy, has since been confirmed by the measurements of Agassiz, Forbes, and Tyndall. There is a constant displacement of the particles of the ice over one another, and alongside one another, to which is opposed that force of resistance which is known in mechanics as *shearing force*.

By the property of ice called regelation, when any surface of ice so sheared is brought into contact with another similar surface, it unites with it, so as to form of the two, one continuous mass. Thus a slow displacement of shearing, by which different similar surfaces were continually being brought into the presence and contact of one another, would exhibit all the phenomena of the motion of glacier ice.

Between this resistance to shearing and the force, whatever it may be, which tends to bring the glacier down, there must be a mechanical relation, so that if the shearing resistance were greater the force would be insufficient to cause the descent. The shearing force of cast iron, for instance, is so great that, although its weight is also very great, it is highly improbable a mass of cast iron would descend if it were made to fill the channel of the Mer de Glace, as the glacier does, because its weight would be found insufficient to overcome its resistance to shearing, and thus to supply the work necessary to those internal displacements, of which a glacier is the subject, or even to shear over the irregularities of the rocky channel. The same is probably true of any other metal.

I can find no discussion which has for its object to determine this mechanical relation between what is assumed to be the cause of the descent of a glacier, and the effect produced,—to show that the work of its weight (supposing that alone to cause it to descend) is equal to the works of the several resistances, internal and external, which are actually overcome in its descent. It is my object to establish such a relation.

The forces which oppose themselves to the descent of a glacier are,—1st, the resistance to the sliding motion of one part of a piece of solid ice on the surface of another, which is taking place continually throughout the mass of the glacier, by reason of the different velocities with which its different parts move. This kind of resistance will be called in this paper (for shortness) *shear*, the *unit* of shear being the pressure in lbs. necessary to overcome the resistance to shearing of one square inch, which may be presumed to be constant throughout the mass of the glacier.

2ndly. The friction of the superimposed laminæ of the glacier (which move with different velocities) on one another, which is greater in the lower ones than the upper.

3rdly. The resistance to abrasion, or shearing of the ice, at the bottom of the glacier, and on the sides of its channel, caused by the roughnesses

sack." "This," says Mr. Cowell, from whose paper read before the Alpine Club in April 1864 the above quotation is made, "is not an isolated example of the scattering that takes place in or on a glacier, for I myself saw on the Theodule Glacier the remains of the Syndic of Val Tournanche scattered over a space of several acres."

of the rock, the projections of which insert themselves into its mass, and into the cavities of which it moulds itself.

4thly. The *friction* of the ice in contact with the bottom and sides so sheared over or abraded.

If the whole mechanical *work* of these several resistances in a glacier could be determined, as it regards its descent, for any relatively small time, one day for instance, and also the *work* of its weight in favour of its descent during that day, then, by the principle of "virtual velocities" (supposing the glacier to descend by its weight only), the aggregate of the *work* of these resistances, opposed to its descent, would be equal to the work of its weight, in favour of it. It is, of course, impossible to represent this equality mathematically, in respect to a glacier having a variable direction and an irregular channel and slope; but in respect to an imaginary one, having a constant direction and a uniform channel and slope, it is possible.

Let such a glacier be imagined, of unlimited length, lying on an even slope, and having a uniform rectangular channel, to which it fits accurately, and which is of a uniform roughness sufficient to tear off the surface of the glacier as it advances. Such a glacier would descend with a uniform motion if it descended by its weight only, because the forces acting upon it would be uniformly distributed and constant forces*. The conditions of the descent of any one portion of it would therefore be the same as those of any other equal and similar portion. The portion, the conditions of whose descent it is sought in this paper to determine, is that which has descended through any given transverse section in a day; or, rather, it is one half this mass of ice, for the glacier is supposed to be divided by a vertical plane, passing through the central line of its surface, it being evident that the conditions of the descent of the two halves are the same. The measurements which have been made of the velocities of the surface-ice at different distances from the sides, make it probable that the differences of the spaces described in a given time would be nearly proportional to the distances from the edge in a uniform channel†; and the similar measurements made on the velocities at different depths on the sides that, under the same circumstances, the increments of velocity would be as the distances from the bottom. This law, which observation indicates as to the surface

* It is supposed that the weight is only just sufficient to cause the descent.

† Prof. Tyndall measured the velocity of the surface of the Mer de Glace at a series of points in the same straight line across it at a place called Les Ponts. The distances of these points in feet along the line up to the point of greatest velocity are set off to a scale in fig. 1; and the space in feet through which each point would pass in thirty-six days, if its velocity continued uniformly the same, is shown by a corresponding line at right angles to the other. The extremities of these last lines are joined. It will be seen that the line joining them is for some distance nearly straight; if it were exactly so, the law stated in the text would, in respect to this ice, be absolutely true. Fig. 2 shows in the same manner the spaces described in thirty-six days by points at different depths on the side of the Glacier du Géant, as measured by Prof. Tyndall at the Tacul. See Phil. Trans. Royal Society, vol. cxlix. part 1, pp. 265, 266. [The figures referred to in this note accompany the MS. of the paper.]

and the sides, is supposed to obtain throughout the mass of the glaciers. Any deviation from it, possible under the circumstances, will hereafter be shown to be such as would not sensibly affect the result.

The trapezoidal mass of ice thus passing through a transverse section in a day is conceived to be divided by an infinite number of equidistant vertical planes, parallel to the central line, or axis of the glacier, and also by an infinite number of other equidistant planes parallel to the bed of the glacier. It is thus cut into rectangular prisms or strips lying side by side and above one another. If any one of these strips be supposed to be prolonged through the whole length of the glacier, every part of it will be moving with the same velocity, and it will be continually shearing over two of the similar adjacent strips, and being sheared over by two others. The position of each of these elementary prisms in the transverse section of the glacier is determined by rectangular coordinates; and in terms of these, its length, included in the trapezoid. The work of its *weight*, while it passes through the transverse section into its actual position, is then determined, and the work of its *shear*, and the work of its *friction*. A double integration of each of the functions, thus representing the internal work in respect to a given elementary prism, determines the whole internal work of the trapezoid, in terms of the space traversed by the middle of the surface in one day, the spaces traversed by the upper and lower edges of the side, and a symbol representing the unit of *shear*. Well-known theorems serve to determine the *work* of the *shear* and the *friction* of the bottom and side in terms of the same quantities. All the terms of the equation above referred to are thus arrived at in terms of known quantities, except the unit of *shear*, which the *equation* thus determines. The comparison of this unit of *shear* (which is the greatest possible, in order that the glacier may descend by its weight alone) with the actual unit of *shear* of glacier ice (*determined by experiment*), shows that a glacier cannot descend by its weight only; its shearing force is too great. The true unit of shear being then substituted for its symbol in the equation of condition, the work of the force, which must come in aid of its weight to effect the descent of the glacier, is ascertained.

The imaginary case to which these computations apply, differs from that of an actual glacier in the following respects. The actual glacier is not straight, or of a uniform section and slope, and its channel is not of uniform roughness. In all these respects the resistance to the descent of the actual glacier is greater than to the supposed one. But this being the case, the resistance to shearing must be less, in order that the same force, viz. the weight, may be just sufficient to bring down the glacier in the one case, as it does in the other. The ice in the natural channel must shear more easily than that in the artificial channel, if both descend by their weight only; so that if we determine the unit of shear necessary to the descent of the glacier in the artificial channel, we know that the unit of

shear necessary to its descent by its weight only in the natural channel must be *less than that*.

A second possible difference between the case supposed and the actual case lies in this, that the velocities of the surface-ice at different distances from the edge, and at different heights from the bottom, are assumed to be proportional to those distances and heights; so that the mass of ice at any time passing through a transverse section may be bounded by plane surfaces, and have a trapezoidal form. This may not strictly be the case. All the measurements, however, show that if the surfaces be not plane, they are convex *downwards*. In so far therefore as the quantity of ice passing through a given section in a day is different from what it is supposed to be, it is greater than it. A greater resistance (other than shearing) is thus opposed to each day's descent, and also a greater weight of ice favours it; but the disproportion is so great between the work of the additional resistance to the descent, and that of the additional weight of ice in favour of it, that it is certain that any such convexity of the trapezoidal surface would necessitate a further reduction of the unit of shear, to make the weight of the actual glacier sufficient to cause it to descend.

A third difference between the actual glacier and the imaginary one, to the computation of whose unit of shear the following formulæ are applied, is this—that the formulæ suppose the daily motion of the surface of the glacier and the daily motion of its side to have been measured at the same place, whereas there exist no measurements of the surface motion and the side motion at the same place. The surface motion used has been that of the Mer de Glace at Les Ponts, and the side motion that of the Glacier du Géant at the Tacul—both from the measurements of Prof. Tyndall. This error again, however, tends to cause the unit of shear, deduced from the case of the artificial glacier, to be greater than that in the actual one; for the Glacier du Géant moves more slowly than the Mer de Glace. The quantity of ice which actually passes through a section at Les Ponts is therefore greater than it is assumed in the computation to be, whence it follows, as in the last case, that the computed unit of shear is greater than the actual unit of shear.

To determine the actual value of μ (the unit of shear in the case of ice) the following experiment was made. Two pieces of hard wood, each three inches thick and of the same breadth, but of which one was considerably longer than the other, were placed together, the surfaces of contact being carefully smoothed, and a cylindrical hole, $1\frac{1}{2}$ inch in diameter, was pierced through the two. The longer piece was then screwed down upon a frame which carried a pulley, over which a cord passed to the middle of the shorter piece, which rested on the longer. There were lateral guides to keep the shorter piece from deviating sideways when moved on the longer. The hole in the upper piece being brought so as accurately to coincide with that in the lower, small pieces of ice were

thrown in, a few at a time, and driven home by sharp blows of a mallet on a wooden cylinder. By this means a solid cylinder of ice was constructed, accurately fitting the hole. Weights were then suspended from the rope, passing over the pulley until the cylinder of ice was sheared across. As by the melting of the ice, during the experiment, the diameter of the cylinder was slightly diminished, it was carefully measured with a pair of callipers.

1st experiment.—Radius of cylinder .65625 in., sheared with 98 lbs.

2nd experiment.—Radius of cylinder .70312 in., sheared with 119 lbs.

By the first experiment the shear per square inch, or *unit* of shear, was 72.433 lbs.; by the second experiment it was 76.619 lbs. The main unit of shear of ice, from these two experiments, is therefore 75 lbs.

Now it appears by the preceding calculations, that to descend by its own weight, at the rate at which Prof. Tyndall observed the ice of the Mer de Glace to be descending at the Tacul, the unit of shearing force of the ice could not have been more than 1.3193 lb.*

To determine how *great* a force, in addition to its weight, would be necessary to cause the descent of a glacier of uniform section and slope, such as has been supposed in the calculations, let u represent, in inch-lbs., the *work* of that force in twenty-four hours. Then assuming the unit of shear (μ) in glacier ice to be 75 lbs., it follows, by the principle of virtual velocities, that

$$\begin{aligned} u &= 94134000 + 1012560 - 2668400 \\ &= 92478160 \text{ inch-lbs.} = 7706513 \text{ foot-lbs.}^\dagger \end{aligned}$$

This computation has reference to half only of the width of the glacier, and to 23.25 inches of its length. The work, in excess of its weight, required to make a mile of the imaginary glacier, 466 yards broad and 140 feet deep, descend, as it actually does descend per twenty-four hours, is represented by the horse-power of an engine, which, working constantly day and night, would yield this work, or by

$$\frac{2 \times 7706513 \times 5280 \times 12}{23.2 \times 24 \times 60 \times 33000} = 883.78 \text{ h. p.}$$

The surface of the mass of ice, on which the work u is required to be done, in aid of its weight, to make it descend as it actually does, is 124771.5 square inches. The work required to be done on each square inch of surface, supposing it to be equally distributed over it, is therefore,

$$\text{in foot-lbs., } \frac{7706513}{124771.5} = 61.76.$$

* By an experiment on the shearing of putty, similar to that which was made on the shearing of ice, its unit of shear was found to vary from 1 lb. to 3 lbs., according to its degree of hardness. If ice were of the same weight per unit of volume as soft putty, and its consistency about the same, it would descend by its weight *only* without the aid of any other force. It would not, however, be possible to walk on such ice.

† Thus the work to be done in aid of the weight is thirty-four times the work of the weight.

These 61·76 foot-lbs. of work are equivalent to ·0635 heat-units, or to the heat necessary to raise ·0635 lb. of water by one degree of Fahrenheit. This amount of heat passing into the mass of the glacier per square inch of surface per day, and reconverted into mechanical work *there*, would be sufficient, together with its weight, to bring the glacier down.

The following considerations may serve to disabuse some persons of the idea of an unlimited reservoir of force residing somewhere in the prolongation of a glacier backward, and in its higher slopes, from which reservoir the pressure is supposed to come which crushes the glacier over the obstacles in its way.

Let a strip of ice one square inch in section, and one mile in length, in the middle of the surface of the imaginary glacier, be conceived to be separated from the rest throughout its whole length, except for the space of one inch, so that throughout its whole length, except for that one inch, its descent is not retarded either by shear or by friction. Let, moreover, this inch be conceived to be at the very end of the glacier, so that there is no glacier beyond it. Now it may easily be calculated that this strip of ice, one inch square and one mile long, lying on a slope of $4^{\circ} 52'$, without any resistance to its descent, except at its end, must press against its end, by reason of its weight, with a force of 194·42 lbs. But the cubical inch of solid ice at its extremity opposes, by the *shear* of its three surfaces, whose attachment to the adjacent ice is unbroken, a resistance of 3×75 lbs., or 225 lbs. That resistance stops therefore the descent of this strip of ice, one mile long, having no other resistance than this opposed to its descent, by reason of its detachment from the rest*. It is clear, then, that it could not have descended by its weight only when it *adhered* to the rest, and when its descent was opposed by the shear of its whole length; and the same may be proved of any number of miles of strip in *prolongation* of this. Also, with obvious modifications, it may be shown, in the same way, to be true of any *other* similar strip of ice in the glacier, whether on the surface or not, and therefore of the whole glacier.

It results from this investigation that the weight of a glacier is insufficient to account for its descent; that it is necessary to conceive, in addition to its weight, the operation of some other and much greater force, which must also be such as would produce those internal molecular displacements and those strains which are observed actually to take place in glacier ice, and must therefore be present to every part of the glacier as its weight is, but more than thirty-four times as great.

* If, however, the glacier were inclined at $35^{\circ} 10'$, instead of $4^{\circ} 52'$, and a strip were detached from its surface, as described above, it would equal the shear of one cubic inch at its lower end, if it were 300 yards long, and if the glacier were vertical, when it was 172·8 yards long.

III. Notes of a Comparison of the Granites of Cornwall and Devonshire with those of Leinster and Mourne.” By the Rev. SAMUEL HAUGHTON, M.D., D.C.L., F.R.S., Fellow of Trinity College, Dublin. Received December 18, 1868.

The granites of Mourne are eruptive, and can be proved to contain albite as their second felspar.

The granites of Leinster are also eruptive ; and although albite has never yet been actually found to occur in them, its existence can be inferred with considerable probability.

During the past summer (1868) I have succeeded in proving that the second felspar that occurs in the granites of Cornwall is albite. I found this mineral as a constituent of the granite at Trewavas Head, where it has the following composition :—

I. *Albite, var. Cleavelandite (Trewavas Head).*

Silica	65·76
Alumina	21·72
Lime.. . . .	0·89
Magnesia	trace
Soda	9·23
Potash	1·76
Water	0·40
	<hr/> 99·76

This albite is opaque, cream-coloured, lamellar, and associated with quartz and orthoclase, which has the following composition :—

II. *Orthoclase (Trewavas Head).*

	No. 1*.	No. 2†.
Silica	63·60	63·20
Alumina	21·04	21·00
Iron and manganese oxides	trace	trace
Lime	0·90	0·68
Magnesia	trace	trace
Soda	3·08	2·75
Potash	9·91	10·30
Water	0·40	0·40
	<hr/> 98·93	<hr/> 98·33

The granites of Cornwall and Devon contain two micas, white and black. I was fortunate enough to obtain, through my friend Mr. W. J. Henwood, F.R.S., of Penzance, a sufficient quantity of white mica from Tremearne, near Trewavas Head, to determine accurately its composition, which proves to be highly interesting. It differs essentially from the white mica of Leinster and Donegal, and proves to be a variety of lepidolite.

* From veins at foot of cliff associated with Cleavelandite albite.
† From the granite at summit of cliff.

III. *White Mica, Lepidolite (Tremearne, near Trewavas Head).*

Silica, SiO ₃	47·60
Fluosilicon, SiF ₃	5·68
Alumina	27·20
Iron peroxide'	5·20
Manganese protoxide	1·20
Lime	0·45
Magnesia	trace
Potash	10·48
Soda	0·72
Lithia	1·14
	<hr/> 99·67

This lepidolite is white, pearly, and occurs in rhombic tables of 60° and 120°. Its oxygen ratios are, reckoning for the fluorine its equivalent of oxygen,—

Oxygen Ratios.

Silica	24·714	} 26·461	8·9
Fluosilicon	1·747		
Alumina	12·713	} 14·270	4·8
Iron peroxide	1·557		
Manganese protoxide ..	0·268	} 2·982	1·00
Lime	0·127		
Magnesia		
Potash	1·776		
Soda	0·184		
Lithia	0·627		

This corresponds with a theoretical formula, in which the oxygen of the silica is to that of the bases as 3 : 2.

The Black Mica of the Cornish granites seems to be more abundant than the White Mica already described. I found a sufficient quantity of it at Coron Bosavern, near St. Just, to enable me to make the following analysis:—

IV. *Black Mica, Lepidomelane (Coron Bosavern, near St. Just).*

Silica (SiO ₃)	39·92
Fluosilicon (SiF ₃)	3·04
Alumina	22·88
Iron peroxide	15·02
Iron protoxide	2·32
Manganese protoxide	1·40
Lime	0·68
Magnesia	1·07
Potash	9·76
Soda	0·99
Lithia	1·71
	<hr/> 98·79

The Black Mica of St. Just is of a blackish-bronze colour and metallic lustre, and occurs in rhombs of 60° and 120° angles. Its oxygen ratios are, reckoning for the fluorine its equivalent of oxygen,—

Oxygen Ratios.

Silica	20·727	} 21·645
Fluosilicon	0·918	
Alumina	10·692	} 15·092
Iron peroxide	4·400	
Iron protoxide	0·514	} 4·292
Manganese protoxide	0·310	
Lime	0·192	
Magnesia	0·427	
Potash	1·655	
Soda	0·254	
Lithia	0·940	

The oxygen ratio of this iron-potash Mica (which is undoubtedly a lepidomelane) for silica and bases is

$$216 : 194, \text{ or } 1 : 1.$$

The granites of Cornwall and Devon, which have been frequently examined by me during the last sixteen years, appear all to contain the two felspars and the two micas above analyzed. In a future communication I hope to describe their composition in detail, and to give a comparison of this composition with that of the granites of Ireland.

The following generalizations will be found, as I believe, capable of proof.

(1) The granites of Ireland may be divided into two distinct classes, marked by characters both geological and mineralogical.

(2) The First Class of granites consists of Eruptive rocks, of ages varying from the Silurian to the Carboniferous periods. To this class may be referred the granites of Leinster and Mourne, and the granites of Cornwall and Devon.

(3) The First Class of granites is characterized by the presence of orthoclase and albite, and by the absence of all the Lime Felspars.

(4) The Second Class of granites consists of Metamorphic rocks, of unknown geological age, but probably subsequent to the Laurentian period. To this class may be referred the granites of Donegal and Galway, and the granites of Scotland, Norway, and Sweden.

(5) The Second Class of granites is characterized by the presence of orthoclase and oligoclase, or Labradorite, or some other of the Lime Felspars, and by the absence of albite.

January 14, 1869.

Lieut.-General SABINE, President, in the Chair.

The following communications were read :—

- I. "On the Relation of Hydrogen to Palladium." By THOMAS GRAHAM, F.R.S., Master of the Mint. Received November 23, 1868.

It has often been maintained on chemical grounds that hydrogen gas is the vapour of a highly volatile metal. The idea forces itself upon the mind that palladium with its occluded hydrogen is simply an alloy of this volatile metal, in which the volatility of the one element is restrained by its union with the other, and which owes its metallic aspect equally to both constituents. How far such a view is borne out by the properties of the compound substance in question will appear by the following examination of the properties of what, assuming its metallic character, would have to be named *Hydrogenium*.

1. *Density*.—The density of palladium when charged with eight or nine hundred times its volume of hydrogen gas is perceptibly lowered; but the change cannot be measured accurately by the ordinary method of immersion in water, owing to a continuous evolution of minute hydrogen bubbles which appears to be determined by contact with the liquid. However, the linear dimensions of the charged palladium are altered so considerably that the difference admits of easy measurement, and furnishes the required density by calculation. Palladium in the form of wire is readily charged with hydrogen by evolving that gas upon the surface of the metal in a galvanometer containing dilute sulphuric acid as usual*. The length of the wire before and after a charge is found by stretching it on both occasions by the same moderate weight, such as will not produce permanent distension, over the surface of a flat graduated measure. The measure was graduated to hundredths of an inch, and by means of a vernier, the divisions could be read to thousandths. The distance between two fine cross lines marked upon the surface of the wire near each of its extremities was observed.

Expt. 1.—The wire had been drawn from welded palladium, and was hard and elastic. The diameter of the wire was 0.462 millimetre; its specific gravity was 12.38, as determined with care. The wire was twisted into a loop at each end and the mark made near each loop. The loops were varnished so as to limit absorption of gas by the wire to the measured length between the two marks. To straighten the wire, one loop was fixed, and the other connected with a string passing over a pulley and loaded with 1.5 kilogramme, a weight sufficient to straighten the wire without occasioning any undue strain. The wire was charged with hydrogen by making it the negative electrode of a small Bunsen's battery consisting of two cells, each of half a litre in capacity. The positive electrode was a thick platinum wire placed side by side with the palladium wire, and

* Proceedings of the Royal Society, p. 422, 1868.

extending the whole length of the latter within a tall jar filled with dilute sulphuric acid. The palladium wire had, in consequence, hydrogen carried to its surface, for a period of $1\frac{1}{2}$ hour. A longer exposure was found not to add sensibly to the charge of hydrogen acquired by the wire. The wire was again measured and the increase in length noted. Finally the wire, being dried with a cloth, was divided at the marks, and the charged portion heated in a long narrow glass tube kept vacuum by a Sprengel aspirator. The whole occluded hydrogen was thus collected and measured; its volume is reduced by calculation to Bar. 760 millims., and Therm. 0° C.

The original length of the palladium wire exposed was 609.144 millims. (23.982 inches), and its weight 1.6832 gm. The wire received a charge of hydrogen amounting to 936 times its volume, measuring 128 cubic centims., and therefore weighing 0.01147 gm. When the gas was ultimately expelled, the loss as ascertained by direct weighing was 0.01164 gm. The charged wire measured 618.923 millims., showing an increase in length of 9.779 millims. (0.385 inch). The increase in linear dimensions is from 100 to 101.605, and in cubic capacity, assuming the expansion to be equal in all directions, from 100 to 104.908. Supposing the two metals united without any change of volume, the alloy may therefore be said to be composed of

	By volume.	
Palladium	100	or 95.32
Hydrogenium	4.908	or 4.68
	104.908	100

The expansion which the palladium undergoes appears enormous if viewed as a change of bulk in the metal only, due to any conceivable physical force, amounting as it does to sixteen times the dilatation of palladium when heated from 0° to 100° C. The density of the charged wire is reduced, by calculation, from 12.3 to 11.79. Again, as 100 is to 4.91, so the volume of the palladium, 0.1358 cubic centim., is to the volume of the hydrogenium, 0.006714 cubic centim. Finally, dividing the weight of the hydrogenium, 0.01147 gm., by its volume in the alloy, 0.006714 cubic centim., we find

Density of hydrogenium 1.708

The density of hydrogenium, then, appears to approach that of magnesium, 1.743, by this first experiment.

Further, the expulsion of hydrogen from the wire, however caused, is attended with an extraordinary contraction of the latter. On expelling the hydrogen by a moderate heat, the wire not only receded to its original length, but fell as much below that zero as it had previously risen above it. The palladium wire first measuring 609.144 millims., and which increased 9.77 millims., was ultimately reduced to 599.444 millims., and contracted 9.7 millims. The wire is permanently shortened. The density of the pal-

ladium did not increase, but fell slightly at the same time, namely from 12·38 to 12·12, proving that this contraction of the wire is in length only. The result is the converse of extension by wire-drawing. The retraction of the wire is possibly due to an effect of wire-drawing in leaving the particles of metal in a state of unequal tension, a tension which is excessive in the direction of the length of the wire. The metallic particles would seem to become mobile, and to right themselves in proportion as the hydrogen escapes; and the wire contracts in length, expanding, as appears by its final density, in other directions at the same time.

A wire so charged with hydrogen, if rubbed with the powder of magnesia (to make the flame luminous), burns like a waxed thread when ignited in the flame of a lamp.

Expt. 2.—Another portion of the same palladium wire was charged with hydrogen in a similar manner. The results observed were as follows:—

Length of palladium wire	488·976	millims.
The same with 867·15 volumes of occluded gas	495·656	„
Linear elongation	6·68	„
Linear elongation on 100	1·3663	„
Cubic expansion on 100	4·154	„
Weight of palladium wire	1·0667	gram.
Volume of palladium wire	0·08072	cub. centim.
Volume of occluded hydrogen gas	75·2	„
Weight of same	0·00684	gram.
Volume of hydrogenium	0·003601	cub. centim.
From these results is calculated		
Density of hydrogenium	1·898.	

Expt. 3.—The palladium wire was new, and on this occasion was well annealed before being charged with hydrogen. The wire was exposed at the negative pole for two hours, when it had ceased to elongate.

Length of palladium wire	556·185	millims.
Same with 888·303 volumes hydrogen ..	563·652	„
Linear elongation	7·467	„
Linear elongation on 100	1·324	„
Cubic expansion on 100	4·025	„
Weight of palladium wire	1·1675	gram.
Volume of palladium wire	0·0949	cub. centim.
Volume of occluded hydrogen gas	84·3	cub. centims.
Weight of same	0·007553	gram.
Volume of hydrogenium	0·003820	cub. centim.
These results give by calculation		
Density of hydrogenium	1·977.	

It was necessary to assume in this discussion that the two metals do not

contract nor expand, but remain of their proper volume on uniting. Dr. Matthiessen has shown that in the formation of alloys generally the metals retain approximately their original densities*.

In the first experiment already described, probably the maximum absorption of gas by wire, amounting to 935·67 volumes, is attained. The palladium may be charged with any smaller proportion of hydrogen by shortening the time of exposure to the gas (329 volumes of hydrogen were taken up in twenty minutes), and an opportunity be gained of observing if the density of the hydrogenium remains constant, or if it varies with the proportion in which hydrogen enters the alloy. In the following statement, which includes the three experiments already reported, the essential points only are produced.

TABLE.

Volumes of hydrogen occluded.	Linear expansion in millimetres.		Density of Hydrogenium.
	From	To	
329	496·189	498·552	2·055
462	493·040	496·520	1·930
487	370·358	373·126	1·927
745	305·539	511·303	1·917
867	488·976	495·656	1·898
888	556·185	563·652	1·977
936	609·144	618·923	1·708

If the first and last experiments only are compared, it would appear that the hydrogenium becomes sensibly denser when the proportion of it is small, ranging from 1·708 to 2·055. But the last experiment of the Table it perhaps exceptional; and all the others indicate considerable uniformity of density. The mean density of hydrogenium, according to the whole experiments, excluding that last referred to, is 1·951, or nearly 2. This uniformity is in favour of the method followed for estimating the density of hydrogenium.

On charging and discharging portions of the same palladium wire repeatedly, the curious retraction was found to continue, and seemed to be interminable. The following expansions, caused by variable charges of hydrogen, were followed on expelling the hydrogen by the retractions mentioned.

	Elongation.		Retraction.	
1st Experiment	9·77	millims.	9·70	millims.
2nd „	5·765	„	6·20	„
3rd „	2·36	„	3·14	„
4th „	3·482	„	4·95	„
				23·99

The palladium wire, which originally measured 609·144 millims., has

* Philosophical Transactions, 1860, p. 177.

suffered, by four successive discharges of hydrogen from it, a permanent contraction of 23·99 millims. ; that is, a reduction of 3·9 per cent. on its original length. The contractions will be observed to exceed in amount the preceding elongations produced by the hydrogen, particularly when the charge of the latter is less considerable. With another portion of wire the contraction was carried to 15 per cent. of its length by the effect of repeated discharges. The specific gravity of the contracted wire was 12·12, no general condensation of the metal having taken place. The wire shrinks in length only.

In the preceding experiments the hydrogen was expelled by exposing the palladium placed within a glass tube to a moderate heat short of redness, and exhausting by means of a Sprengel tube ; but the gas was also withdrawn in another way, namely, by making the wire the positive electrode, and thereby evolving oxygen upon its surface. In such circumstances a slight film of oxide of palladium is formed on the wire, but it appears not to interfere with the extraction and oxidation of the hydrogen. The wire measured,

		Difference.
Before charge	443·25 millims.	
With hydrogen	449·90 „	+ 6·65 millims.
After discharge	437·31 „	— 5·94 „

The retraction of the wire therefore does not require the concurrence of a high temperature. This experiment further proved that a large charge of hydrogen may be removed in a complete manner by exposure to the positive pole (for four hours in this case) ; for the wire in its ultimate state gave no hydrogen on being heated *in vacuo*.

That particular wire, which had been repeatedly charged with hydrogen, was once more exposed to a maximum charge, for the purpose of ascertaining whether or not its elongation under hydrogen might now be facilitated and become greater in consequence of the previous large retraction. No such extra elongation, however, was observed on charging the retracted wire more than once ; and the expansion continued to be in the usual proportion to the hydrogen absorbed. The final density of the wire was 12·18.

The wire retracted by heat is found to be altered in another way, which appears to indicate a molecular change. When the gas has been expelled by heat, the metal gradually loses much of its power to take up hydrogen. The last wire, after it had already been operated upon six times, was again charged with hydrogen for two hours, and was found to occlude only 320 volumes of gas, and in a repetition of the experiment, 330·5 volumes. The absorbent power of the palladium had therefore been reduced to about one-third of its maximum.

The condition of the retracted wire appeared, however, to be improved by raising its temperature to full redness by sending through it an electrical

current from a battery. The absorption rose thereafter to 425 volumes of hydrogen, and in a second experiment to 422·5 volumes.

The wire becomes fissured longitudinally, acquires a thready structure, and is much disintegrated on repeatedly losing hydrogen, particularly when the hydrogen has been extracted by electrolysis in an acid fluid. The palladium in the last case is dissolved by the acid to some extent. The metal appeared, however, to recover its full power to absorb hydrogen, now condensing upwards of 900 volumes of gas.

The effect upon its length of simply annealing the palladium wire by exposure in a porcelain tube to a full red heat, was observed. The wire measured 556·075 millims. before, and 555·875 millims. after heating; or a minute retraction of 0·2 millim, was indicated. In a second annealing experiment, with an equal length of new wire, no sensible change whatever of length could be discovered. There is no reason, then, to ascribe the retraction after hydrogen, in any degree, to the heat applied when the gas is expelled. Palladium wire is very slightly affected in physical properties by such annealing, retaining much of its first hardness and elasticity.

2. *Tenacity*.—A new palladium wire, similar to the last, of which 100 millims. weighed 0·1987 grm., was broken, in experiments made on two different portions of it, by a load of 10 and of 10·17 kilogrammes. Two other portions of the same wire, fully charged with hydrogen, were broken by 8·18, and by 8·27 kilogrammes. Hence we have—

Tenacity of palladium wire 100

Tenacity of palladium and hydrogen 81·29

The tenacity of the palladium is reduced by the addition of hydrogen, but not to any great extent. It is a question whether the degree of tenacity that still remains is reconcileable with any other view than that the second element present possesses of itself a degree of tenacity such as is only found in metals.

3. *Electrical Conductivity*.—Mr. Becker, who is familiar with the practice of testing the capacity of wires for conducting electricity, submitted a palladium wire, before and after being charged with hydrogen, to trial, in comparison with a wire of German silver of equal diameter and length, at 10°·5. The conducting-power of the several wires was found as follows, being referred to pure copper as 100 :—

Pure copper 100

Palladium 8·10

Alloy of 80 copper + 20 nickel 6·63

Palladium + hydrogen 5·99

A reduced conducting-power is generally observed in alloys, and the charged palladium wire falls 25 per cent. But the conducting-power remains still considerable, and the result may be construed to favour the metallic character of the second constituent of the wire. Dr. Matthiessen confirms these results.

4. *Magnetism*.—It is given by Faraday as the result of all his experiments, that palladium is “feebly but truly magnetic;” and this element he placed at the head of what are now called the paramagnetic metals. But the feeble magnetism of palladium did not extend to its salts. In repeating such experiments, a horseshoe electromagnet of soft iron, about 15 centims. (6 inches) in height, was made use of. It was capable of supporting 60 kilogs., when excited by four large Bunsen cells. This is an induced magnet of very moderate power. The instrument was placed with its poles directed upwards; and each of these was provided with a small square block of soft iron terminating laterally in a point, like a small anvil. The palladium under examination was suspended between these points in a stirrup of paper attached to three fibres of cocoon silk, 3 decimetres in length, and the whole was covered by a bell glass. A filament of glass was attached to the paper, and moved as an index on a circle of paper on the glass shade divided into degrees. The metal, which was an oblong fragment of electro-deposited palladium, about 8 millims. in length and 3 millims. in width, being at rest in an equatorial position (that is, with its ends averted from the poles of the electromagnet), the magnet was then charged by connecting it with the electrical battery. The palladium was deflected slightly from the equatorial line by 10° only, the magnetism acting against the torsion of the silk suspending thread. The same palladium charged with 604.6 volumes of hydrogen was deflected by the electromagnet through 48° , when it set itself at rest. The gas being afterwards extracted, and the palladium again placed equatorially between the poles, it was not deflected in the least perceptible degree. The addition of hydrogen adds manifestly, therefore, to the small natural magnetism of the palladium. To have some terms of comparison, the same little mass of electro-deposited palladium was steeped in a solution of nickel, of sp. gr. 1.082, which is known to be magnetic. The deflection under the magnet was now 35° , or less than with hydrogen. The same palladium being afterwards washed and impregnated with a solution of protosulphate of iron of sp. gr. 1.048, of which the metallic mass held 2.3 per cent. of its weight, the palladium gave a deflection of 50° , or nearly the same as with hydrogen. With a stronger solution of the same salt, of sp. gr. 1.17, the deflection was 90° , and the palladium pointed axially.

Palladium in the form of wire or foil gave no deflection when placed in the same apparatus, of which the moderate sensitiveness was rather an advantage in present circumstances; but when afterwards charged with hydrogen, the palladium uniformly gave a sensible deflection of about 20° . A previous washing of the wire or foil with hydrochloric acid, to remove any possible traces of iron, did not modify this result. Palladium reduced from the cyanide and also precipitated by hypophosphorous acid, when placed in a small glass tube, was found to be not sensibly magnetic by our test; but it always acquired a sensible magnetism when charged with hydrogen.

It appears to follow that hydrogenium is magnetic, a property which is confined to metals and their compounds. This magnetism is not perceptible in hydrogen gas, which was placed both by Faraday and by M. E. Becquerel at the bottom of the list of diamagnetic substances. This gas is allowed to be upon the turning-point between the paramagnetic and diamagnetic classes. But magnetism is so liable to extinction under the influence of heat, that the magnetism of a metal may very possibly disappear entirely when it is fused or vaporized, as appears to be the case with hydrogen in the form of gas. As palladium stands high in the series of the paramagnetic metals, hydrogenium must be allowed to rise out of that class, and to take place in the strictly magnetic group, with iron, nickel, cobalt, chromium, and manganese.

5. *Palladium with Hydrogen at a high Temperature.*—The ready permeability of heated palladium by hydrogen gas would imply the retention of the latter element by the metal even at a bright red heat. The hydrogenium must in fact travel through the palladium by cementation, a molecular process which requires time. The first attempts to arrest hydrogen in its passage through the red-hot metal were made by transmitting hydrogen gas through a metal tube of palladium with a vacuum outside, rapidly followed by a stream of carbonic acid, in which the metal was allowed to cool. When the metal was afterwards examined in the usual way, no hydrogen could be found in it. The short period of exposure to the carbonic acid seems to have been sufficient to dissipate the gas. But on heating palladium foil red-hot in a flame of hydrogen gas, and suddenly cooling the metal in water, a small portion of hydrogen was found locked up in the metal. A volume of metal amounting to 0.062 cubic centim., gave 0.080 cubic centim. of hydrogen; or, the gas, measured cold, was 1.306 times the bulk of the metal. This measure of gas would amount to three or four times the volume of the metal at a red heat. Platinum treated in the same way appeared also to yield hydrogen, although the quantity was too small to be much relied upon, amounting only to 0.06 volume of the metal. The permeation of these metals by hydrogen appears therefore to depend on absorption, and not to require the assumption of anything like porosity in their structure.

The highest velocity of permeation observed was in the experiment where four litres of hydrogen (3992 cub. centims.) per minute passed through a plate of palladium 1 millim. in thickness, and calculated for a square metre in surface, at a bright red heat a little short of the melting-point of gold. This is a travelling movement of hydrogen through the substance of the metal with the velocity of 4 millimetres per minute.

6. *Chemical Properties.*—The chemical properties of hydrogenium also distinguish it from ordinary hydrogen. The palladium alloy precipitates mercury and calomel from a solution of the chloride of mercury without any disengagement of hydrogen; that is, hydrogenium decomposes chloride of mercury, while hydrogen does not. This explains why M. Stanislas

Meunier failed in discovering the occluded hydrogen of meteoric iron, by dissolving the latter in a solution of chloride of mercury; for the hydrogen would be consumed, like the iron itself, in precipitating mercury. Hydrogen (associated with palladium) unites with chlorine and iodine in the dark, reduces a persalt of iron to the state of protosalt, converts red prussiate of potash into yellow prussiate, and has considerable deoxidizing powers. It appears to be the active form of hydrogen, as ozone is of oxygen.

The general conclusions which appear to flow from this inquiry are, that in palladium fully charged with hydrogen, as in the portion of palladium wire now submitted to the Royal Society, there exists a compound of palladium and hydrogen in a proportion which may approach to equal equivalents*. That both substances are solid, metallic, and of a white aspect. That the alloy contains about 20 volumes of palladium united with a volume of hydrogenium; and that the density of the latter is about 2, a little higher than magnesium to which hydrogenium may be supposed to bear some analogy. That hydrogenium has a certain amount of tenacity, and possesses the electrical conductivity of a metal. And finally, that hydrogenium takes its place among magnetic metals. The latter fact may have its bearing upon the appearance of hydrogenium in meteoric iron, in association with certain other magnetic elements.

I cannot close this paper without taking the opportunity to return my best thanks to Mr. W. C. Roberts for his valuable cooperation throughout the investigation.

II. "A Memoir on the Theory of Reciprocal Surfaces."

By Professor CAYLEY, F.R.S. Received November 12, 1868.

(Abstract.)

The present Memoir contains some extensions of Dr. Salmon's theory of Reciprocal Surfaces. I wish to put the formulæ on record, in order to be able to refer to them in a "Memoir on Cubic Surfaces," but without at present attempting to completely develop the theory.

Dr. Salmon's fundamental formulæ (A), (B) are replaced by

$$\begin{aligned} a(n-2) &= \kappa - B + \rho + 2\sigma, \\ b(n-2) &= \rho + 2\beta + 3\gamma + 3t, \\ c(n-2) &= 2\sigma + 4\beta + \gamma + \theta, \end{aligned}$$

$$\begin{aligned} a(n-2)(n-3) &= 2(\delta - C) + 3(ac - 3\sigma - \chi) + 2(ab - 2\rho - j), \\ b(n-2)(n-3) &= 4k + (ab - 2\rho - j) + 3(bc - 3\beta - \gamma - i), \\ c(n-2)(n-3) &= 6h + (ac - 3\sigma - \chi) + 2(bc - 3\beta - \gamma - i), \end{aligned}$$

where j , θ , χ , B , C refer to singularities not taken account of in his theory; viz. j is the number of pinch-points on the nodal curve θ , χ , the numbers of certain singular points on the cuspidal curve, C the number of conic nodes, B the number of biplanar nodes: the reciprocal singularities j' , θ' , χ' ,

* Proceedings of the Royal Society, 1868, p. 425.

B, C' , are of course also considered. An equation of Dr. Salmon's is presented in the extended form,

$$\sigma' = 4n(n-2) - 8b - 11c - 2j' - 3\chi' - 2C' - 4B';$$

and it is remarked that σ' denotes the order of the spinode-curve. The Memoir contains an entirely new formula giving the value of β' , but some of the constants of the formula remain undetermined.

III. "A Memoir on Cubic Surfaces."

By Professor CAYLEY, F.R.S. Received November 12, 1868.

(Abstract.)

The present Memoir is based upon, and is in a measure supplementary to that by Professor Schläfli, "On the Distribution of Surfaces of the Third Order into Species, in reference to the presence or absence of Singular Points, and the reality of their Lines," *Phil. Trans.* vol. cliii. (1863) pp. 193-241. But the object of the Memoir is different. I disregard altogether the ultimate division depending on the reality of the lines, attending only to the division into (twenty-two, or as I prefer to reckon it) twenty-three cases depending on the nature of the singularities. And I attend to the question very much on account of the light to be obtained in reference to the theory of Reciprocal Surfaces. The memoir referred to furnishes in fact a store of materials for this purpose, inasmuch as it gives (partially or completely developed) the equations in plane-coordinates of the several cases of cubic surfaces; or, what is the same thing, the equations in point-coordinates of the several surfaces (orders 12 to 3) reciprocal to these respectively. I found by examination of the several cases, that an extension was required of Dr. Salmon's theory of Reciprocal Surfaces in order to make it applicable to the present subject; and the preceding "Memoir on the Theory of Reciprocal Surfaces" was written in connexion with these investigations on Cubic Surfaces. The latter part of the Memoir is divided into sections headed thus:—"Section I=12, equation $(X, Y, Z, W)^3=0$ " &c. referring to the several cases of the cubic surface; but the paragraphs are numbered continuously through the Memoir.

The principal results are included in the following Table of singularities. The heading of each column shows the number and character of the case referred to, viz. C denotes a conic node, B a biplanar node, and U a uniplanar node; these being further distinguished by subscript numbers, showing the reduction thereby caused in the class of the surface: thus XIII=12-B₃-2 C₂ indicates that the case XIII is a cubic surface, the class whereof is 12-7, =5, the reduction arising from a biplanar node, B₃, reducing the class by 3, and from 2 conic nodes, C₂, each reducing the class by 2.

[illegible]

IV. "On the Blue Colour of the Sky, the Polarization of Skylight, and on the Polarization of Light by Cloudy matter generally." By JOHN TYNDALL, LL.D., F.R.S. Received December 16, 1868.

Since the communication of my brief abstract "On a new Series of Chemical Reactions produced by Light," the experiments upon this subject have been continued, and the number of the substances thus acted on considerably augmented. New relations have also been established between *mixed vapours* when subjected to the action of light.

I now beg to draw the attention of the Royal Society to two questions glanced at incidentally in the abstract referred to,—the blue colour of the sky, and the polarization of skylight. Reserving the historic treatment of the subject for a more fitting occasion, I would merely mention now that these questions constitute, in the opinion of our most eminent authorities, the two great standing enigmas of meteorology. Indeed it was the interest manifested in them by Sir John Herschel, in a letter of singular speculative power, that caused me to enter upon the consideration of these questions so soon.

The apparatus with which I work consists, as already stated to the Society, of a glass tube about a yard in length, and from $2\frac{1}{2}$ to 3 inches internal diameter. The vapour to be examined is introduced into this tube in the manner described in my last abstract, and upon it the condensed beam of the electric lamp is permitted to act until the neutrality or the activity of the substance has been declared.

It has hitherto been my aim to render the chemical action of light upon vapours *visible*. For this purpose substances have been chosen, *one* at least of whose products of decomposition under light shall have a boiling-point so high that as soon as the substance is formed it shall be *precipitated*. By graduating the quantity of the vapour, this precipitation may be rendered of any degree of fineness, forming particles distinguishable by the naked eye, or particles which are probably far beyond the reach of our highest microscopic powers.

I have no reason to doubt that particles may be thus obtained whose diameters constitute but a very small fraction of the length of a wave of violet light.

In all cases when the vapours of the liquids employed are sufficiently attenuated, no matter what the liquid may be, the visible action commences with the formation of a *blue cloud*. I would guard myself at the outset against all misconception as to the use of this term. The blue cloud to which I here refer is totally invisible in ordinary daylight. To be seen, it requires to be surrounded by darkness, *it only* being illuminated by a powerful beam of light. This blue cloud differs in many important particulars from the finest ordinary clouds, and might justly have assigned to it an intermediate position between these clouds and true cloudless vapour.

With this explanation, the term "cloud," or "incipient cloud," as I propose to employ it, cannot, I think, be misunderstood.

I had been endeavouring to decompose carbonic acid gas by light. A faint bluish cloud, due it may be, or it may not be, to the residue of some vapour previously employed, was formed in the experimental tube. On looking across this cloud through a Nicol's prism, the line of vision being horizontal, it was found that when the short diagonal of the prism was vertical, the quantity of light reaching the eye was greater than when the long diagonal was vertical.

When a plate of tourmaline was held between the eye and the bluish cloud, the quantity of light reaching the eye when the axis of the prism was perpendicular to the axis of the illuminating beam, was greater than when the axes of the crystal and of the beam were parallel to each other.

This was the result all round the experimental tube. Causing the crystal of tourmaline to revolve round the tube, with its axis perpendicular to the illuminating beam, the quantity of light that reached the eye was in all its positions a maximum. When the crystallographic axis was parallel to the axis of the beam, the quantity of light transmitted by the crystal was a minimum.

From the illuminated bluish cloud, therefore, polarized light was discharged, the direction of maximum polarization being at right angles to the illuminating beam; the *plane of vibration* of the polarized light, moreover, was that to which the beam was perpendicular*.

Thin plates of selenite or of quartz, placed between the Nicol and the bluish cloud, displayed the colours of polarized light, these colours being most vivid when the line of vision was at right angles to the experimental tube. The plate of selenite usually employed was a circle, thinnest at the centre, and augmenting uniformly in thickness from the centre outwards. When placed in its proper position between the Nicol and the cloud, it exhibited a system of splendidly coloured rings.

The cloud here referred to was the first operated upon in the manner described. It may, however, be greatly improved upon by the choice of proper substances, and by the application in proper quantities of the substances chosen. Benzol, bisulphide of carbon, nitrite of amyl, nitrite of butyl, iodide of allyl, iodide of isopropyl, and many other substances may be employed. I will take the nitrite of butyl as illustrative of the means adopted to secure the best result with reference to the present question.

And here it may be mentioned that a vapour, which when alone, or mixed with air in the experimental tube, resists the action of light, or shows but a feeble result of this action, may, by placing it in proximity with an-

* I assume here that the plane of vibration is perpendicular to the plane of polarization. This is still an undecided point; but the probabilities are so much in its favour, and it is in my opinion so much preferable to have a physical image on which the mind can rest, that I do not hesitate to employ the phraseology in the text. Even should the assumption prove to be incorrect, no harm will be done by the provisional use of it.

other gas or vapour, be caused to exhibit under light vigorous, if not violent action. The case is similar to that of carbonic acid gas, which diffused in the atmosphere resists the decomposing action of solar light, but when placed in contiguity with the chlorophyl in the leaves of plants, has its molecules shaken asunder.

Dry air was permitted to bubble through the liquid nitrite of butyl until the experimental tube, which had been previously exhausted, was filled with the mixed air and vapour. The visible action of light upon the mixture after fifteen minutes' exposure was slight. The tube was afterwards filled with half an atmosphere of the mixed air and vapour, and another half atmosphere of air which had been permitted to bubble through fresh commercial hydrochloric acid. On sending the beam through this mixture, the action paused barely sufficiently long to show that at the moment of commencement the tube was optically empty. But the pause amounted only to a small fraction of a second, a dense cloud being immediately precipitated upon the beam which traversed the mixture.

This cloud began *blue*, but the advance to whiteness was so rapid as almost to justify the application of the term instantaneous. The dense cloud, looked at perpendicularly to its axis, showed scarcely any signs of polarization. Looked at obliquely the polarization was strong.

The experimental tube being again cleansed and exhausted, the mixed air and nitrite-of-butyl vapour was permitted to enter it until the associated mercury column was depressed $\frac{1}{10}$ of an inch. In other words, the air and vapour, united, exercised a pressure not exceeding $\frac{1}{300}$ of an atmosphere. Air passed through a solution of hydrochloric acid was then added till the mercury column was depressed three inches. The condensed beam of the electric light passed for some time in darkness through this mixture. There was absolutely nothing within the tube competent to scatter the light. Soon, however, a superbly blue cloud was formed along the track of the beam, and it continued blue sufficiently long to permit of its thorough examination. The light discharged from the cloud at right angles to its own length was *perfectly* polarized. By degrees the cloud became of whitish blue, and for a time the selenite colours obtained by looking at it normally were exceedingly brilliant. The direction of maximum polarization was distinctly at right angles to the illuminating beam. This continued to be the case as long as the cloud maintained a decided blue colour, and even for some time after the pure blue had changed to whitish blue. But as the light continued to act the cloud became coarser and whiter, particularly at its centre, where it at length ceased to discharge polarized light in the direction of the perpendicular, while it continued to do so at both its ends.

But the cloud which had thus ceased to polarize the light emitted normally, showed vivid selenite colours when looked at *obliquely*. The direction of maximum polarization changed with the texture of the cloud. This point shall receive further illustration subsequently.

A blue, equally rich and more durable, was obtained by employing the

nitrite-of-butyl vapour in a still more attenuated condition. Now the instance here cited is *representative*. In all cases, and with all substances, the cloud formed at the commencement, when the precipitated particles are sufficiently fine, is *blue*, and it can be made to display a colour rivalling that of the purest Italian sky. In all cases, moreover, this fine blue cloud polarizes *perfectly* the beam which illuminates it, the direction of polarization enclosing an angle of 90° with the axis of the illuminating beam.

It is exceedingly interesting to observe both the perfection and the decay of this polarization. For ten or fifteen minutes after its first appearance the light from a vividly illuminated incipient cloud, looked at horizontally, is absolutely quenched by a Nicol's prism with its longer diagonal vertical. But as the sky-blue is gradually rendered impure by the introduction of particles of too large a size, in other words, as real clouds begin to be formed, the polarization begins to deteriorate, a portion of the light passing through the prism in all its positions. It is worthy of note that for some time after the cessation of perfect polarization the *residual* light which passes, when the Nicol is in its position of minimum transmission, is of a gorgeous blue, the whiter light of the cloud being extinguished*. When the cloud texture has become sufficiently coarse to approximate to that of ordinary clouds, the rotation of the Nicol ceases to have any sensible effect on the quality of the light discharged normally.

The perfection of the polarization in a direction perpendicular to the illuminating beam is also illustrated by the following experiment. A Nicol's prism large enough to embrace the entire beam of the electric lamp was placed between the lamp and the experimental tube. A few bubbles of air carried through the liquid nitrite of butyl were introduced into the tube, and they were followed by about 3 inches (measured by the mercurial gauge) of air which had been passed through aqueous hydrochloric acid. Sending the polarized beam through the tube, I placed myself in front of it, my eye being on a level with its axis, my assistant Mr. Cottrell occupying a similar position behind the tube. The short diagonal of the large Nicol was in the first instance vertical, the plane of vibration of the emergent beam being therefore also vertical. As the light continued to act, a superb blue cloud visible to both my assistant and myself was slowly formed. But this cloud, so deep and rich when looked at from the positions mentioned, *utterly disappeared when looked at vertically downwards, or vertically upwards*. Reflection from the cloud was not possible in these directions. When the large Nicol was slowly turned round its axis, the eye of the observer being on the level of the beam, and the line of vision perpendicular to it, entire extinction of the light emitted horizontally occurred where the longer diagonal of the large Nicol was vertical. But now a vivid blue cloud was seen when looked at downwards or upwards. This truly fine experiment was first definitely suggested by a remark addressed to me in a letter by Prof. Stokes.

* This seems to prove that particles too large to polarize the blue, polarize perfectly light of lower refrangibility.

Now, as regards the polarization of skylight, the greatest stumblingblock has hitherto been that, in accordance with the law of Brewster, which makes the index of refraction the tangent of the polarizing angle, the reflection which produces perfect polarization would require to be made *in air upon air*; and indeed this led many of our most eminent men, Brewster himself among the number, to entertain the idea of *molecular reflection*. I have, however, operated upon substances of widely different refractive indices, and therefore of very different polarizing angles as ordinarily defined, but the polarization of the beam by the incipient cloud has thus far proved itself to be *absolutely independent of the polarizing angle*. The law of Brewster does not apply to matter in this condition, and it rests with the undulatory theory to explain why. Whenever the precipitated particles are sufficiently fine, no matter what the substance forming the particles may be, the direction of maximum polarization is at right angles to the illuminating beam, the polarizing angle for matter in this condition being invariably 45° . This I consider to be a point of capital importance with reference to the present question*.

That *water-particles*, if they could be obtained in this exceedingly fine state of division, would produce the same effects, does not admit of reasonable doubt. And that they must exist in this condition in the higher regions of the atmosphere is, I think, certain. At all events, no other assumption than this is necessary to completely account for the firmamental blue and the polarization of the sky†.

Suppose our atmosphere surrounded by an envelope impervious to light, but with an aperture on the sunward side through which a parallel beam of solar light could enter and traverse the atmosphere. Surrounded on all sides by air not directly illuminated, the track of such a beam through the air would resemble that of the parallel beam of the electric lamp through an incipient cloud. The sunbeam would be *blue*, and it would discharge laterally light in precisely the same condition as that discharged by the in-

* The difficulty referred to above is thus expressed by Sir John Herschel:—"The cause of the polarization is evidently a reflection of the sun's light upon *something*. The question is on what? Were the angle of maximum polarization 76° , we should look to water or ice as the reflecting body, however inconceivable the existence in a cloudless atmosphere, and a hot summer's day of unevaporated molecules (particles?) of water. But though we were once of this opinion, careful observation has satisfied us that 90° , or thereabouts, is a correct angle, and that therefore whatever be the body on which the light has been reflected, *if polarized by a single reflection*, the polarizing angle must be 45° , and the index of refraction, which is the tangent of that angle, unity; in other words, the reflection would require to be made *in air upon air*!" ('Meteorology,' par. 233).

† Any particles, if small enough, will produce both the colour and the polarization of the sky. But is the existence of small water-particles on a hot summer's day *in the higher regions of our atmosphere* inconceivable? It is to be remembered that the oxygen and nitrogen of the air behave as a vacuum to radiant heat, the exceedingly attenuated vapour of the higher atmosphere being therefore in practical contact with the cold of space.

ipient cloud. In fact the azure revealed by such a beam would be to all intents and purposes that which I have called a "blue cloud" *.

But, as regards the polarization of the sky, we know that not only is the direction of maximum polarization at right angles to the track of the solar beams, but that at certain angular distances, probably variable ones, from the sun, "neutral points," or points, of no polarization exist, on both sides of which the planes of atmospheric polarization are at right angles to each other.

I have made various observations upon this subject which I reserve for the present; but pending the more complete examination of the question the following facts and observations bearing upon it are submitted to the Royal Society.

The parallel beam employed in these experiments tracked its way through the laboratory air exactly as sun-beams are seen to do in the dusty air of London. I have reason to believe that a great portion of the matter thus floating in the laboratory air consists of organic germs, which are capable of imparting a perceptibly bluish tint to the air. This air showed, though far less vividly, all the effects of polarization obtained with the incipient clouds. The light discharged laterally from the track of the illuminating beam was polarized, though not perfectly, the direction of maximum polarization being at right angles to the beam.

The horizontal column of air thus illuminated was 18 feet long, and could therefore be looked at very obliquely without any disturbance from a solid envelope. At all points of the beam throughout its entire length the light emitted normally was in the same state of polarization. Keeping the positions of the Nicol and the selenite constant, the same colours were observed throughout the entire beam when the line of vision was perpendicular to its length.

I then placed myself near the end of the beam as it issued from the electric lamp, and looking through the Nicol and selenite more and more obliquely at the beam, observed the colours fading until they disappeared. Augmenting the obliquity the colours appeared once more, *but they were now complementary to the former ones*.

Hence this beam, like the sky, exhibited its neutral point, at opposite sides of which the light was polarized in planes at right angles to each other.

Thinking that the action observed in the laboratory might be caused in

* The opinion of Sir John Herschel, connecting the polarization and the blue colour of the sky is verified by the foregoing results. "The more the subject [the polarization of skylight] is considered," writes this eminent philosopher, "the more it will be found beset with difficulties, and its explanation when arrived at will probably be found to carry with it that of the blue colour of the sky itself and of the great quantity of light it actually does send down to us." "We may observe, too," he adds, "that it is only where the purity of the sky is most absolute that the polarization is developed in its highest degree, and that where there is the slightest perceptible tendency to cirrus it is materially impaired." This applies word for word to the "incipient clouds."

some way by the vaporous fumes diffused in its air, I had a battery and an electric lamp carried to a room at the top of the Royal Institution. The track of the beam was seen very finely in the air of this room, a length of 14 or 15 feet being attainable. This beam exhibited all the effects observed with the beam in the laboratory. Even the uncondensed electric light falling on the floating matter showed, though faintly, the effects of polarization*.

When the air was so sifted as to entirely remove the visible floating matter, it no longer exerted any sensible action upon the light, but behaved like a vacuum.

I had varied and confirmed in many ways those experiments on neutral points, operating upon the fumes of chloride of ammonium, the smoke of brown paper, and tobacco smoke, when my attention was drawn by Sir Charles Wheatstone to an important observation communicated to the Paris Academy in 1860 by Professor Govi, of Turin†. His observations on the light of comets had led M. Govi to examine a beam of light sent through a room in which was diffused the smoke of incense. He also operated on tobacco smoke. His first brief communication stated the fact of polarization by such smoke, but in his second communication he announced the discovery of a neutral point in the beam, at the opposite sides of which the light was polarized in planes at right angles to each other.

But unlike my observations on the laboratory air, and unlike the action of the sky, the direction of maximum polarization in M. Govi's experiment enclosed a very small angle with the axis of the illuminating beam. The question was left in this condition, and I am not aware that M. Govi or any other investigator has pursued it further.

I had noticed, as before stated, that as the clouds formed in the experimental tube became denser, the polarization of the light discharged at right angles to the beam became weaker, the direction of maximum polarization becoming oblique to the beam. Experiments on the fumes of chloride of ammonium gave me also reason to suspect that the position of the neutral point *was not constant*, but that it varied with the density of the illuminated fumes.

The examination of these questions led to the following new and remarkable results:—the laboratory being well filled with the fumes of incense, and sufficient time being allowed for their uniform diffusion, the electric beam was sent through the smoke. From the track of the beam polarized light was discharged, but the direction of maximum polarization, instead of being along the normal, now enclosed an angle of 12° or 13° with the axis of the beam.

A neutral point, with complementary effects at opposite sides of it, was also exhibited by the beam. The angle enclosed by the axis of the beam, and a line drawn from the neutral point to the observer's eye, measured in the first instance 66° .

* I hope to try Alpine air next summer.

† Comptes Rendus, tome li. pp. 360 & 669.

The windows of the laboratory were now opened for some minutes, a portion of the incense smoke being permitted to escape. On again darkening the room and turning on the beam, the line of vision to the neutral point was found to enclose with the axis of the beam an angle of 63° .

The windows were again opened for a few minutes, more of the smoke being permitted to escape. Measured as before the angle referred to was found to be 54° .

This process was repeated three additional times; the neutral point was found to recede lower and lower down the beam, the angle between a line drawn from the eye to the neutral point and the axis of the beam falling successively from 54° to 49° , 43° and 33° .

The distances, roughly measured, of the neutral point from the lamp, corresponding to the foregoing series of observations, were these:—

1st observation	2	feet	2	inches.
2nd	„	2	„	6 „
3rd	„	2	„	10 „
4th	„	3	„	2 „
5th	„	3	„	7 „
6th	„	4	„	6 „

At the end of this series of experiments the direction of maximum polarization had again become normal to the beam.

The laboratory was next filled with the fumes of gunpowder. In five successive experiments, corresponding to five different densities of the gunpowder smoke, the angles enclosed between the line of vision to the neutral point and the axis of the beam were 63° , 50° , 47° , 42° , and 38° respectively.

After the clouds of gunpowder had cleared away the laboratory was filled with the fumes of common resin, rendered so dense as to be very irritating to my lungs. The direction of maximum polarization enclosed in this case an angle of 12° , or thereabouts, with the axis of the beam. Looked at, as in the former instances, from a position near the electric lamp *no neutral point* was observed throughout the entire extent of the beam.

When this beam was looked at normally through the selenite and Nicol, the ring system, though not brilliant, was distinct. Keeping the eye upon the plate of selenite and the line of vision normal, the windows were opened, the blinds remaining undrawn. The resinous fumes slowly diminished, and as they did so the ring system became paler. It finally disappeared. Continuing to look along the perpendicular, the rings revived, but now the colours were complementary to the former ones. *The neutral point had passed me in its motion down the beam consequent upon the attenuation of the fumes of resin.*

In the fumes of chloride of ammonium substantially the same results were obtained as those just described. Sufficient I think has been here stated to illustrate the variability of the position of the neutral point. The

explanation of the results will probably give new work to the undulatory theory*.

Before quitting the question of the reversal of the polarization by cloudy matter, I will make one or two additional observations. Some of the clouds formed in the experiments on the chemical action of light are astonishing as to form. The experimental tube is often divided into segments of dense cloud, separated from each other by nodes of finer matter. Looked at normally, as many as four reversals of the plane of polarization have been found in the tube in passing from node to segment, and from segment to node. With the fumes diffused in the laboratory, on the contrary, there was no change in the polarization along the normal, for here the necessary differences of cloud-texture did not exist.

Further. By a puff of tobacco smoke or of condensed steam blown into the illuminated beam, the brilliancy of the colours may be greatly augmented. But with different clouds two different effects are produced. For example, let the ring system observed in the common air be brought to its maximum strength, and then let an attenuated cloud of chloride of ammonium be thrown into the beam at the point looked at; the ring system flashes out with augmented brilliancy, and the character of the polarization remains unchanged. This is also the case when phosphorus or sulphur is burned underneath the beam, so as to cause the fine particles of phosphoric acid or of sulphur to rise into the light. With the sulphur-fumes the brilliancy of the colours is exceedingly intensified; but in none of these cases is there any change in the character of the polarization.

But when a puff of aqueous cloud, or of the fumes of hydrochloric acid, hydriodic acid, or nitric acid is thrown into the beam, there is a complete reversal of the selenite tints. Each of these clouds twists the plane of polarization 90° . On these and kindred points experiments are still in progress†.

The idea that the colour of the sky is due to the action of finely divided matter, rendering the atmosphere a turbid medium, through which we look at the darkness of space, dates as far back as Leonardo da Vinci. Newton conceived the colour to be due to exceedingly small water particles acting as thin plates. Goethe's experiments in connexion with this subject are well known and exceedingly instructive. One very striking observation of Goethe's referred to what is technically called "chill" by painters, which is due no doubt to extremely fine varnish particles interposed between the eye and a dark background. Clausius, in two very able memoirs,

* Brewster has proved the variability of the position of the neutral point for skylight with the sun's altitude. Is not the proximate cause of this revealed by the foregoing experiments?

† Sir John Herschel has suggested to me that this change of the polarization from positive to negative may indicate a change from polarization by reflection to polarization by refraction. This thought repeatedly occurred to me while looking at the effects; but it will require much following up before it emerges into clearness.

endeavoured to connect the colours of the sky with suspended water-vesicles, and to show that the important observations of Forbes on condensing steam could also be thus accounted for. Bruecke's experiments on precipitated mastic were referred to in my last abstract. Helmholtz has ascribed the blueness of the eyes to the action of suspended particles. In an article written nearly nine years ago by myself, the colours of the peat smoke of the cabins of Killarney* and the colours of the sky were referred to one and the same cause, while a chapter of the "Glaciers of the Alps," published in 1860, is also devoted to this question. Roscoe, in connexion with his truly beautiful experiments on the photographic power of sky-light, has also given various instances of the production of colour by suspended particles. In the foregoing experiments the azure was produced in *air*, and exhibited a depth and purity far surpassing anything that I have ever seen in mote-filled liquids. Its polarization, moreover, was *perfect*.

In his experiments on fluorescence Professor Stokes had continually to separate the light reflected from the motes suspended in his liquids, the action of which he named "false dispersion," from the fluorescent light of the same liquids, which he ascribed to "true dispersion." In fact it is hardly possible to obtain a liquid without motes, which polarize by reflection the light falling upon them, truly dispersed light being unpolarized. At p. 530 of his celebrated memoir "On the Change of the Refrangibility of Light," Prof. Stokes adduces some significant facts, and makes some noteworthy remarks, which bear upon our present subject. He notices more particularly a specimen of plate glass which, seen by reflected light, exhibited a blue which was exceedingly like an effect of fluorescence, but which, when properly examined, was found to be an instance of false dispersion. "It often struck me," he writes, "while engaged in these observations, that when the beam had a continuous appearance, the polarization was more nearly perfect than when it was sparkling, so as to force on the mind the conviction that it arose merely from motes†. Indeed in the former case the polarization has often appeared perfect, or all but perfect. It is possible that this may in some measure have been due to the circumstance, that when a given quantity of light is diminished in a given ratio, the illumination is perceived with more difficulty when the light is diffused uniformly, than when it is spread over the same space, but collected into specks. Be this as it may, there was at least no tendency observed towards polarization in a plane perpendicular

* I have sometimes quenched almost completely, by a Nicol, the light discharged normally from burning leaves in Hyde Park. The blue smoke from the *ignited end* of a cigar polarizes also, but not perfectly.

† The azure may be produced in the midst of a field of motes. By turning the Nicol, the interstitial blue may be completely quenched, the shining, and apparently unaffected motes, remaining masters of the field. A blue cloud, moreover, may be precipitated in the midst of the azure. An aqueous cloud thus precipitated reverses the polarization; but on the melting away of the cloud the azure and its polarization remain behind.

to the plane of reflection, when the suspended particles became finer, and therefore the beam more nearly continuous."

Through the courtesy of its owner, I have been permitted to see and to experiment with the piece of plate glass above referred to. Placed in front of the electric lamp, whether edgeways or transversely, it discharges bluish polarized light laterally, the colour being by no means a bad imitation of the blue of the sky.

Prof. Stokes considers that this deportment may be invoked to decide the question of the direction of the vibrations of polarized light. On this point I would say, if it can be demonstrated that when the particles are small in comparison to the length of a wave of light, the vibrations of a ray reflected by such particles cannot be perpendicular to the vibrations of the incident light; then assuredly the experiments recorded in the foregoing communication decide the question in favour of Fresnel's assumption.

As stated above, almost all liquids have motes in them sufficiently numerous to polarize sensibly the light, and very beautiful effects may be obtained by simple artificial devices. When, for example, a cell of distilled water is placed in front of the electric lamp, and a slice of the beam permitted to pass through it, scarcely any polarized light is discharged, and scarcely any colour produced with a plate of selenite. But while the beam is passing through it, if a bit of soap be agitated in the water above the beam, the moment the infinitesimal particles reach the beam the liquid sends forth laterally almost perfectly polarized light; and if the selenite be employed, vivid colours flash into existence. A still more brilliant result is obtained with mastic dissolved in a great excess of alcohol.

The selenite rings constitute an extremely delicate test as to the quantity of motes in a liquid. Commencing with distilled water, for example, a thickish beam of light is necessary to make the polarization of its motes sensible. A much thinner beam suffices for common water; while with Brücke's precipitated mastic, a beam too thin to produce any sensible effect with most other liquids, suffices to bring out vividly the selenite colours.

January 21, 1869.

JOHN PETER GASSIOT, Esq., Vice-President, in the Chair.

The Chairman stated that Sir John Macneill and Mr. Edward Solly, who, by reason of non-payment of their annual contributions, ceased to be Fellows of the Society at the last Anniversary, had applied for readmission. Extracts from their letters, explaining the circumstances under which non-payment had occurred, were read, and notice was given that the question of their readmission would be put to the vote at the next Meeting.

The following communications were read :—

I. "On the Thermal Resistance of Liquids." By FREDERICK GUTHRIE, F.C.S. Communicated by Dr. TYNDALL. Received October 16, 1868.

(Abstract.)

The memoir of which the following is an abstract gives an account of some experiments made by the author with the object of determining the laws according to which heat travels by conduction through liquids.

After pointing out the importance of the subject, and briefly recapitulating the methods previously used and the results obtained by other experimenters, the "Diathermometer" is described.

This instrument, which may be employed for the examination of the thermal resistance or conducting power of solids as well as liquids, has the following form. A hollow brass cone, having a platinum base, is screwed with its apex downwards into a tripod-stand which rests upon adjusting screws. The apex of the cone is tubular, and carries a cock, through which passes a vertical glass tube graduated and dipping into water. The level of the water in the tube is nearly as high as the apex of the cone. By means of a micrometer screw, a second cone, exactly similar and equal to the first, having its apex upwards, may be brought to any required distance from the lower cone. The brass cones and their platinum faces are highly polished, and the latter are cleaned by washing successively with hot nitric acid, caustic soda, alcohol, and water. The upper surface of the lower cone is brought into an exactly horizontal position, and the upper cone is lowered to any required distance from it. There is thus formed between the platinum faces a cylindrical interval of known height or thickness and diameter, and having its opposite faces parallel and horizontal. This wall-less chamber receives the liquid whose thermal resistance has to be measured. A liquid, introduced by means of a strangulated pipette of known capacity (equal, say, to the interstitial space when the cone-faces are 1 millim. apart) between the cones, remains there by means of its adhesion and cohesion. A description is given of the method used to get a constant current of water of uniform and known temperature to pass through the upper cone. When such a current passes, the platinum face of the upper cone becomes heated; it communicates its heat to the liquid in contact with it. The heat passes downwards through the liquid, heats the upper surface of the lower cone, expands the air therein, and depresses the level of the water in the tube attached to the lower cone.

A description is given of the most prominent sources of error of this instrument, and the means which were employed to eliminate them. It is concluded, from direct experiments (1st, by measuring the time required for the production of the first heat-effect in the lower cone; 2ndly, by showing the smallness of the difference caused by the introduction of athermanous disks), and from comparison with recent results of Magnus, that the effect of radiation in all the cases tried is negligible if not nothing.

By measuring resistance rather than conductivity, several sources of

experimental error are eliminated. If the two cones are brought into actual contact, and water of a known temperature is led for a given time through the upper cone, a certain thermal effect is produced in the lower one. If the cones be then separated, and a liquid be interposed between them, and if water of the same temperature as before be led for the same time as before through the upper cone, a less thermal effect is produced. *The difference between the two effects is a measure of the resistance of the liquid.* Results so obtained have to be corrected for the varying pressure to which the air in the lower cone is subjected as the water in the glass tube sinks. To find the absolute results in thermal units, we have to take into account the diameter of the surface of the lower cone, its capacity, and the specific heat of the air which is in it.

The following are the chief results obtained :—

(1) The connexion in the instance of water between the thickness and the time required for the first-heat effect.

(2) The connexion between the temperature and the time required for the first-heat effect. It is shown that hotter water conducts heat better than colder ; and that the hotter the conducting-water, the greater is the difference in rate.

(3) The connexion between the entire quantity of heat passing in a given time and the thickness and temperature of the conducting-water.

(4) The effect of the solution of various salts in altering the thermal resistance of water. Every salt tried was found, when dissolved in water, to increase its thermal resistance. The author submits that the effect of the dissolved salt is chiefly, perhaps wholly, due to the displacement of a portion of the water by a substance having greater resistance, and to the modification in the specific heat of the liquid, caused by the introduction of the salt.

(5) The resistance of the liquids in the following list was examined under precisely similar circumstances. The thickness was in each case 1 millim. The initial temperature of the liquid was $20^{\circ}\cdot 17\text{ C.}$, and the temperature-difference, ΔT , was 10°C. That is, the platinum surface of the upper cone was maintained at $30^{\circ}\cdot 17\text{ C.}$ The duration of the experiment in each case was 1'. The numbers show the specific resistance under the above circumstances, that is, the ratio between the quantities of heat arrested by the several liquids and that arrested by water.

Liquid.	Specific resistance.	Liquid.	Specific resistance.
Water	1	Acetate of amyl	10·00
Glycerine.....	3·84	Amylamin	10·14
Acetic acid (glacial)	8·38	Amylic alcohol	10·23
Acetone	8·51	Oil of turpentine	11·75
Oxalate of ethyl	8·85	Nitrate of butyl	11·87
Sperm-oil	8·85	Chloroform.....	12·10
Alcohol	9·08	Bichloride of carbon	12·92
Acetate of ethyl	9·08	Mercury amyl	12·92
Nitrobenzol.....	9·86	Bromide of ethylen	13·16
Oxalate of amyl	10·06	Iodide of amyl	13·27
Butylic alcohol	10·00	Iodide of ethyl (?)	?

The more salient points of these results are pointed out, such as the preeminently small resistance of water and of bodies containing a large proportion of the elements of water (potential water); the possible connexion of this fact with the results of Magnus concerning the conductivity of hydrogen; the increased resistance accompanying increased molecular complexity in the case of isotypic liquids, as exemplified by the alcohols and their derivatives: the great resistance shown by the liquids containing halogens. The results obtained by Tyndall in regard to relative diathermancy are shown to be in accord with the author's results concerning resistance. *A highly diathermanous liquid invariably offers great resistance to conducted heat.* The relation between electrical and thermal resistance in the case of liquids is also briefly discussed.

II. "Results of a preliminary Comparison of certain Curves of the Kew and Stonyhurst Declination Magnetographs." By the Rev. W. SIDGREAVES and BALFOUR STEWART, LL.D., F.R.S. Received October 28, 1868.

The observatories of Kew and Stonyhurst are not far apart, both being in England; the first in the county of Surrey (Lat. $51^{\circ} 28' 6''$ N., Long. $0^{\circ} 18' 47''$ W.), and the second in the county of Lancashire (Lat. $53^{\circ} 50' 40''$ N., Long. $2^{\circ} 28' 10'' \cdot 2$ W.).

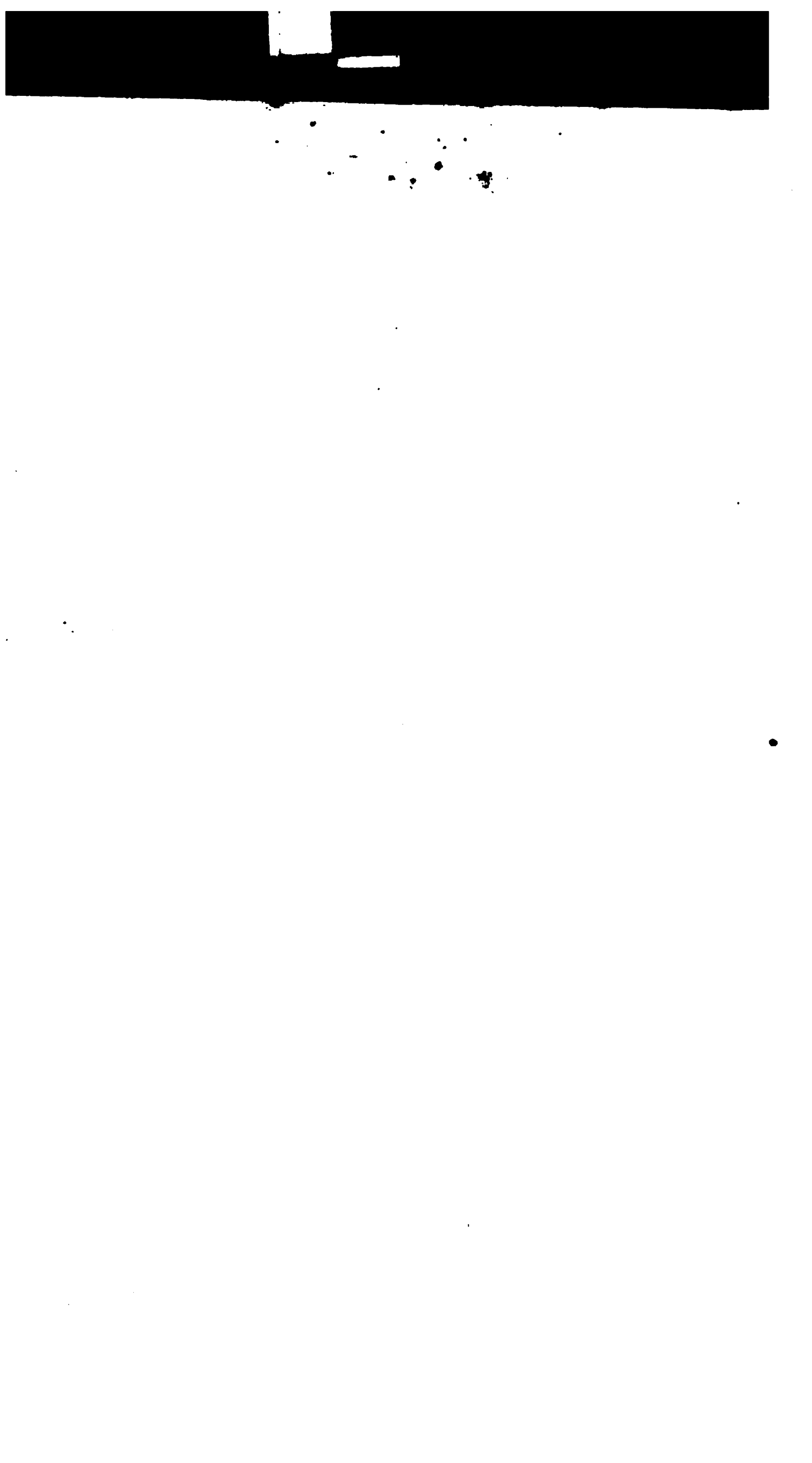
If we bear in mind (as a fact well proved, chiefly by the researches of General Sabine) that magnetic disturbances are of a cosmical nature, we cannot evidently expect any considerable difference between these two stations, and it might be very naturally supposed that the magnetic variations should be precisely the same in each.

This is no doubt approximately true, but nevertheless there is on certain occasions a residual difference between the indications of the two places, and one which is caught by the eye from the automatic records with very great ease, inasmuch as the instrumental time-scale of these is precisely the same for both places; and not only is the time-scale the same, but *for slow disturbances* the vertical spaces traversed by the traces are the same for both declination magnetographs.

We venture to bring before the Royal Society certain results of an inter-comparison of the declination curves of these two observatories, although only of a preliminary nature, because the subject is one of much interest, and because these results appear to exhibit, superposed upon a disturbance which is mainly cosmical, a comparatively small effect, which appears to be more of a local nature, but which is not unworthy of investigation.

The records which we have investigated are represented graphically in Plates III. and IV.; and in them the disturbances which have been measured are denoted by figures attached to their extremities.

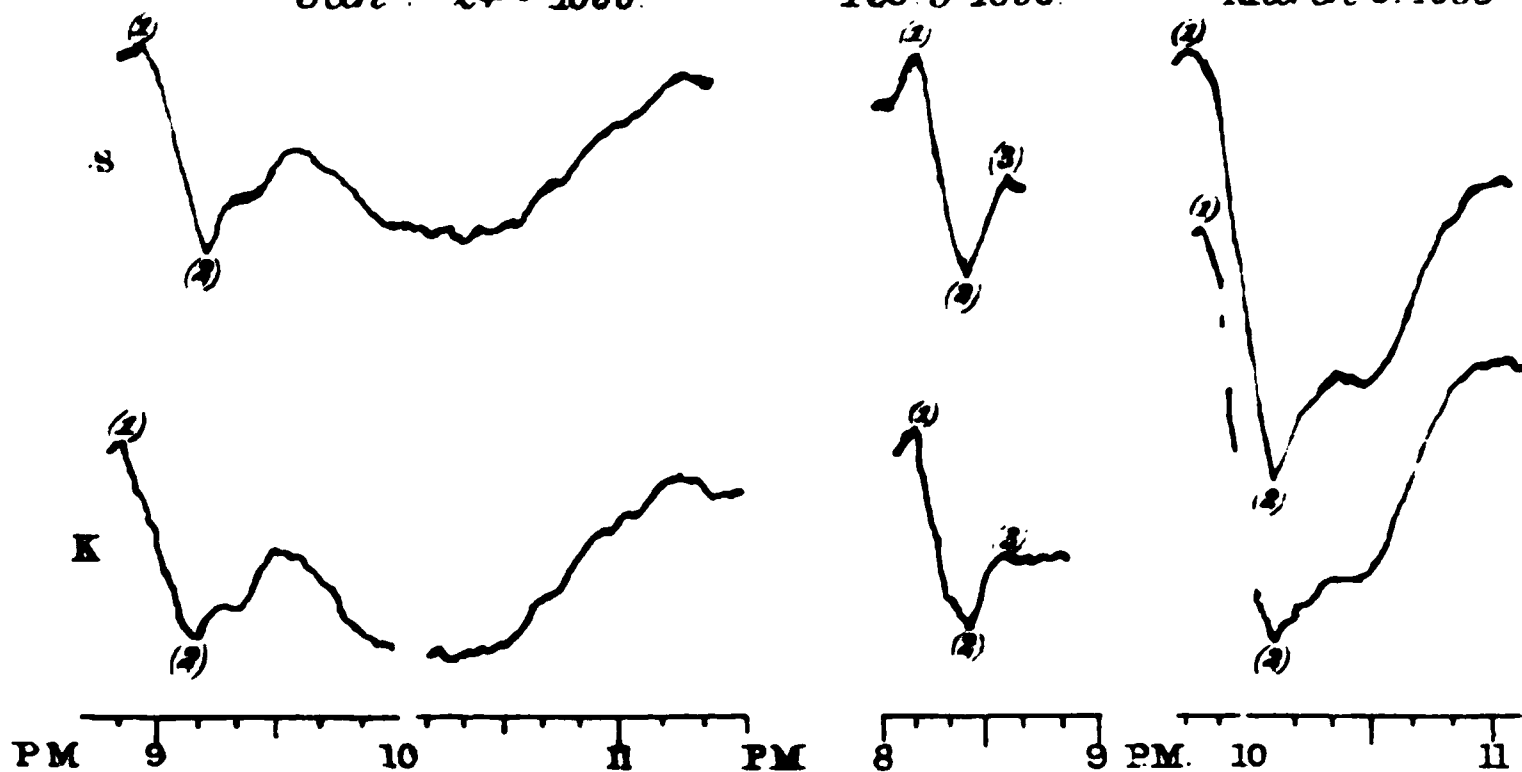
The following Table exhibits the results of these measurements:—



Jan 7 24th 1868.

Feb 5 1868.

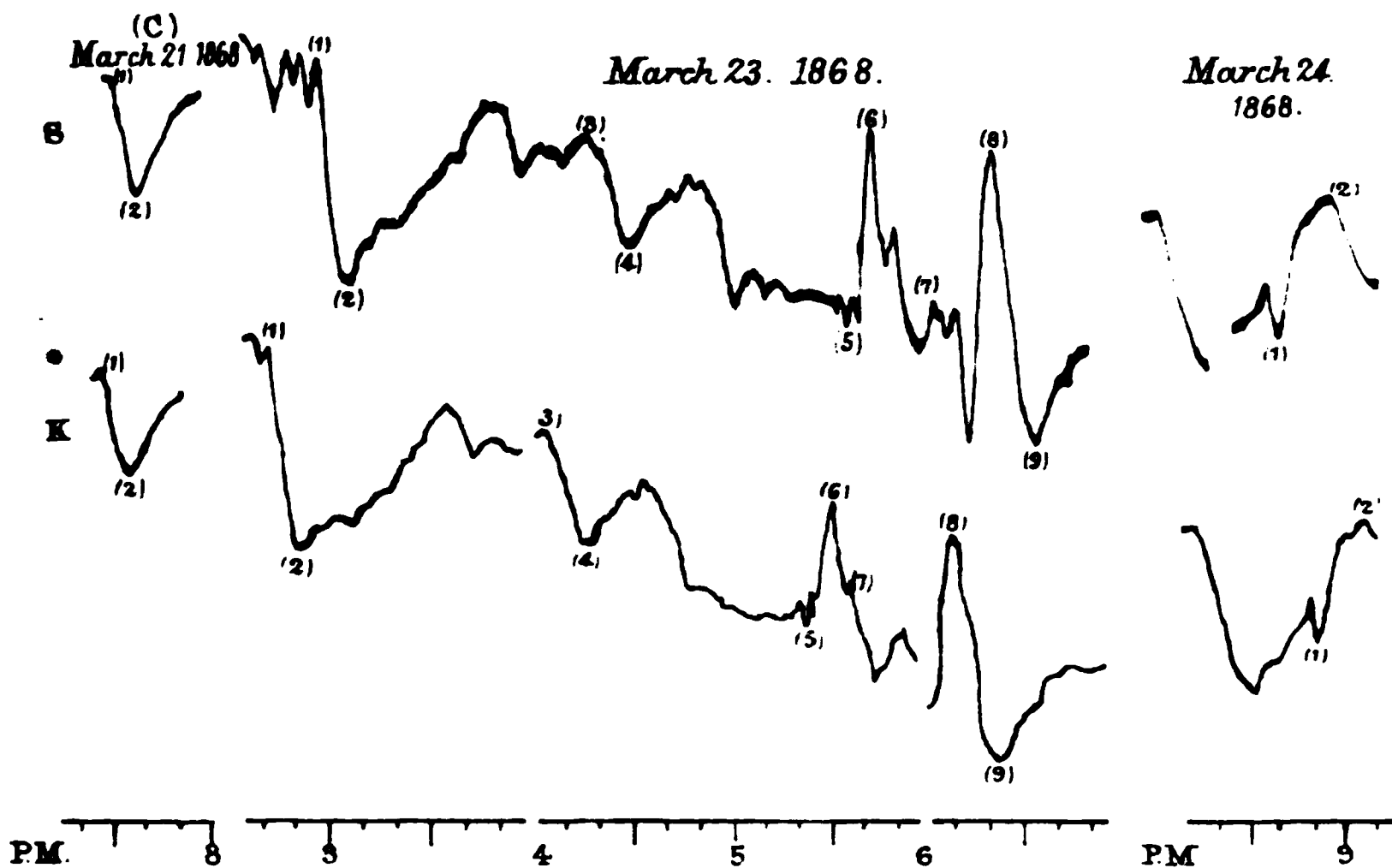
March 6. 1868



(C)
March 21 1868

March 23. 1868.

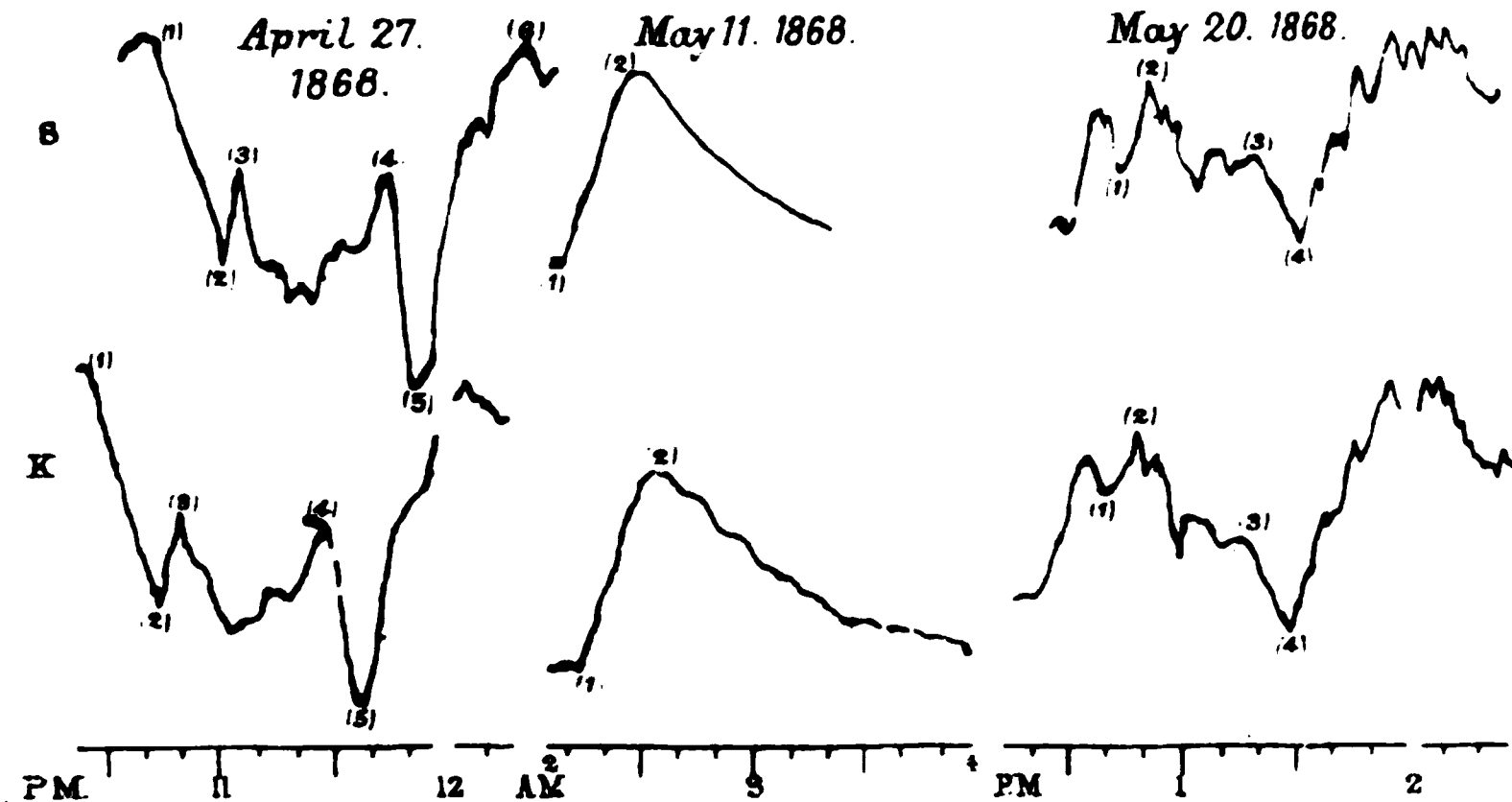
March 24.
1868.



April 27.
1868.

May 11. 1868.

May 20. 1868.

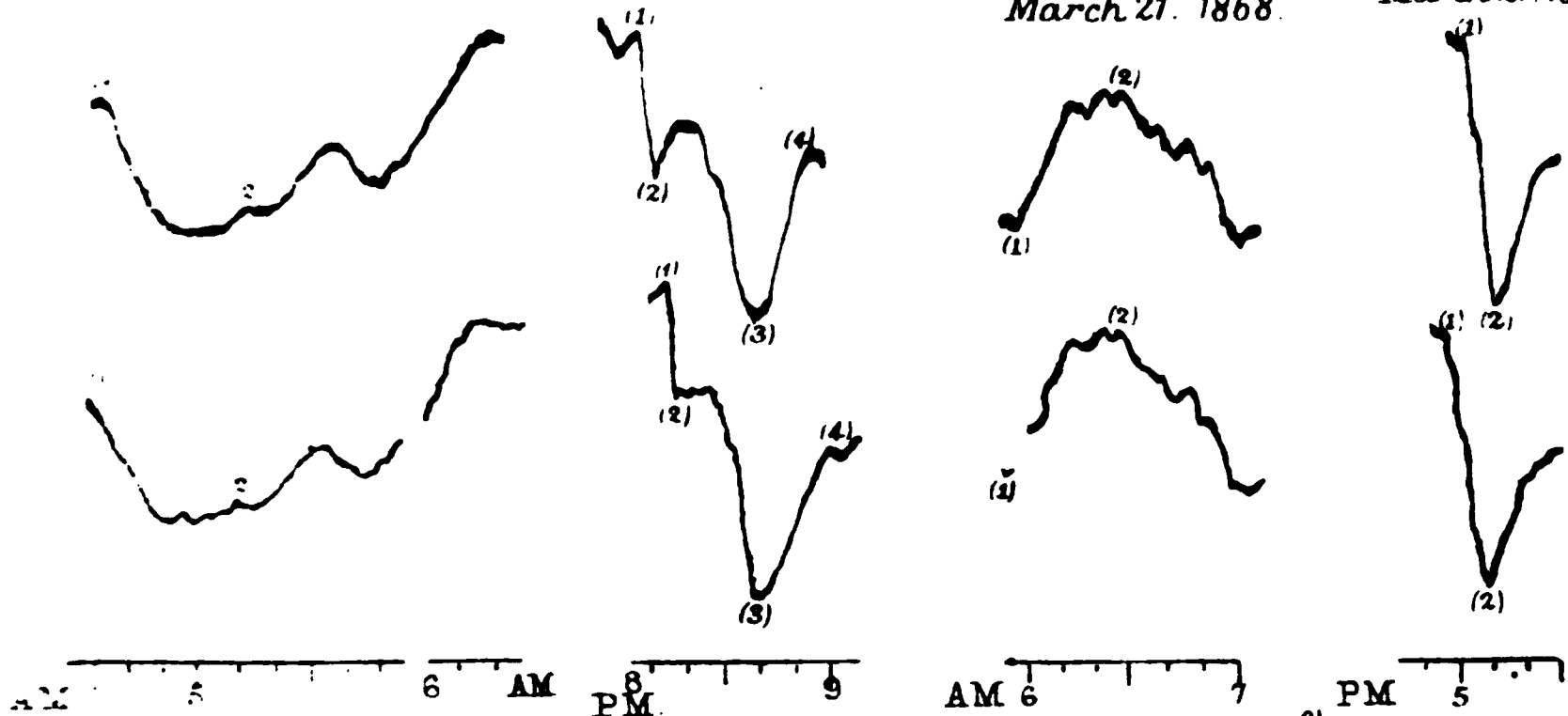


March 17. 1868.

(B)
March 20. 1868.

(A)
March 21. 1868.

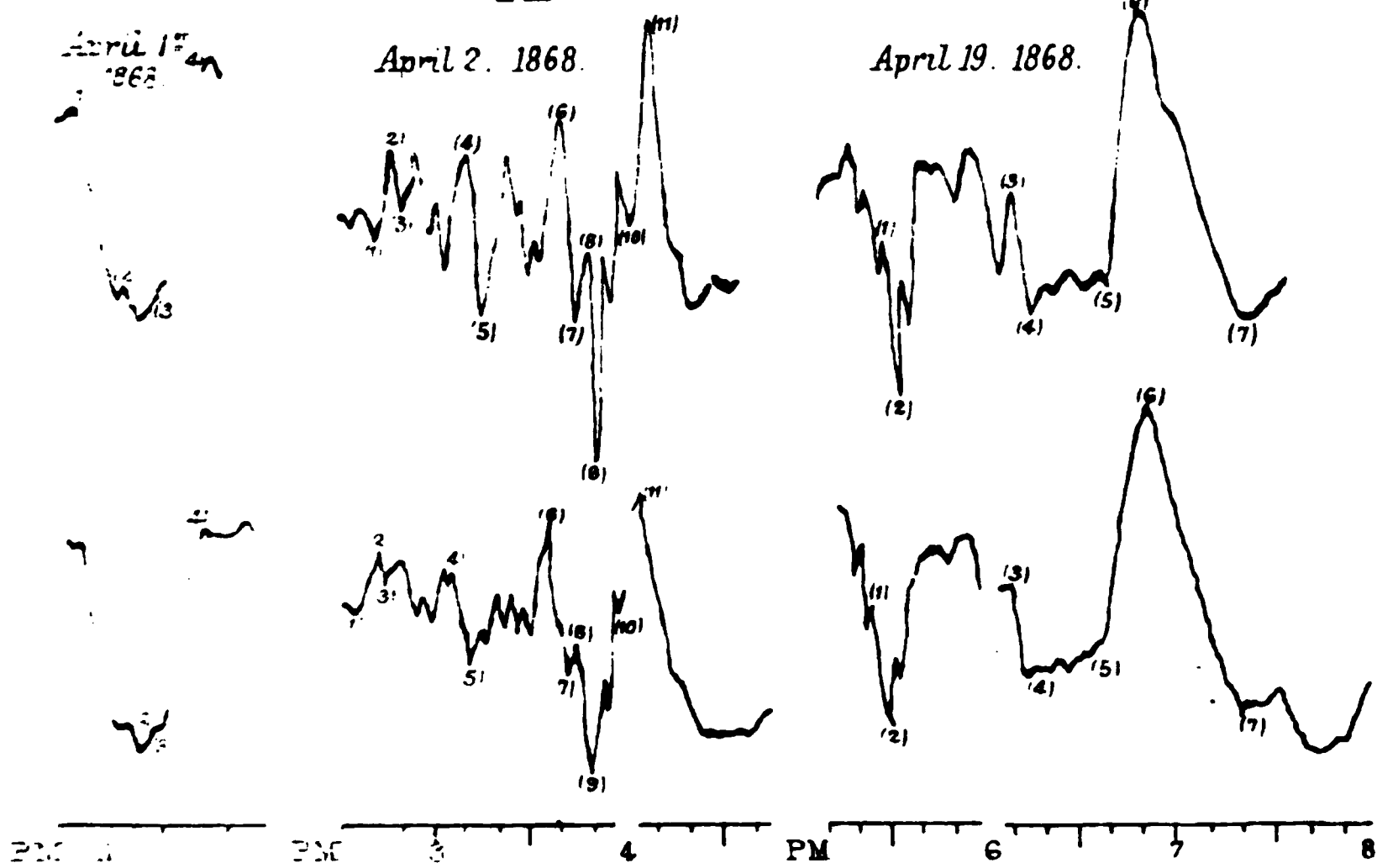
(B)
March 21. 1868.



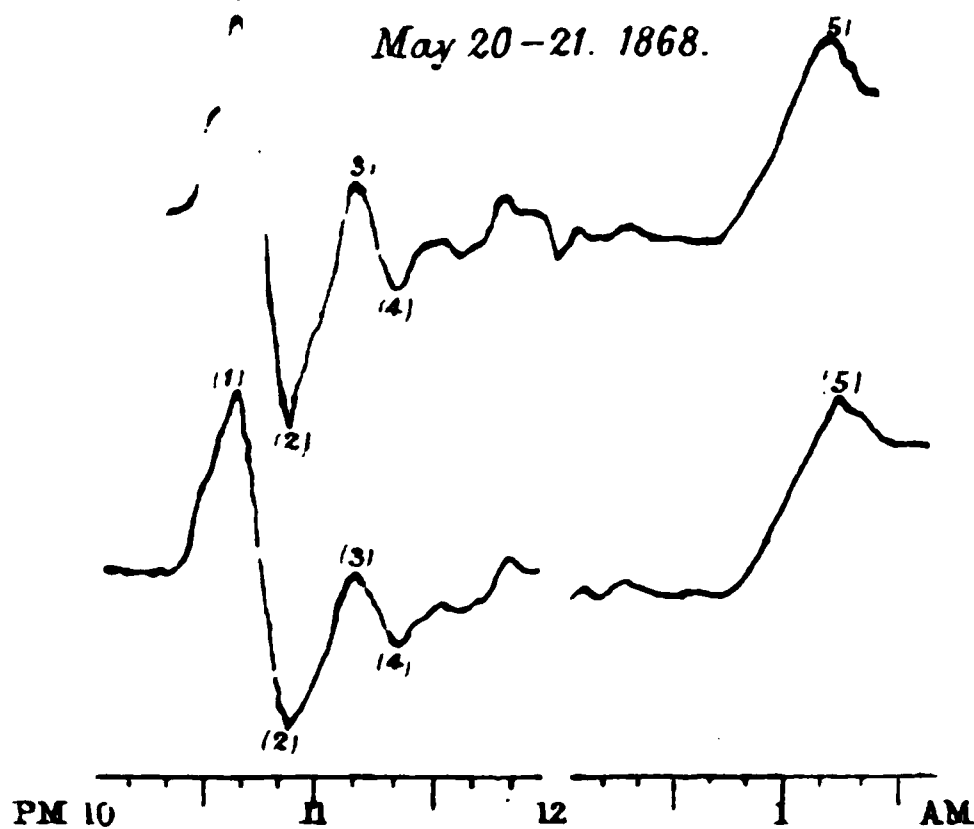
April 1st. 1868.

April 2. 1868.

April 19. 1868.



May 20-21. 1868.





Date (see Plate).	Disturbance measured.	Duration, in minutes.	Amount of vertical dis- turbance in units of scale (hundredths of an inch.)		Abruptness repre- sented by vertical disturbance at Kew in one minute.	Stonyhurst minus Kew distur- bance.
1868.			Kew.	Stonyhurst.		
Jan. 24.	(1) to (2)	14	52	54	3.7	+ 2
Feb. 5.	(1) to (2)	12	■	57	4.2	+ 6
"	(2) to (3)	7	18	24	2.6	+ 6
Mar. 6.	(1) to (2)	17	107	115	6.3	+ 8
20 (A).	(1) to (2)	long con- tinued.	30	31	slow and curved disturbance.	+ 1
20 (a).	(1) to (2)	4	30	40	7.5	+10
"	(3) to (4)	12	40	45	3.3	+ 5
21 (a).	(1) to (2)	11	71	73	6.4	+ 2
21 (A).	(1) to (2)	long con- tinued.	41	40	slow and curved disturbance.	- 1
21 (c).	(1) to (2)	8	30	35	3.5	+ 5
23.	(1) to (2)	7	61	72	8.7	+11
"	(3) to (4)	doubtful.			
"	(5) to (6)	3	32	57	10.7	+25
"	(6) to (7)	2.5	30	40	12.0	+10
"	(8) to (9)	10	70	■	7.0	+20
24.	(1) to (2)	12	40	44	3.3	+ 4
Apr. 1.	(1) to (2)	11	57	60	5.2	+ 3
"	(3) to (4)	10	63	70	6.3	+ 7
2	(1) to (2)	4.5	21	30	4.7	+ 9
"	(2) to (3)	4	11	21	2.8	+10
"	(4) to (5)	4.5	30	51	6.6	+21
"	(6) to (7)	4	45	66	11.2	+21
"	(8) to (9)	4.5	43	65	9.6	+22
"	(10) to (11)	■	39	63	7.8	+24
19.	(1) to (2)	5.5	35	50	6.4	+15
"	(3) to (4)	5.5	27	38	4.9	+11
"	(5) to (6)	10	74	87	7.4	+13
"	(6) to (7)	23	94	99	4.1	+ 5
27.	(1) to (2)	16	63	60	4.0	- 3
"	(2) to (3)	7	22	■	3.1	0
"	(4) to (5)	6	52	60	8.7	+ 8
May 11.	(1) to (2)	17	53	53	3.1	0
20.	(1) to (2)	7	20	24	2.9	+ 4
"	(3) to (4)	12	22	23	1.8	+ 1
20-21.	(1) to (2)	12	90	111	7.5	+21
"	(2) to (3)	14	40	65	2.9	+25
"	(3) to (4)	10	20	30	2.0	+10
"	(4) to (5)	long con- tinued.	65	65	slow and curved disturbance.	0

It may be inferred from this Table that where the disturbances are slow and long continued, that is to say, where there is scarcely any abruptness, the amount of disturbance as represented by the traces is the same for both places; and this is quite confirmed by placing the curves the one over the other, when they will be found to coincide even in their most minute features.

Let us now take the excesses of Stonyhurst over Kew for the various disturbances, and endeavour to see if this element is in any way connected with the abruptness of the disturbance.

We may for convenience sake divide these excesses into four groups.

Group I. Excesses not exceeding 4 scale-units.

II. Excesses exceeding 4 and not exceeding 9 scale-units.

III. Excesses exceeding 9 and not exceeding 19 scale-units.

IV. Excesses above 19 scale-units.

Group I.		Group II.		Group III.		Group IV.	
Excess (under 5).	Abruptness.	Excess (under 10).	Abruptness.	Excess (under 20).	Abruptness.	Excess (above 20).	Abruptness.
2	3.7	6	4.2	10	7.5	21	7.5
2	6.4	6	2.6	10	2.0	25	2.9
-3	4.0	8	6.3	11	8.7	25	10.7
0	3.1	5	3.3	10	12.0	20	7.0
0	3.1	8	8.7	10	2.8	21	6.6
4	2.9	5	3.5	15	6.4	21	11.2
1	1.8	7	6.3	11	4.9	22	9.6
4	3.3	9	4.7	13	7.4	24	7.8
3	5.2	5	4.1				
Means 1.5	3.7	6.6	4.9	11	6.5	22	7.9

It would appear from these groups that generally, and on an average, the excess of Stonyhurst over Kew in declination disturbances varies with the abruptness of the disturbance, being great when the disturbance is very abrupt.

It is hoped that on some future occasion further results, derived from an intercomparison of these curves, may be presented to the Society.

III. "On the reappearance of some periods of Declination Disturbance at Lisbon during two, three, or several days." By Senhor CAPELLO, of the Lisbon Observatory. Communicated by BALFOUR STEWART, LL.D. Received October 28, 1868.

Any one who carefully examines the magnetograph curves must often notice that there are, during periods of disturbance, synchronous movements of the needle during corresponding hours for two, three, or more days.

In some cases the repetition is only in two or three parallel movements, in others there are true periods of some hours in duration.

The repeated periods are not entirely similar, their phases being in general so modified that in some cases their identity can only be recognized by a very minute investigation.

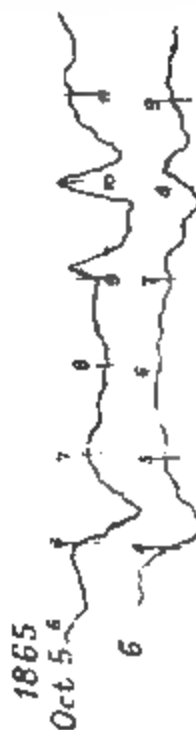
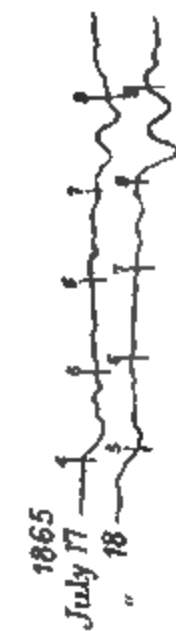
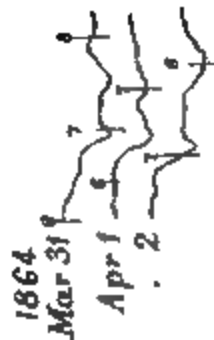
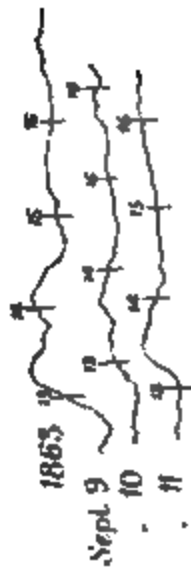
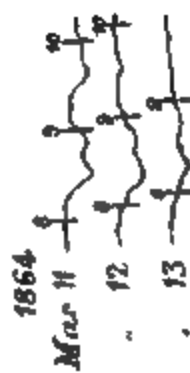
The same periods, when repeated, have not always the same total duration; nor do they recommence at the same precise hour, but sometimes earlier, and sometimes later, the differences varying from a few minutes to two or three hours.

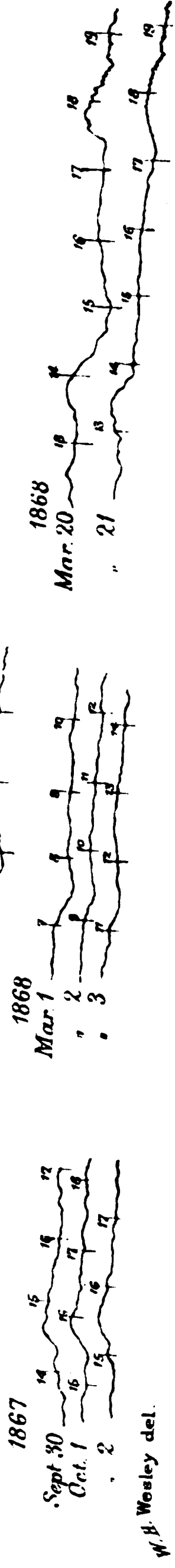
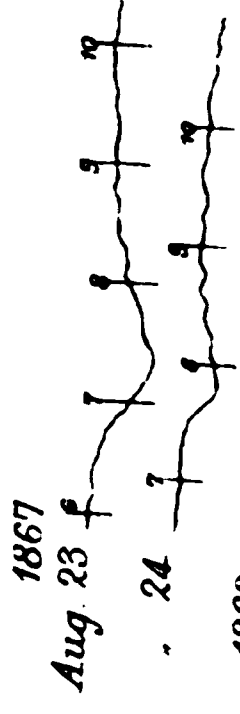
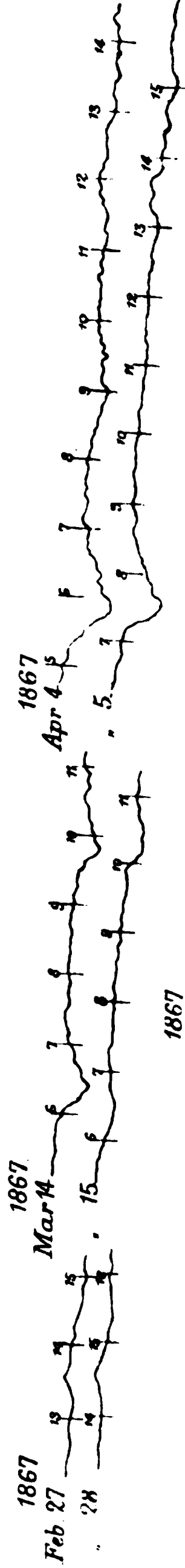
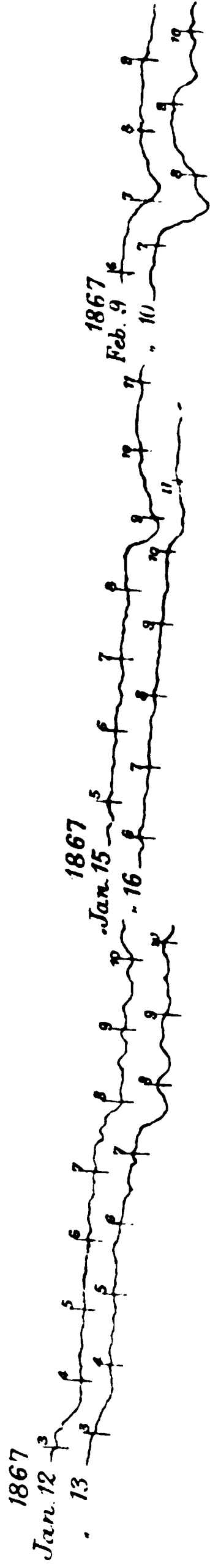
There is also to be remarked in the repeated disturbances a tendency to modify in form, or to level their *peaks* and *hollows*, or, on the contrary, to augment the angular forms.



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We also see that the greatest number of repetitions belong to the night hours, that is to say, those hours when the movements of the needle are easterly. In the morning hours there do not appear to be any well-marked repetitions.

I append examples chosen from the five years complete (July 1863 to June 1868) of the declination curves of the Observatory at Lisbon (see Plates V. & VI.).

A greater number of cases might be quoted, but those I have chosen are sufficient for our purpose.

Meanwhile I must mention that, in the majority of instances, where no relation apparently exists between one disturbance and the disturbances of the days preceding and following, the disturbances are generally violent.

There are twenty-four examples, fifteen of which show repetition on two days, eight on three days, and one only where the curve appears repeated for four days.

In order that the identity may be easily recognized, I have placed the curves with their corresponding periods vertical, the hours being marked on each curve.

It appears that all the facts exhibited in these examples agree with the cosmical theory; the cause (existing in the sun or in space) appears to continue sometimes during two, three, or several days without undergoing remarkable transformations.

The repetition, being sometimes earlier, sometimes later, seems also to indicate that the cause possesses a proper movement; the cause persists, but only comes again into operation when the earth by its diurnal rotation is placed in a similar position or conjunction to that of the preceding days.

It would be very curious to analyze the photographs of the sun so as to see if there were any spots in the days of the examples, and if these spots remained without sensible alteration during the days when the disturbances remained so similar.

Remarks by B. STEWART.

I have compared Senhor Capello's curves with the corresponding traces of the declination at Kew, and it would appear that the Lisbon disturbances are almost invariably reproduced at Kew at the same time, only to a greater extent. It would further appear that the same amount of similarity which the various Lisbon curves exhibit is also exhibited in the corresponding Kew curves.

Opinions may differ with regard to the strength of the evidence exhibited by Senhor Capello in support of the peculiar action of the disturbing forces of which he is an advocate. It would appear to me that the strongest point in favour of the hypothesis is not so much the repetition of a single disturbance as the repetition of a complicated disturbance in most if not all of its sinuosities. Several examples of this occur in these diagrams.

IV. "On the Action of Solid Nuclei in liberating Vapour from Boiling Liquids." By CHARLES TOMLINSON, F.R.S. Received November 5, 1868.

History.—During many years after the invention of the barometer and the consequent discovery of atmospheric pressure, the boiling-point of a liquid was defined to be the temperature at which its evaporating tendency is equal to the common pressure of the atmosphere, or the lowest temperature at which its vapour can have the elasticity of common air.

About the middle of the last century it was noticed by several distinguished Fellows of this Society that the boiling-point of water under a constant pressure varies within certain limits, according to the depth to which the thermometer is plunged into the boiling liquid.

In the Report on Thermometers published in the Transactions for 1777, it was stated that the steam from boiling water fairly represents the atmospheric pressure; and it was recommended that, in determining the boiling-point, the water be boiled in a metal vessel constructed so as to allow the bulb, and nearly the whole of that part of the stem that contained mercury, to be surrounded by the steam.

In the fine experiments undertaken by Dalton, Watt, Robison, Southern, and others for determining the pressures of saturated steam at different temperatures above and below the standard boiling-point, it was noticed that if a minute portion of soda or of some salt soluble in water, and not capable of rising in vapour with it, be allowed to ascend to the top of the mercury, the column rises, thereby indicating a diminished pressure of steam. The adhesion of the soda to the water tends to restrain the water from evaporating, and thus the steam-emitting tension of a solution of soda is measurably less than that of pure water at the same temperature.

In 1785 Achard* showed by a number of experiments that the boiling-point of water, under a constant pressure, is much more inconstant in metallic than in glass vessels. He also noticed that if, while water was boiling steadily in a glass vessel, a drachm of iron-filings or of some other insoluble solid were added to the water, the boiling-point was lowered 1° Reaumur or even more, and that there were considerable differences in the amount of depression, according as the same substance was in powder or in lump.

In 1803 De Luc† stated, in very precise terms, that boiling is produced by the bubbles of air which the heat disengages from the liquid. If the water be completely purged of air it cannot boil, because steam can only form on free surfaces, such as the air-bubbles present. Deprived of air, water can boil only on the upper or free surface. Water in a tube from which the air had been carefully expelled was raised to 234½° F. without boiling.

* Nouveaux Mémoires de l'Académie Royale de Berlin for 1784–85.

† Introduction à la Physique terrestre par les Fluides expansibles. Paris, 1803.

In 1812, and again in 1817, Gay-Lussac* noticed Achard's experiments on the effect of the vessel on the boiling-point, and of metal turnings, charcoal powder, and pounded glass in lowering it. He supposed the boiling-point to vary in different vessels according to the nature of their surfaces, and that the variation depends both on the conducting-power of the material for heat and on the polish of the surfaces. When water is boiling in a glass vessel, the temperature is higher than in a metal one; but if a few pinches of iron-filings be put in, the boiling goes on as in a metal vessel. Without this aid the water boils in bursts, the steam having to overcome the cohesion or viscosity of the liquid, and its resistance to change of state. The adhesion of the liquid to the vessel must also be a force analogous to its viscosity. The use of platinum is recommended for preventing *soubresauts*.

In 1825 Bostock† noticed that ether in a matras over a spirit-lamp boiled at 112° F.; but in a test-tube put into hot water it did not begin to boil under 150°, and on one occasion 175°. Bits of cedar-wood put into the ether made it boil at 110°; the wood was covered with bubbles until (according to Bostock) having discharged all its air, it became inactive and sank. Bits of quill, feather, wire, pounded glass, &c. also lowered the boiling-point considerably. A thermometer plunged into the ether produced bubbles many degrees below the point at which ebullition took place when the thermometer was not inserted: this effect soon ceased; but by alternately plunging the thermometer into the ether and removing it, the bubbles were produced at each immersion.

Legrand‡ in 1835 also referred bursting ebullition and *soubresauts* to the absence of air in the liquid. Many salts prevent *soubresauts*; others, such as the neutral tartrate of potash, favour them.

In 1842 Marcet§ considered that iron, zinc, and other substances tend to depress the boiling-point, because they have a less molecular adhesion for water than glass has. If the vessel be coated with a thin layer of sulphur, gum-lac, or any similar substance that has no sensible adhesion for water, the temperatures of the water and of the steam are alike. The boiling-point varies in flasks of different kinds of glass, and in the same flask at different times. In a flask used for holding sulphuric acid the boiling-point of pure water was 106° C. These variations are referred to molecular changes on the surface of the glass.

In 1843 Donny|| referred to the powerful influence of air or gases dissolved in the liquid on the phenomena of ebullition; but as his theory is the same as that advanced by De Luc many years before, it is not necessary to notice it further.

In 1861 Dufour¶ described an experiment in which globules of water

* *Annales de Chimie*, vol. lxxxii. p. 171; *Annales de Chimie et de Physique*, vol. vii. p. 307.

† *Annals of Philosophy*, N. S. vol. ix. p. 106.

‡ *Annales de Chimie et de Physique*, vol. lix.

§ *Annales de Chimie et de Physique*, 3^e Série, vol. v. p. 449.

|| *Mémoires couronnés par l'Académie Royale de Bruxelles*, vol. xvii. pub. 1845.

¶ *Archives de la Bibliothèque Universelle de Genève*.

suspended in hot oil are said to have been raised to from 110° to 178° C. without boiling; but the moment they were touched with a solid they burst into steam. Porous bodies acted best because, it is said, they carried down air to the globules.

Such is a very brief notice of a few of the numerous memoirs that have been published on the subject of boiling. The writers are all more or less disposed to adopt the following conclusions:—(1) that liquids boil with difficulty, or produce only sudden flashes of steam, as soon as the air which had been dissolved in them is expelled by heat; (2) that those liquids that have the weakest affinity for air, such as sulphuric acid, alcohol, ether, &c., boil with the greatest difficulty; (3) that the adhesion of the liquid to the vessel, and the mutual cohesion of its own molecules, cause the liquid to boil in bursts, and produce *soubresauts*; (4) that the action of solid substances in preventing *soubresauts* is by carrying down air.

My object in introducing a new set of experiments on boiling is (1) to show the action of solid nuclei in liberating vapour from liquids at or near the boiling-point; (2) to define the conditions under which *soubresauts* take place; and (3) to show what is the best remedy for the same.

Definition.—A liquid at or near the boiling-point is a supersaturated solution of its own vapour, constituted exactly like soda-water, Seltzer-water, champagne, and solutions of some soluble gases.

Action of Nuclei.—If the above definition be admitted, the behaviour of solid substances in liberating vapour on some occasions, and remaining “inactive” on others, becomes clear. If the solid be chemically clean, the solution of vapour will adhere to it as a whole, and there will be no liberation of vapour. If, on the contrary, the solid be unclean, the adhesion between the vapour and the solid will remain the same as before, while the adhesion between the liquid and the solid will be more or less diminished, according to the nature of the impurity and the liquid operated on; and hence there will be a separation of vapour.

But not only is it necessary to distinguish bodies as chemically clean or unclean, but also as porous or compact. The same force by which one volume of charcoal absorbs 98 volumes of ammoniacal gas, enables charcoal and some other porous bodies, when thrown into a boiling liquid, to separate the vapour from it, and thus to act as most efficient nuclei.

The liquids operated on were water, alcohol, ether, wood-spirit, naphtha, carbonic disulphide, benzole, paraffine oil, oil of turpentine, rosemary, and a few other essential oils.

The liquids with low boiling-points are convenient for illustrating the phenomena in question. Any one of them in a tube about one-third or one-half filled may be raised to the boiling-point by putting the tube into hot water contained in a six- or eight-ounce German flask standing on the ring of a retort-stand. The tube should fit loosely into the neck, and rest on the bottom of the flask. In reheating the water a small spirit-lamp flame may be applied, not directly under the tube, but on one side of it, the

object being to keep the liquid in the tube at or near the boiling-point, but not actually boiling. The temperature of the liquid in the tube may be taken from time to time, and the thermometer, when not in use, may be kept in a tall narrow glass containing a little of the liquid under examination.

Carbonic disulphide, from its low boiling-point, gives off vapour with facility, and is consequently well adapted to show the action of solid nuclei. When the tube was placed in the hot water, a quantity of dense white vapour ascended from the surface of the liquid to the top of the tube; but instead of overflowing it condensed in copious tears, which fell back into the liquid and caused a strong descending current. On touching the surface of the liquid with the end of a brass wire, violent ebullition set in, the bubbles rising to near the mouth of the tube. The boiling ceased altogether as soon as the wire was removed; but when the surface was touched with a strip of paper, it set in as violently as before.

Now in these two experiments no air could have been carried down, since the surface only was touched, and the boiling continued only while the solid was kept in contact with such surface. Iron wire also liberated vapour abundantly. The end of a glass rod was active at two small points, liberating from each a rapid stream of bubbles, the remaining portions being clean, or having soon become so by the action of the hot liquid, since glass is readily cleansed by liquids near the boiling-point.

But it is said that rough bodies are most favourable to the liberation of vapour. The hot carbonic disulphide was touched with a rat's-tail file, and it produced furious boiling. The file was then held in the flame of a spirit-lamp, and while hot placed in the upper part of the tube, so that it might cool down to about the temperature of the liquid, and yet be sheltered from the air. On touching the surface of the disulphide with the end of the file, there was no liberation of vapour; and the file was slowly passed to the bottom of the liquid, but still there was no action. The file was now taken out and waved in the air; on reinserting it into the liquid, there was a burst of vapour arising from some mote or speck of dust caught by the file from the air. The file was quickly cleaned by the liquid, and it became inactive as before. It was again taken out and waved in the air, and on once more putting it into the liquid boiling set in again.

A tube containing ether was put into the hot-water bath; it quickly reached the boiling-point, and two specks in the tube became active in discharging rapid streams of bubbles. Specks of this kind are often powerful as nuclei in separating gas from soda-water &c., and in causing the sudden crystallization of supersaturated saline solutions. Such specks in the bottoms of flasks, beakers, and retorts are powerful nuclei in separating vapour from a liquid during the boiling. The vapour seems to be generated by these points, and to proceed from them to the surface in rapidly enlarging bubbles. These specks consist of iron, carbon, or some other material which is not so readily cleaned as the glass, or they present a porous point to the vapour.

As the temperature of the water-bath fell, the ether in the tube ceased to give off bubbles of vapour. A small pellet of writing-paper was now thrown in: the liquid boiled up furiously, the paper being much agitated, when suddenly it sank as if dead, and all vapour-giving action ceased. It had become, in fact, chemically clean. The paper was removed and a brass wire passed to the bottom of the tube, when the whole liquid boiled up briskly during a few seconds; when, the wire becoming chemically clean, all action ceased, except from a point near the bottom of the wire, which continued to pour off a fine stream of bubbles during some minutes. The wire was now taken out and filed, in order to get rid of this nucleus. On returning it to the tube the ether boiled up as before, the handling and filing having made the whole immersed surface unclean; but the ether soon cleaned it, and it became inactive; but the active point was not only not got rid of, but there were now two points rapidly discharging vapour. These points are probably portions of porous dross entangled with the metal. During these experiments the ether was maintained at about 96° , and it boiled only when a solid nucleus was introduced.

Methylated spirit was raised to about 178° . A piece of flint that had long been exposed to the air was put into the tube; it gave off vapour from its surface in abundance. The flint was taken out and broken, and the two fragments were returned to the spirit. The newly fractured surfaces, being chemically clean, were quite inactive, not a single bubble of vapour appearing on them, while the outer surface continued to give off vapour as before. A strip of slate gave off vapour from a number of points in both surfaces; it was split into two strips and replaced in the hot liquid; the old surfaces were active as before, but the fresh surfaces were perfectly inactive. Mica and selenite do not answer well for these experiments. In the specimens tried, air containing dust had been dragged in in patches between the plates; these, when newly split and put into soda water, showed considerable portions that were chemically clean in the midst of unclean patches.

The action of nuclei can be well exhibited in oils with high boiling-points, such as the essential oil of turpentine, rosemary, &c. When the oil is boiling in a tube over a spirit-lamp, a strip of slate with one new surface may be introduced before the lamp is removed, so as to prevent the oil from being chilled. If, now, the lamp be taken away, vapour will pour off from the unclean surface of the slate during some minutes, while the freshly fractured surface will be quite inactive.

The behaviour of nuclei, as thus far described, is the same as for supersaturated saline and gaseous solutions. A chemically clean nucleus will not separate either the salt or the gas from solution; a chemically unclean nucleus will do so immediately it comes in contact with the solution.

If the definition I have given of a liquid at or near the boiling-point be accepted, and it be admitted that solid nuclei behave in the same manner under the same conditions in separating salt, or gas, or vapour from solution, what is the action in this respect of air and gases?

It has been maintained that air is a powerful nucleus in separating salt from a supersaturated solution, that it is the air alone, as carried down by the solid, that acts as a nucleus in separating gases from solution, and that if air be absent from a liquid it cannot boil, because there is nothing for the vapour to expand upon.

I have shown in former experiments that, in the case of supersaturated saline solutions, air is not a nucleus; but that when it appears to be so, it is merely acting the part of a carrier of some chemically unclean mote or speck of dust. I have also shown that masses of air may be introduced into soda-water without any separation of the gas, provided the conditions of chemical purity be observed. A wire-gauze cage, for example, full of air can be lowered into soda water without producing any discharge of gas into the cage, or any separation of gas from the surface of the cage, so long as it is chemically clean; when unclean, there is an abundant separation of gas from the surface of the cage, but the enclosed air remains purely passive all the time.

A similar result may be obtained in the case of a liquid at or near the boiling-point, if precautions be taken to raise the cage to the temperature of the liquid before introducing it.

The cage used in these experiments was smaller than that used in the soda-water experiments. It was five-eighths of an inch in diameter, and an inch and a half in length, and made of fine iron-wire gauze, such as is used by millers in bolting meal. Two of these cages were prepared. One was cleaned by being put into boiling spirits of wine; it was rinsed in clean water, and so held in the steam of pure water boiling in a test-tube, so that the cage and the enclosed air might be adjusted to the temperature of the water. The cage was gently lowered into the water the moment the spirit-lamp was withdrawn. There was no escape of vapour; there was no violent boiling up, which must have ensued had air been a nucleus. But here was a mass of air in the midst of the liquid, and yet the steam did not expand into it. The openings into the cage must have been very much larger than the diameter of the globules of air which are supposed always to be present when a liquid is boiling, and yet there was no separation of vapour. This clean cage was removed, and the other cage, just as it had left the hands of the maker, was held in the steam of the water of the same tube, and the moment the lamp was removed gently lowered into the water. It was instantly and completely covered with bubbles of steam; but there was no expansion of steam into the cage, and no escape upwards either of steam or of air.

A good result was obtained with paraffine oil boiling at 320° . While the cage was being lowered, it became filled about one-half with the liquid, but when completely submerged there was no action whatever. But, perhaps, it may be said that the liquid was now so far below the boiling-point as to be incapable of giving off vapour to any nucleus, clean or unclean. To test this, a small pellet of paper was thrown in; the liquid immediately

began to seethe audibly, and it continued to give off vapour during more than two minutes, the paper pellet resting during the latter part of the time on the top of the cage. Similar good results were also obtained with oil of turpentine. The cage was also lowered into naphtha, and some of the other low boiling liquids, and whenever there was an escape of vapour, it could always be referred to some unclean portion of the cage. Care is required in lowering the cage, so as to expand the air; for unless this be properly done, there may be a violent burst of air when the cage is near the bottom of the tube.

It really does seem to me that too much importance has been attached to the presence of air and gases in water and other liquids as a necessary condition of their boiling. Cold water dissolves only one-fiftieth of its volume of nitrogen, and one twenty-fifth of its volume of oxygen, and these small quantities must be reduced to an almost inappreciable amount in hot or boiling water; and yet some observers represent boiling water purged of air as reabsorbing it eagerly while still boiling. The only function I should assign to air would be that of diminishing somewhat the cohesive force of the liquid molecules. If the tube be of narrow bore and chemically clean, or becomes so by the action of the liquid, adhesion has some influence in raising the boiling-point. But the mode of heating the liquid is of still greater importance in this respect, as is evident in De Luc's experiments, and was well brought out in Bostock's. In the latter case ether in a matrass over a spirit-lamp, boiled at 112° ; but in a test-tube in hot water at 150° and even 175° F. The difference in the conditions of heating has doubtless been regarded as too evident to be insisted on; and yet it is of great importance in studying the conditions under which the boiling-point of a liquid becomes raised. When the vessel is placed over a flame, that part in contact with the flame is heated, or tends to become heated, much more strongly than the rest of the vessel. This produces active convective currents, the effect of which is to loosen the cohesion of the particles, and so allow vapour to form more easily. When the water once begins to assume the elastic form, it does so from the overheated part of the vessel in contact with the flame. In a clean glass vessel containing distilled water placed over a spirit-lamp, no air-bubbles form, either on the sides or on the clean thermometer. They appear on the bottom surface only, playing about and disappearing upwards until the water is at about 160° . At about 180° small steam-bubbles are given off from the bottom heated surface with a crackling noise; they rise rapidly, expand, and disappear before reaching the surface; and until they succeed in doing so, the convective currents are active. When the bubbles reach the surface and discharge steam into the air, the whole column is broken up, cohesion is overcome, and the boiling is maintained, while the liquid gradually disappears. Such is the process of boiling in a vessel heated by a flame from below. When, however, all that part of the vessel (such as a test-tube) that contains liquid is put into a hot bath, the whole column is equably heated, or

rather the top of the column is a little more heated than the bottom (since the upper layers of hot water are at a higher temperature than the lower ones), and the effect of this is that there are no convective currents; cohesion is diminished by expansion, not by convection. The whole column being thus about equally heated at the same moment, vapour cannot form at one part in preference to another, except at the surface; but the whole column of liquid goes on expanding under an increasing temperature until, becoming more and more supersaturated with its own vapour, the increasing elastic force suddenly overcoming pressure, cohesion, and adhesion, there is a sudden burst of vapour. Or before this disruption takes place, if the surface be touched with a chemically unclean solid, the vapour adhering to it and thus set free, starts the vapour-giving action, just as touching a cold supersaturated saline solution starts crystallization, and the action once begun is propagated.

If, however, the tube containing the ether &c. be not chemically clean, if there be minute specks and points in the glass (as there often are) all but invisible to the naked eye, and these be porous or not chemically clean, vapour will stream from them long before the temperature of disruption is attained, and there will be no disruption at all. These points and specks account for many anomalous cases of crystallization which occur in operating with supersaturated saline solutions, and which puzzled Löwel and other observers. We may have, for example, two tubes apparently precisely alike, cleaned in the same manner, containing a hot filtered solution of the same salt, of the same strength, and exposed to the same cooling influence. One of the solutions in cooling will suddenly become solid, while the other will remain liquid, and continue so during weeks and months. On examining the solidified solution, it will be found that crystallization has been promoted by a minute speck or point at some part of the tube, no matter where, and from this point, as from a centre, proceed fine crystalline needles radiating in all directions.

Soubresauts.—Liquids which render the surface of the vessel in which they are boiled or distilled chemically clean, thereby favour the production of *soubresauts*, or jumping ebullition. This is a mechanical action which does not seem to have been sufficiently explained. Thus Gay-Lussac says, “When the liquid is above the boiling-point, it is in a forced state: instantly a burst of vapour is formed, the liquor is thrown out, and the vessel itself raised.”

But why should the vessel be raised? The burst of vapour follows an upward motion along the line of least resistance, which, so far from raising the vessel, has a precisely contrary effect. It produces an equal reaction in a downward direction, tending to force the vessel further into the ring of the retort-stand, or other support, and it is the rebound from this that causes the vessel to rise. If proof be required of the truth of this explanation, it can easily be supplied by suspending, by means of an india-rubber line or a bit of elastic, a tube containing crystals of sodic sulphate and a

very little water. If the flame of a spirit-lamp be applied to the bottom of the tube, the crystals soon fuse and throw down a portion of the anhydrous salt, which is highly favourable to the production of soubresauts. If the tube be suspended against an upright surface, with a mark opposite the mouth, it will be easily seen that every burst of steam is accompanied by a violent downward jerk.

In order to mitigate or prevent this bumping ebullition, it has long been the practice to introduce into the retort or other vessel, a few angular pieces of solid matter, metallic being the best—such as platinum-foil, silver, copper, or platinum-wire or filings, fragments of cork or torn cartridge-paper. Faraday names these substances “promoters of vaporization,” without explaining their action; and he remarks that if any one of these substances be suddenly introduced, “it is probable that the consequent burst of vapour would be so instantaneous and strong as to do more harm than the bumping itself” *. This is precisely the action of an unclean solid introduced into a supersaturated gaseous solution, or in the case of a liquid at or near the boiling-point, into a supersaturated vaporous solution.

When sand, fragments of glass, or other non-metallic substances are used for preventing bumping, they facilitate the escape of vapour only so long as they are unclean; but as siliceous bodies are readily cleansed by the action of boiling water and other boiling liquids, they often aggravate the evil. For example, two ounces of distilled water containing a little sand from the sand-bath, were boiled in a six-ounce German flask over a spirit-lamp. The boiling proceeded briskly without any kicking. The lamp was removed and the flask left to cool. Next morning the lamp was again put under the flask, when the water boiled with such violent kickings as to endanger the safety of the vessel. The sand had become chemically clean during the first boiling. If sand, cleaned by means of sulphuric acid and much rinsing, be added to water in the first instance, the kickings set in at once. Similar results were obtained with fragments of glass; when chemically clean, they serve to enlarge the adhesion surfaces, instead of the vapour-giving surfaces, and so increase the resistance to be overcome.

With respect to the action of metals, there is no advantage in making them sharp-pointed, nor in having their surfaces rough; only, in the latter case, unclean vapour-giving substances are apt to lodge in the rough lines, or between the teeth, and so far a file or other rough body may be of advantage. Metal filings are also liable to collect dust and specks of dirt, which act as nuclei. The following experiment shows the action of clean, as compared with unclean iron-filings. A flask cleaned by means of sulphuric acid contained four ounces of distilled water, which boiled at 215° . Some iron-filings that had long been kept in spirits of wine were thrown in. There was a good deal of kicking, and the temperature oscillated between $213\frac{2}{3}^{\circ}$ and $213\frac{5}{6}^{\circ}$. Some unclean filings were thrown in, and the effect

* Chemical Manipulation, p. 200.

was instantaneous. Copious streams of bubbles proceeded from the filings, the *soubresauts* ceased, and the temperature fell to $211\frac{1}{2}^{\circ}$. Similar results were obtained with copper-filings, and copper and brass wire, clean and unclean, and also with platinum foil and wire.

An experiment with mercury may perhaps be of interest. The metal was cleaned by being repeatedly shaken up with dilute nitric acid; and after standing some time under it, a portion was drawn off from the bottom. Five ounces of water in a clean flask boiled at $213\frac{1}{2}^{\circ}$. Enough mercury was poured in to form a ring at the bottom of the flask. The water soon regained its temperature, and even rose to 214° , with a good deal of bumping—steam forming under the mercury and distending it into a large hemisphere, which burst with a kick. The temperature varied between $213\frac{8}{10}^{\circ}$ and 214° . It would have been dangerous to have entirely covered the bottom with the metal; for, as it was, the bursts of vapour were of an explosive character. While this uneasy boiling was going on, a very little dirty mercury was added to the flask, and, although the quantity was not more than one-sixth of that previously added, the effect was remarkable. Instead of the uneasy, kicking, jerking bursts, the whole instantly changed into a brisk, easy, soft boiling, rapid volleys of steam-balls being given off by the metal, breaking up the mass of water, while the temperature remained steady at $212\frac{2}{10}^{\circ}$.

It will thus be seen that the vitreous and metallic bodies employed in these experiments, as also the bits of paper, shavings of cedar-wood, &c., are efficient as nuclei only so long as they are chemically unclean. When clean they become inactive as “promoters of vaporization.”

Action of Porous Bodies.—But there are certain bodies, such as charcoal, coke, &c., that I have not been able to make inactive, either by the action of strong sulphuric or nitric acid, or by repeated boiling in water, ether, spirits of wine, naphtha, &c. The same piece of charcoal held in the flame of a spirit-lamp and then put into soda-water, or into a liquid at or near the boiling-point, will liberate gas or vapour without any apparent diminution of its powers. It may be transferred from one liquid to another, from ether to alcohol, from alcohol to water, and from water to oil of turpentine without ceasing to perform useful work in setting vapour free, making the boiling soft and easy, and preventing *soubresauts**. The same remark applies to coke. It may be cleaned in the strongest acids, washed in water and alkalies without losing any of its vigour as a liberator of vapour from a hot liquid. It is quite remarkable to see how efficiently a lump of coke acts in a vessel of boiling water in giving off vapour, promoting tranquil boiling, and preventing the jumping of the vessel. Platinum sponge is also active. A small piece of this substance at the bottom of a flask of boiling water will send up vigorous jets of steam-bubbles, raising the water

* Box-wood charcoal, which Saussure found so efficacious in his experiments on the absorption of gases, is very active in boiling liquids; but specimens of charcoal and of charcoal-bark from the softer wood are also of untiring activity.

far above the surface. As in the case of charcoal and coke, the liberation of vapour is confined to the solid nucleus, no part of the flask giving off visible vapour. The following data show the influence of the solid upon the temperature. Five ounces of distilled water in a clean flask boiled at $213\frac{1}{2}^{\circ}$. A small lump of platinum sponge was held in the flame of a spirit-lamp and then put into the flask. The temperature subsided to $212\frac{4}{10}^{\circ}$, and remained so for some time. A second small piece of sponge was similarly heated and put into the flask; it was as active as the former piece in liberating vapour, but there was no further depression of temperature. The water was now allowed to get cold; and on again applying the spirit-lamp there was a good deal of loud explosive bumping, until the water was near 200° , when the platinum sponge began to give off steam and the boiling became soft and regular.

I have not the command of apparatus for determining the volume of vapour absorbed by platinum sponge, charcoal, &c. at given temperatures; but it would not be difficult to do so by one or other of the contrivances made by Dalton and Gay-Lussac in determining the elasticity of the vapours of liquids at the boiling-point. It would also be interesting to study the action of nuclei on liquids heated above the pressure of one atmosphere.

Meerschaum is also an active nucleus. A bit of this substance was thrown into a tube filled about one-third with newly distilled oil of turpentine which boiled at about 310° . The whole tube became filled with bubbles; and long after the lamp was removed the nucleus continued to liberate numerous streams of bubbles, an effect that is common to all porous bodies tried in these experiments, but more remarkable in some cases than others*.

A fine-grained pumice-stone cleaned in nitric acid, and another piece not cleaned, were both very active in giving off vapour from liquids. As in the case of charcoal and meerschaum, they soon sank to the bottom of the vessel, unless buoyed up by the steam while the lamp was burning under the flask, and continued to pour off vapour so long as the liquid was at or near the boiling-point. When the water was below 100° , the flask was put under the receiver of an air-pump and the air exhausted; the water soon boiled, and the pumice was as active as before in liberating vapour.

Chalk, plumbago, and platinum balls are all active promoters of vaporization.

In the absorption of gases by charcoal, Saussure found that, if a piece of charcoal impregnated with one gas were introduced into another gas, a por-

* Illustrations were frequently afforded in these experiments of the different action of a clean as compared with an unclean surface. In the experiment in hand, in order to take the temperature of the boiling turpentine, the thermometer-bulb and part of the stem were made chemically clean; but having on one occasion to leave the thermometer in the tube, its bulb was made to rest on the bottom, so that about an inch and a half of the stem that had not been made chemically clean became immersed. This portion was instantly covered with minute beads of vapour, so as to give it a frosted appearance exactly distinguishing the clean from the unclean portion.

tion of the absorbed gas might either be driven out or further condensed. A somewhat similar action may be noticed by transferring a piece of charcoal from one boiling liquid to another. For example, a small piece of well-burnt charcoal from the centre of a lump was held in the flame of a spirit-lamp until it was red-hot, and so put into boiling water. It was not very active at first, but it soon became so, and continued so as long as the heat was kept up. After about half an hour's action the charcoal was taken out, dried in a cloth, and put into boiling turpentine; here it was amazingly active, and continued so during some minutes after the lamp had been removed. The charcoal was dried on filtering-paper and put into spirits of wine; it was now much less active than fresh charcoal would have been; and in ether its activity was still more diminished. The charcoal was next put into hot water, and it at once started into activity; it was far more vigorous than clean charcoal is in water under any of the circumstances that have come under my notice. The charcoal was doubly active, not only from its porosity, but also from its want of chemical purity. On this latter account charcoal that has been used in boiling turpentine is singularly active in boiling water. And this sufficiently accounts for the fact noticed by Dufour, that when globules of water in hot oil came into contact with the thermometer or the sides of the vessel, they at once exploded into steam; but I believe the globules of water were in the spheroidal state in all the cases of very high temperature cited by him.

The diminished activity of charcoal and other porous bodies depends on the order in which they are introduced into liquids of different boiling-points. If transferred from a liquid with a high into one with a low boiling-point, the charcoal is more or less inactive, its absorptive powers being already satisfied; but if transferred from a liquid with a low into one with a high boiling-point its activity is increased, not only by the expulsion of the liquid absorbed, but also by the want of chemical purity that accompanies the process. Thus meerschau or coke that is very active in turpentine becomes inactive when transferred to spirits of wine; but after a time a single point in the solid may become active, and produce a rapidly rising inverted cone of vapour that has a very striking effect.

Conclusions.—The conclusions to which the foregoing details seem to lead are :—

(1) That a liquid at or near the boiling-point is a supersaturated solution of its own vapour.

(2) That a solid non-porous nucleus either is or is not efficient in liberating vapour, according as it is chemically unclean or clean.

(3) That as porous bodies do not become inactive, the proper nucleus for liberating vapour in the operations of boiling and distilling liquids, and for preventing *soubresauts*, is charcoal, coke, or some other porous body.

P.S. (Jan. 21, 1869).—As it seemed probable that some numerical results as to the action of porous nuclei in increasing the amount of the

distillate might be expected, I asked my friend Mr. Hatcher, late of King's College, to perform some experiments for me. The following are selected from his results.

1. A glass flask with a wide neck was filled about one-third with distilled water ; it was boiled over a gas-burner, rapidly weighed, and replaced over the burner. After boiling twenty minutes, it was weighed again. The flask was once more filled to the original quantity, and some bits of coke were added ; it was boiled and weighed as before, the gas-flame remaining unaltered all the time.

Results.—Water boiled away in the first trial (water only) 995 grains, in the second trial (with coke) 1130 grains.

Ratio of products, as 100 : 113·6.

2. Water was made to distil freely from a still, and the quantity collected in fifteen minutes was weighed. A few pieces of coke were then added to the water in the still, and the distillate collected again during fifteen minutes.

Results.—Distillate from water only, 293 grains ; from water with coke, 310 grains.

Ratio of products, as 100 : 105·8.

3. A similar trial was made with common wood-charcoal ; but the vessel having been made much cleaner by the action of the first boiling, the water boiled irregularly, with bumping. The addition of the charcoal made the boiling tranquil and regular.

Results.—Distillate from water only, 262 grains ; from water with charcoal, 334 grains.

Ratio of results, as 100 : 127·4.

January 28, 1869.

Lieut.-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, Sir Henry Holland proposed, and Lord Justice Bovill seconded The Most Noble the Marquis of Salisbury for election and immediate ballot.

The ballot having been taken, the Marquis of Salisbury was declared duly elected.

Pursuant to notice given at the last Meeting, the question of the readmission of Sir John Macneill and Mr. Edward Solly was put to the vote, and the ballot having been taken, those two gentlemen were declared readmitted into the Society.

The following communications were read :—

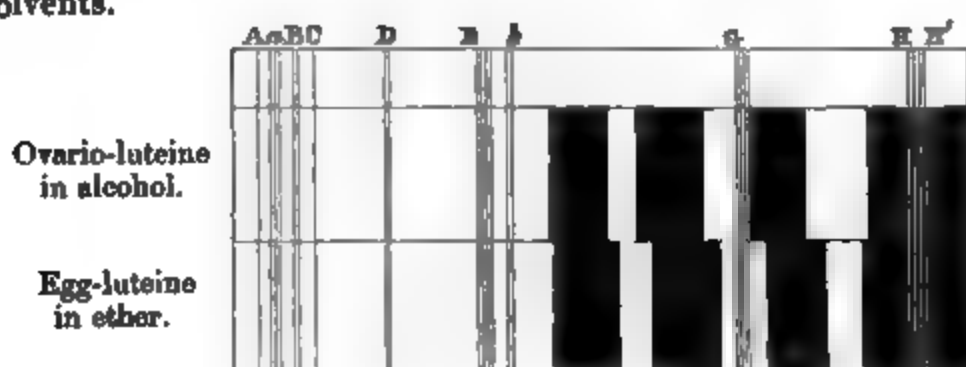
I. "Researches conducted for the Medical Department of the Privy Council, at the Pathological Laboratory of St. Thomas's Hospital." By J. I. W. THUDICHUM, M.D. Communicated by JOHN SIMON, Esq. Third Series.—Results of Researches on *Luteine* and the Spectra of Yellow Organic Substances contained in Animals and Plants. Received November 11, 1868.

1. *Name*.—Various parts of animals and plants contain a yellow crystallizable substance which has hitherto not been defined, and to which, from its prominent property, I assign the name of "*luteine*."

2. *Occurrence*.—It occurs normally in the corpora lutea of the ovaries of mammals, in the serum of the blood, the cells of the adipose tissue, and the yellow fat of the secretion of the mammary gland, or butter; in mammals it occurs abnormally in ovarian tumours and cysts, and in serous effusions. It is a regular ingredient of the yolks of the eggs of oviparous animals. In the vegetable world it is observed in seeds, such as maize; in the husks and pulps of fruits, such as anatto; in roots, such as carrots; in leaves, such as those of the coleus; and in the stamina and petals of a great variety of flowers.

3. *Properties*.—*Luteine* is easily soluble in alcohol, ether, and chloroform, but is insoluble in water. It is soluble in albuminous liquids, such as the contents of ovarian cysts and the serum of the blood. All these solutions are yellow; but the chloroform solution when concentrated has an orange-red colour.

4. *Spectrum*.—The spectrum of these solutions is distinguished by great brilliancy of the red, yellow, and green part, and by three absorption-bands, which are situated in the blue, indigo, and violet part of the spectrum. The positions of the absorption-bands vary a little with the different solvents.



5. *Crystallization*.—The crystals of *luteine* are apparently rhombic



plates, as shown in the accompanying figure, of which two or more are

always superposed in a curious manner. Possibly these crystals may be rhombohedra imperfectly developed on four of their edges. They are microscopic, yellow when thin, orange to red when thick, and have no resemblance to any other known animal or vegetable substance.

6. *Reactions*.—Luteine combines with few substances, mercury-acetate being perhaps the only ordinary reagent by which it is immediately and completely precipitated, as a yellow deposit. Mercury-nitrate produces a yellow precipitate, which on standing becomes white. Nitric acid poured over the crystals produces a blue colour, which immediately passes into yellow. The blue is not produced when nitric acid is added to either alcohol, chloroform, or ether solution, but appears with the solution in acetic acid and disappears again rapidly.

7. *Affinity for Fats*.—In the corpora lutea luteine is deposited in granules, which become the darker and larger the older the corpora grow. In the yolks of eggs it also exists in granules; and when extracted from any of these bodies it is always mixed with a considerable amount of an oily fat which contains cerebrine, and neutral fats, amongst them a peculiar fat containing phosphorus, like cerebrine. In butter after clarification it is found dissolved.

8. *Affinity for Albumen*.—On the other hand, luteine has great attraction to albumen, and can only with difficulty be extracted from serum or the fluid of ovarian cysts.

9. *Luteine in Vegetables*.—In vegetable matters luteine is contained in such a form that a clear watery solution cannot easily be obtained. All vegetable matters, however, readily yield their luteine to alcohol, and form by proper treatment clear solutions. In maize, luteine is accompanied by fats which are somewhat similar to those of eggs.

10. *Type of new Spectra*.—The spectrum of luteine is the type of the spectra of a series of bodies which are probably chemically identical; but not all yellow vegetable, animal, or chemical products are identical with luteine.

11. *New Spectra like that of Luteine*.—The yellow-coloured matters of the following plants present the spectrum of luteine, or one closely resembling it:—(1) Crocus or saffron (stamina); (2) *Helianthus annuus* (flower); the petals of the following plants—(3) *Leontodon taraxacum*, (4) *Leontodon* (*varietas* ?), (5) *Gazania elegans*, (6) Marigold common, (7) *Hypericum oblongifolium*, (8) *Acacia leprosa*, (9) *Galphimia splendens*, (10) *Stigmatophyllum ciliatum*, (11) *Lankesteria elegans*, (12) *Allamanda neriifolia*, (13) *Colutea frutescens*, (14) *Tagetes lucida*, (15) *Schkuhria atrovirens*, (16) *Diplotaxis tenuifolia*, (17) *Virgilia sylvatica*, (18) *Oenothera grandiflora*, (19) *Verbascum phlomoides*, (20) *Tagetes pumila*, (21) *Helianthus macrophyllus*, (22) *Chrysopsis villosa*, (23) *Helenium autumnale*, (24) *Obeliscaria pinnata*, (25) *Heliopsis laevis*, (26) *Linosyris vulgaris*, (27) *Berberis Darwinii*, (28) *Solidago serotina*, (29) *Ruta graveolens*, (30) *Melilotus elegans*, (31) *Medicago*

elegans, (32) *Allamanda Hendersonii*; (33) the root of the common carrot, *Daucus carota*; (34) the seeds of Indian corn, *Zea mays*. The extracts of the berries of the following plants also give the luteine spectrum:—(35) Anatto; (36) Asparagus; (37) *Physalis Alkekengi* (outer shell and inner berry); (38) *Solanum dulcamara*; (39) *Solanum capsicastrum*; (40) *Cyphomandra betacea*; (41) *Cratægus crus-galli*; (42) *Pyrus aria*.

12. *Uncertainty*.—In several of these matters only two absorption-bands are with certainty distinguished. The third, clearly observable *e.g.* in the extract from the common marigold, requires further researches with more powerful light.

13. *Yellow Bodies with one Band*.—The yellow principles contained in yellow-wood or fustic, in the flowers of the *Calceolaria* of ornamental gardens, and in the yellow fæces of sucking infants, show but one absorption-band, in the blue.

14. *Uranium Salts*.—The yellow solutions of uranium salts exhibit two absorption-bands in the blue, which are very different from any of the above bands.

15. *Spectra of Yellow Bodies with continued Absorption of Blue*.—A great number of yellow substances, amongst them some of the most important dye-stuffs, show spectra with continued absorption of blue, indigo, and violet, without any bands. On dilution the absorption gradually recedes towards violet. To this class belong (1) Rhamnine, from French berries; (2) Luteoline, from weld; (3) Quercitrine, from extract of quercitron or fluorine; (4) Turmeric; (5) Picric, and (6) Purrée, or Indian yellow; the orange-coloured solution of the petals of (7) *Coreopsis lanceolata*, (8) *Helichrysum bracteatum*; the light-yellow solution of (9) *Viola lutea*, (10) *Acacia decurrens*, (11) *Helianthus macrophyllus* (?), (12) *Berberis Darwinii* (?), (13) *Gnaphalium fætidum*.

16. *Luteine not identical with Hematoidine or Cholophæine*.—Luteine differs entirely from hematoidine on the one, and from cholophæine on the other hand, and ought not, and after the elucidation of its spectral phenomena cannot, any longer be confounded with either of them.

17. *Error of Stædeler and Holm*.—The bodies described by Holm and Stædeler under the name of hematoidine are not hematoidine, but luteine.

18. *Robin's Hematoidine is Cholophæine*.—The bodies described by Valentiner, and by Robin, Riche, and Mercier, under the name of hematoidine, are not hematoidine, but cholophæine or bilirubine.

19. *Hematoidine peculiar*.—Hematoidine is a useful expression for certain microscopical crystals and amorphous bodies occurring in effused blood, the substance of which has not as yet been chemically isolated or defined.

20. *Luteine leads to new morphological views*.—The discovery of the identity of luteine from corpora lutea of mammals with that from yolks of eggs will probably lead to a revision of the present doctrines regarding the

homologies of the various parts of the ova of mammals and the eggs of birds and lower animals. Chemically the corpus luteum is the homologue of the yolk, genetically it is nearly so; but its use and destiny are totally different.

Note.—The foregoing researches are technical parts of inquiries carried on by the author at the Pathological Laboratory, St. Thomas's Hospital, for the Medical Department of the Privy Council, in continuation of researches already published in the Ninth and Tenth Reports of the Medical Officer of the Privy Council.

The special thanks of the author are due to Dr. Hooker, Director of the Royal Botanical Gardens, Kew, for the kindness and liberality with which he supplied, through Mr. Smith, the Curator, most of the botanical specimens examined in the course of this research.

II. "On Hydrofluoric Acid." By G. GORE, F.R.S. Received November 14, 1868.

(Abstract.)

A. *Anhydrous Hydrofluoric Acid*.

This paper contains a full description of the leading physical and chemical properties of anhydrous hydrofluoric acid, and also an account of various properties of pure aqueous hydrofluoric acid. The author obtained the anhydrous acid by heating dry double fluoride of hydrogen and potassium to redness in a suitable platinum apparatus (shown by a figure accompanying the paper), and states the conditions under which it may be obtained in a state of purity.

The composition and purity of the anhydrous acid are shown and carefully verified by various methods of analysis, both of the double fluoride from which it was prepared and of the acid itself; and particulars are given of all the circumstances necessary to insure reliable and accurate results. Nearly all the operations of preparing, purifying, analyzing, and examining the properties of the acid were conducted in vessels of platinum, with linings of paraffin, sulphur, and lampblack; articles of transparent and colourless fluor-spar were also employed in certain cases. Nearly all the manipulations with the acid were effected while the vessels containing it were immersed in a strong freezing-mixture of ice and crystallized chloride of calcium.

The pure anhydrous acid is a highly dangerous substance, and requires the most extreme degree of care in its manipulation. It is a perfectly colourless and transparent liquid at 60° Fahr., very thin and mobile, extremely volatile, and densely fuming in the air at ordinary temperatures, and absorbs water very greedily from the atmosphere. It was perfectly retained in platinum bottles, the bottle having a flanged mouth with a platinum plate secured with clamp-screws, and a washer of paraffin.

A number of attempts were made, finally with success, to determine the molecular volume of the pure anhydrous acid in the gaseous state, the acid in these cases being prepared by heating pure anhydrous fluoride of silver with hydrogen in a suitable platinum apparatus over mercury. Particulars are given of the apparatus employed and of the manipulation. The results obtained show that one volume of hydrogen, in uniting with fluorine, produces not simply one volume of gaseous product as it does when uniting with oxygen, but two volumes, as in the case of its union with chlorine. The gaseous acid transferred to glass vessels over mercury did not corrode the glass, or render it dim in the slightest degree during several weeks, provided that moisture was entirely absent.

The author concludes that the anhydrous acid he has obtained is destitute of oxygen, not only from the various analyses and experiments already referred to, but also, 1st, because the double fluoride from which it was prepared, when fused and electrolyzed with platinum electrodes, evolved abundance of inflammable gas at the cathode, but no gas at the anode, although oxides are by electrolysis decomposed before fluorides; 2nd, because the electrolysis of the acid with platinum electrodes yielded no odour of ozone, whereas the aqueous acid of various degrees of strength evolved that odour strongly; and, 3rd, because the properties of the acid obtained from hydrogen and fluoride of silver agree with those of the acid obtained from the double salt. He considers also that the acid obtained from pure fluor-spar and monohydrated sulphuric acid heated together in a platinum retort is free from oxygen and water.

The specific gravity of the anhydrous liquid acid was several times determined, both in a specific-gravity bottle of platinum, and also by means of a platinum float submerged and weighed in the acid. Concordant and reliable results were obtained; the specific gravity found was 0.9879 at 55° Fahr., that of distilled water being = 1.000 at the same temperature.

The anhydrous acid was much more volatile than sulphuric ether. Its boiling-point was carefully determined in a special apparatus of platinum, and was found to be 67° Fahr. Not the slightest sign of freezing occurred on cooling the acid to -30° Fahr. (= -34° 5 C.); and it is highly probable that its solidifying temperature is a very great many degrees below this. Its vapour-tension at 60° Fahr. was also approximately determined, and was found to be = 7.58 lbs. per square inch. On loosening the lid of a bottle of the acid at 60° Fahr., the acid vapour is expelled in a jet like steam from a boiler; this, together with the low boiling-point, the extremely dangerous and corrosive nature of the acid, and its great affinity for water, illustrates the very great difficulty of manipulating with it and retaining it in a pure state. Nevertheless, by the contrivances described, and by placing the bottles in a cool cellar (never above a temperature of 60° Fahr.), the author has succeeded in keeping the liquid acid perfectly, without loss and unaltered, through the whole of the recent hot summer.

The electrical relations of different metals &c. in the acid were found to

be as follows at 0° Fahr. :—zinc, tin, lead, cadmium, indium, magnesium, cobalt, aluminium, iron, nickel, bismuth, thallium, copper, iridium, silver, gas-carbon, gold, platinum, palladium.

Numerous experiments were made of electrolyzing the anhydrous acid with anodes of gas-carbon, carbon of *lignum-vitæ* and of many other kinds of wood, of palladium, platinum, and gold. The gas-carbon disintegrated rapidly ; all the kinds of charcoal flew to pieces quickly ; and the anodes of palladium, platinum, and gold were corroded without evolution of gas. The acid with a platinum anode conducted electricity much more readily than pure water ; but with one of gold it scarcely conducted at all. These electrolytic experiments presented extreme difficulties, and were conducted in a platinum apparatus (shown by a figure) specially devised for the purpose. The particulars of the conditions and results obtained are described in the paper. Various mixtures of the anhydrous acid with monohydrated nitric acid, with sulphuric anhydride, and with monohydrated sulphuric acid were also electrolyzed by means of platinum anodes, the particulars and results of which are also described.

To obtain an idea of the *general* chemical behaviour of the pure anhydrous acid, numerous substances (generally anhydrous) were immersed in separate portions of the acid in platinum cups, kept at a low temperature (0° to —20° Fahr.). The acid had scarcely any effect upon any of the metalloids or noble metals ; and even the base metals in a state of fine powder did not cause any evolution of hydrogen. Sodium and potassium behaved much the same as with water. Nearly all the salts of the alkali and alkaline-earth metals produced strong chemical action. Various anhydrides (specified) dissolved freely. Strong aqueous hydrochloric acid produced active effervescence. The alkalies and alkaline earths united strongly with the acid. Peroxides gave no effect. Numerous oxides (specified) produced strong chemical action, some of them dissolving. Some nitrates were not chemically affected ; others (those of lead, barium, and potassium) were decomposed. Fluorides generally were unchanged ; but those of the alkali-metals and of thallium produced different degrees of chemical action, those of ammonium, rubidium, and potassium uniting powerfully. Numerous chlorides were also unaffected, whilst those of phosphorus (the *solid* one only), antimony (the perchloride), titanium, and of the alkaline-earth and alkali metals, were decomposed with strong action, and generally with effervescence. The chlorates of potassium and sodium were also decomposed with evolution of chloric acid ; the bromides of the alkaline-earth and alkali metals behaved like their chlorides. Bromate of potassium rapidly set free bromine. Numerous iodides were unaffected ; but those of the alkaline-earth and alkali metals were strongly decomposed, and iodine (in some cases only) set free. The anhydrous acid decomposed all carbonates with effervescence, and those of the alkaline-earth and alkali metals with violent action. Borates of the alkalies also produced very strong action. Silico-fluorides of the alkali metals dissolved with effervescence.

All sulphides, except those of the alkaline-earth and alkali metals, exhibited no change; the latter evolved sulphuretted hydrogen violently. Bisulphite of sodium dissolved with effervescence. Sulphates were variously affected. The acid chromates of the alkali metals dissolved with violent action to blood-red liquids, with evolution of vapour of fluoride of chromium. Cyanide of potassium was violently decomposed, and hydrocyanic acid set free. Numerous organic bodies (specified) were also immersed in the acid; most of the solid ones were quickly disintegrated. The acid mixed with pyroxylic spirit, ether, and alcohol, but not with benzole; with spirit of turpentine it exploded, and produced a blood-red liquid. Gutta percha, india-rubber, and nearly all the gums and resins were rapidly disintegrated and generally dissolved to red liquids. Spermaceti, stearic acid, and myrtle wax were but little affected, and paraffin not at all. Sponge was also but little changed. Gun-cotton, silk, paper, cotton-wool, calico, gelatine, and parchment were instantly converted into glutinous substances, and generally dissolved. The solution of gun-cotton yielded an inflammable film on evaporation to dryness. Pinewood instantly blackened.

From the various physical and chemical properties of the anhydrous acid, the author concludes that it lies between hydrochloric acid and water, but is much more closely allied to the former than to the latter. It is more readily liquefied than hydrochloric acid, but less readily than steam; like hydrochloric acid it decomposes all carbonates; like water it unites powerfully with sulphuric and phosphoric anhydrides, with great evolution of heat. The fluorides of the alkali metals unite violently with hydrofluoric acid, as the oxides of those metals unite with water; the hydrated fluorides of the alkali metals also, like the hydrated fixed alkalies, have a strongly alkaline reaction, and are capable of expelling ammonia from its salts. It may be further remarked that the atomic number of fluorine lies between that of oxygen and chlorine; and the atomic number of oxygen, added to that of fluorine, nearly equals that of chlorine.

B. Aqueous Hydrofluoric Acid.

Under the head of the aqueous acid the author enumerates the various impurities usually contained in the commercial acid, and describes the modes he employed to detect and estimate them, and to estimate the amount of HF in it. The process employed by him for obtaining the aqueous acid in a very high degree of purity from the commercial liquid, is also fully described. It consists essentially in passing an excess of sulphuretted hydrogen through the acid, then neutralizing the sulphuric and hydrofluosilicic acids present by carbonate of potassium, decanting the liquid after subsidence of the precipitate, removing the excess of sulphuretted hydrogen by carbonate of silver, distilling the filtered liquid in a leaden retort with a condensing-tube of platinum, and, finally, rectifying.

The effect of cold upon the aqueous acid was briefly examined, the result being that a comparatively small amount of hydrofluoric acid lowers the freezing-point of water very considerably.

The chemico-electric series of metals &c. in acid of 10 per cent. and in that of 30 per cent. were determined. In the latter case it was as follows:—zinc, magnesium, aluminium, thallium, indium, cadmium, tin, lead, silicon, iron, nickel, cobalt, antimony, bismuth, mercury, silver, copper, arsenic, osmium, ruthenium, gas-carbon, platinum, rhodium, palladium, tellurium, osmi-iridium, gold, iridium. Magnesium was remarkably unacted upon in the aqueous acid. The chemico-electric relation of the aqueous acid to other acids with platinum was also determined.

Various experiments of electrolysis of the aqueous acid of various degrees of strength were made with anodes of platinum. Ozone was evolved, and, with the stronger acid only, the anode was corroded at the same time. Mixtures of the aqueous acid with nitric, hydrochloric, sulphuric, selenious, and phosphoric acids were also electrolyzed with a platinum anode, and the results are described.

III. "On a momentary Molecular Change in Iron Wire."

By G. GORE, F.R.S. Received November 14, 1868.

Whilst making some experiments of heating a strained iron wire to redness by means of a current of voltaic electricity, I observed that, on disconnecting the battery and allowing the wire to cool, during the process of cooling the wire *suddenly elongated*, and then gradually shortened until it became quite cold.

On attempting, some little time afterwards, to repeat this experiment, although a careful record of the conditions of the experiment had been kept, it was with some difficulty, and after numerous trials, that I succeeded in obtaining the same result. Having again obtained it, I next examined and determined the successful conditions of the experiment, and devised the following arrangement of apparatus.

A A (fig. 1) is a wooden base 61 centimetres long and 15·5 centimetres wide. B and C are binding-screws; they are provided with small brass mercury-cups fixed in the heads of the screws for attachment of the wires of a voltaic battery. D is a binding-screw for holding fast the sliding wire hook E. F is a cylindrical binding-screw, fixed to the sliding wire G, which is held fast by the binding-screw B. H is the iron or other wire (or ribbon) to be heated: one end of this wire passes through the screw F and is tightly secured by it, whilst the other end is held fast by the cylindrical binding-screw I; the binding-screw I has a small projecting bent piece of copper wire secured to it, which dips into a little shallow dish or cup of mercury, J; and the mercury in this cup is connected by a screw and strip of brass to the binding-screw C. K is a stretched band of vulcanized india-rubber, attached at one end to the hook of the wire E, and



at the other end to the hook L (see fig. 2). The cylindrical binding-screw I has a hook by which it is attached to the loop M (fig. 2). N is an axis suspended delicately upon centres, and carrying a very light index pointer O. The hook L and loop M are separate pieces of metal, and move freely upon an axis, P (fig. 2). The distance from the centre of the axis N to that of P is 12.72 millimetres ($=0.5$ inch), and to the top of the index pointer 25.45 centimetres ($=10.0$ inches); every movement horizontally, therefore, of the loop M is attended by a movement, twenty times the amount, of the top of the pointer. Q is a screw for supporting the axis N. I have found it convenient to put the zero-figure of the index towards the left-hand side of the index-plate. R is a separate piece of wood fitting into a rectangular hole in the base board; it carries a graduated rule, S, for measuring the length of the wire to be heated, and is easily removed, so that the wire may, if necessary, be heated by means of a row of Bunsen's burners. The rule T is used when measuring the amount of strain. U is a vertical stud or pin of brass (of which there are two) for limiting the range of movement of the pointer O.

In using this apparatus, a straight wire or ribbon, H, of a suitable length and thickness was inserted, the index pointer brought to 0 by adjustment of the sliding-wire G, and a suitable amount of strain (varying from less than two ounces to upwards of twenty) put upon the wire by adjusting the sliding hooked wire E. One pole of a voltaic battery, generally consisting of six Grove's elements, was connected with the binding-screw C, and the other pole then inserted in the mercury-cup of B. As soon as the needle O attained a maximum or stationary amount of deflection, the battery-wire was suddenly removed from B, and the wire allowed to cool. The movement of the needle O was carefully watched both during its movement to the right hand and also during its return, to see if any irregularity of motion occurred.

Wires of the following metals and alloys were employed:—palladium, platinum, gold, silver, copper, iron, lead, tin, cadmium, zinc, brass, german-silver, aluminium, and magnesium; metallic ribbon was also employed in certain cases.

In these experiments the thickness and length of the conducting-wire or ribbon had to be carefully proportioned to the quantity and electromotive power of the current, so as to produce in the first experiments with each metal only a very moderate amount of heat; thinner (and sometimes also shorter) wires were then successively used, so as ultimately to develop sufficient heat to make the metal closely approach its softening or fusion-point. The battery employed consisted in each case of six Grove's cells, each cell containing two zinc plates $3\frac{3}{4}$ inches wide, and a platinum plate 3 inches wide, each immersed about 5 inches in their respective liquids. The amount of tension imparted by the elastic band required to be carefully adjusted to the cohesive power of each metal; if the stretching power was too weak, the phenomenon sought for was not clearly deve-

loped; and if too great, the wire was overstretched or broken when it approached the softening-point. The amount of strain imparted was approximately measured by temporarily substituting the body of a small spring balance for the hooked wire F. The heated wire must be protected from currents of cold air.

With wires of iron 0.65 millimetre thick (size "No. 23") and 21.5 centimetres long, strained to the extent of 10 ounces or more, and heated to full redness, the phenomenon was clearly developed. As an example, the needle of the instrument went with regularity to 18.5 of index-plate; the current was then stopped; the needle instantly retreated to 17.75, then as quickly advanced to 19.75, and then went slowly and regularly back, but not to zero. If the temperature of the wire was not sufficiently high, or the strain upon the wire not enough, the needle went directly back without exhibiting the momentary forward movement. The temperature and strain required to be sufficient to actually stretch the wire somewhat at the higher temperature. A higher temperature with a less degree of strain, or a greater degree of strain with a somewhat lower temperature, did not develop the phenomenon. The wire was found to be permanently elongated on cooling. The amount of elongation of the wire during the momentary molecular change was usually about $\frac{1}{40}$ part of the length of the heated part of the wire; but it varied in different experiments; it was greatest in amount when the maximum degrees of strain were applied. The molecular change evidently includes a diminution of cohesion at a particular temperature during the process of cooling; and it is interesting to notice that at the same temperature during the *heating*-process no such loss of cohesion (nor any increase of cohesion) takes place; a certain temperature and strain are therefore not alone sufficient to produce it; the condition of *cooling* must also be included. The phenomena which occur during cooling are not the exact converse of those which take place during heating.

The phenomenon of elongation of iron wire during the process of cooling evidently lies within very narrow limits; it could only be obtained (with the particular battery employed) with wires about 21.5 centimetres ($=8\frac{7}{8}$ inch) long, and about 0.65 millimetre ($=$ Nos. 22 & 23 of ordinary wire-gauge) thick, having a strain upon them of 10 ounces or upwards; with a weaker battery the phenomenon could only be obtained by employing a shorter and thinner wire.

The experiment may easily be verified in a simpler manner by stretching an iron wire about 1.0 millimetre diameter between two fixed supports, keeping it in a sufficient and proper degree of tension by means of an elastic band, then heating it to full redness by means of a row of Bunsen's burners, and, as soon as it has stretched somewhat, suddenly cutting off the source of heat. In some experiments of this kind, with a row (42 centimetres long) of 21 burners and a row (76 centimetres long) of 43 burners, and the wire attached to a needle with index-plate, as in the figure, conspicuous effects were obtained; but the momentary elongation was relatively

much less (in one instance $\frac{1}{800}$ of the length of the heated part) than when a battery was employed, apparently in consequence of the wire being less intensely heated.

A large number of experiments were made with wires of palladium, platinum, gold, silver, copper, lead, tin, cadmium, zinc, brass, german-silver, aluminium, and magnesium (wire and ribbon), diminishing the length and thickness of the wire in each case, and adjusting the tension until suitable temperature and strain were obtained; but in no instance could a similar molecular change to that observed in iron be detected. Palladium and platinum wires of different lengths, thickness, and degrees of strain were examined at various temperatures, up to that of a white heat; but no irregularity of cohesion, except that of gradual softening at the higher temperatures, was observed; they instantly contracted with regular action on stopping the current. Several gold wires were similarly examined at different temperatures up to that of a full red heat; no irregularity occurred either during heating or cooling; but little tension (about 4 ounces) was applied, on account of the weak cohesion of this metal. Wires of silver similarly examined would only bear a strain of about 2 ounces, and a temperature of feeble red heat visible in daylight; no irregularity of elongation or contraction occurred during heating and cooling. By employing exactly the proper temperature and strain, a very interesting phenomenon was observed; the wire melted distinctly *on its surface* without fusing in its interior, although the surface was most exposed to the cooling influence of the air; this occurred without the wire breaking, as it would have done if its interior portion had melted; the phenomenon indicates the passage of the electricity by the *surface* of the wire in preference to passing by its interior. Wires of copper expanded regularly until they became red-hot; they then contracted slightly (notwithstanding the strain applied to them), probably in consequence of a cooling effect of increased radiation produced by the oxidized surface, as a similar effect occurred with brass and german-silver*. On stopping the current the wire contracted without manifest irregularity. Wires of lead and tin were difficult to examine by this method, on account of their extremely feeble cohesion and the low temperature at which they softened: wires about 1.63 millimetre diameter, 25.5 centimetres long (with a strain upon them of about one ounce), were employed; no irregularity was detected. Wires of cadmium from 1.255 millimetre to 1.525 millimetre thick, and 24.2 centimetres long (with a strain of two ounces), exhibited a slight irregularity of expansion at the lower temperatures; they elongated, and also cooled, with extreme slowness, more slowly than those of any other metal. Wires of zinc exhibited a slight irregularity of expansion, like those of cadmium; the most suitable ones were about 25 centimetres long and 1.2 millimetre in diameter, with a strain of 10 ounces. Wires of brass and german-silver, when heated to redness,

* This supposition does not agree with the results obtained with iron wire, which also oxidizes freely.

behaved like those of copper in expanding regularly until a maximum was attained, and then contracting slightly to a definite point whilst the battery remained connected ; on stopping the current they contracted without irregularity. When examined at lower temperatures, with a greater degree of strain, no irregularity was observed. Various wires of aluminium were examined ; the most suitable was one 0·88 millimetre thick, 20·4 centimetres long, with a strain of 12 ounces ; no irregularity was observed at any temperature below redness ; aluminium expanded and cooled very slowly, but less so than cadmium. Various wires and ribbon of magnesium were also examined below a red heat, but no irregularity of cohesion, except that due to gradual softening by heat, was detected.

All the metals examined exhibited gradual loss of cohesion at the higher temperatures if a suitable strain was applied to develope it. It is probable that if the fractions of time occupied by the needle in passing over each division of the index were noted, and the wire perfectly protected from currents of air, small irregularities of molecular or cohesive change might be detected by this method ; cadmium and zinc offer a prospect of this kind.

This molecular change would probably be found to exist in large masses of wrought iron as well as in the small specimens of wire which I have examined, and would come into operation in various cases where those masses are subjected to the conjoint influence of heat and strain, as in various engineering operations, the destruction of buildings by fire, and other cases.

IV. "On the Development of Electric Currents by Magnetism and Heat." By G. GORE, F.R.S. Received November 14, 1868.

I have devised the following apparatus for demonstrating a relation of current electricity to magnetism and heat.

A A, fig. 3, is a wooden base, upon which is supported, by four brass clamps, two, B, B, on each side, a coil of wire, C ; the coil is 6 inches long, $1\frac{1}{2}$ inch external diameter, and $\frac{3}{8}$ of an inch internal diameter, lined with a thin glass tube ; it consists of 18 layers, or about 3000 turns of insulated copper wire of 0·415 millim. diameter (or size No. 26 of ordinary wire-gauge) ; D is a permanent bar-magnet held in its place by the screws E, E, and having upon its poles two flat armatures of soft iron, F, F, placed edgewise. Within the axis of the coil is a straight wire of soft iron, G, one end of which is held fast by the pillar-screw H, and the other by the cylindrical binding-screw I ; the latter screw has a hook, to which is attached a vulcanized india-rubber band, J, which is stretched and held secure by the hooked brass rod K and the pillar-screw L. The screw H is surmounted by a small mercury cup for making connexions with one pole of a voltaic battery, the other pole of the battery being secured to the pillar-screw M,

which is also surmounted by a small mercury cup, and is connected with the cylindrical binding-screw I by a copper wire with a middle flattened portion O to impart to it flexibility. The two ends of the fine wire coil are soldered to two small binding-screws at the back; those screws are but partly shown in the sketch, and are for the purpose of connexion with a suitable galvanometer. The armatures F, F are grooved on their upper edges, and the iron wire lies in these grooves in contact with them; and to prevent the electric current passing through the magnet, a small piece of paper or other thin non-conductor is inserted between the magnet and one of the armatures. The battery employed consisted of six Grove's elements (arranged in one series), with the immersed portion of platinum plates about 5 inches by 3 inches; it was sufficiently strong to heat an iron wire 1.03 millim. diameter and 20.5 centims. long to a low red heat.

By making the contacts of the battery in unison with the movements of the galvanometer-needles, a swing of about 12 degrees of the needles each way was obtained. The galvanometer was not a very sensitive one; it contained 192 turns of wire. Similar results were obtained with a coil 8 inches long and $1\frac{1}{4}$ inch diameter containing 16 layers, or about 3776 turns of wire of 0.415 millim. diameter (or No. 26 of ordinary wire-gauge), and a permanent magnet 10 inches long. Less effects were obtained with a 6-inch coil consisting of 40 layers, or about 10,000 turns of wire 0.10 millim. diameter, also with several other coils. The maximum effect of 12 degrees each way with six Grove's cells in one series was obtained when the wire became visibly red-hot, and this occurred with an iron wire 1.03 millim. diameter (or No. 19 of ordinary wire-gauge); but when employing ten such cells as a double series of five, the maximum effect was then obtained with an iron wire (size Nos. 17 and 18) 1.28 to 1.58 millim. diameter, the deflection being 16 degrees each way. By employing a still thicker wire and a battery of greater heating-power still greater effects were obtained.

The galvanometer was placed about 8 (and in some instances 12) feet distant from the coil. A reversal of the direction of the battery current did not reverse or perceptibly affect the current induced in the coil; but by reversing the poles of the magnet, the direction of the induced current was reversed. On disconnecting the battery, and thereby cooling the iron wire, a reversed direction of induced current was produced. By substituting a wire of pure nickel 24.5 centims. long and 2.1 millims. diameter, induced currents were obtained as with the iron, but they were more feeble. No induced current occurred by heating the iron wire if the magnet was absent; nor was any induced current obtained if the magnet was present and wires of palladium, platinum, gold, silver, copper, brass, or german-silver were heated to redness instead of iron wire; nor with a rod of bismuth 3.63 millims. diameter enclosed in a glass tube and heated nearly to fusion; it is evident, therefore, that the axial wire must be composed of a *magnetic* metal.

No continuous current (or only a very feeble one) was produced in the

coil by *continuously* heating the iron wire. In several experiments, by employing twelve similar Grove's elements as a double series of six intensity, an iron wire 1.56 millim. diameter was made *bright* red-hot; and by keeping the current continuous until the galvanometer-needles settled nearly at zero, and then suddenly disconnecting the battery, the needles remained nearly stationary during several seconds, and then went rapidly to about 10: this slow decline of the current during the first few seconds of cooling was probably connected with the "momentary molecular change of iron wire" during cooling which I have described in the preceding paper. The irregularity of movement of the needles did not occur unless the wire was *bright* red-hot, a condition which was also necessary for obtaining the molecular change.

The direction of the current induced by *heating* the iron wire was found by experiment to be the same as that which was produced by removing the magnet *from* the coil; therefore the heat acted simply by *diminishing* the magnetism, and the results were in accordance with, and afford a further confirmation of, the general law, that wherever there is increasing or decreasing magnetism, there is a tendency to an electric current in a conductor at right angles to it.

February 4, 1869.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President,
in the Chair.

The following communications were read:—

- I. "On Fossil Teeth of Equines from Central and South America, referable to *Equus conversidens*, *Equus tau*, and *Equus arcidens*."
By Professor OWEN, F.R.S. Received November 17, 1868.

(Abstract.)

The author, referring to his previous paper on the Equine fossil remains from the cavern of Bruniquel, finds, in the preliminary illustrations of the dental characters of existing species of the Horse-kind, the requisite and much-needed basis of comparison for the determination of other fossils of the Solidungulate group, and he devotes the present paper to the elucidation of those which have reached him from Central and South America.

He commences by referring to the type-specimens of teeth, from two localities in South America, on which he founded the species *E. curvidens*, describing it (in 1840) "as one coexisting with the Megatherium, Toxodon, &c. in that continent, and which had become extinct at a prehistoric period."

He then proceeds to describe more complete evidences of the dentition of an allied extinct Horse discovered by Don Antonio del Castillo, mining engineer, in newer Tertiary deposits of the Valley of Mexico. Besides repeating the originally described characters of the curvature of

the grinder, with a certain resemblance of enamel-pattern to the grinding-surface of the *E. curvidens*, they show a greater degree of curvature of the alveolar series of the upper jaw, with corresponding greater convergence of the right and left molar series toward the fore part of the palate, than in any previously described species of *Equus*.

Deciduous teeth of the *Equus conversidens* from the same deposits of the Valley of Mexico are described. Having determined these corroborative and distinctive characters of aboriginal and now extinct American horses, the author remarks, "It is unlikely, seeing the avidity with which the Indians of the Pampas have seized and subjugated the stray descendants of the European horses introduced by the Spanish 'Conquistadors' of South America, and the able use the nomad natives make of the multitudinous progeny of those war-horses at the present day, that any such tameable Equine should have been killed off or extirpated by the ancestors of the South-American aborigines." If, therefore, the fossil Equine teeth do belong, as the author deems that he has proved, to a species distinct from *Equus caballus*, Linn., "the circumstances of their discovery, and the fact of the extinction of such (curvident and conversident) species of Horse would point to some other cause than that of man's hostility to so useful an animal, and such doubt as to extinction by human means may then be extended to the contemporaries of the *Equus curvidens* and *E. conversidens*, viz. *Megatherium*, *Myiodon*, *Toxodon*, *Nesodon*, *Macrauchenia*, *Glyptodon*, *Mastodon*, &c."

The author next proceeds to describe fossil teeth from the upper and lower jaws, discovered by Don A. del Castillo in the same deposits of the Valley of Mexico, and referable to a third species of *Equus*, viz. *Equus tau*, Ow. Finally the author proceeds to the description of some fossil upper molar teeth from Pampas deposit, in the bed of a brook falling into the "Arroyo Negro" near Paysandi, Monte Video, showing characters more decisively distinct from any other known species of *Equus* than have hitherto been described.

The degree of curvature of the upper molar teeth exceeds that in *Equus curvidens*, and equals that in *Toxodon*; and the specific name "*arcidens*" is accordingly proposed for this aboriginal American species of Horse. It is compared with so much of the characters as have been given by Dr. Lund of his *Equus neogæus* and *E. principalis* from Brazilian caverns; and the differences from all other Equines which these species and the *E. arcidens* agree in presenting lead the author to view them as having, like the *Hippotherium* of Kaup, formed a generic group in the *Equidæ*, for which he proposes the name *Hippidion*.

The fossil teeth of *H. arcidens* were found associated with remains of *Megatherium* and *Glyptodon* in the above-named locality; the specimens were transmitted and presented to the British Museum (in 1867) by the Hon. W. G. Lettsom, Her Britannic Majesty's Minister at Monte Video.

This paper is illustrated by drawings of the specimens described.

II. "Compounds Isomeric with the Sulphocyanic Ethers.—III. Transformations of Ethylic Mustard-oil and Sulphocyanide of Ethyl." By A. W. HOFMANN, Ph.D., M.D., LL.D., F.R.S.
Received November 19, 1868.

In the present paper I beg leave to communicate to the Royal Society some experiments made for the purpose of testing the views which I have lately* advanced respecting the constitution of the mustard-oils and the sulphocyanic ethers isomeric with them. These experiments were exclusively performed in the ethyl-series. Not only is ethylamine much more readily prepared than the methyl-base, but the elucidation of the metamorphoses examined was not unfrequently facilitated by the selection of compounds for the construction of which the material had simultaneously been taken from the monocarbon- and dicarbon-series.

Action of Hydrogen in condicione nascendi upon Ethylic Mustard-oil.

I have, in the first place, examined this reaction, because experiments performed by M. Oeser† have already supplied some information on the behaviour of allylic mustard-oil under analogous conditions.

On adding zinc and hydrochloric acid to an alcoholic solution of ethylic mustard-oil an evolution of sulphuretted hydrogen becomes at once perceptible; it soon diminishes, but continues for several days. In the several stages of this process the gas evolved was examined for carbonic acid; but not a trace of this gas could be detected. As soon as the evolution of sulphuretted hydrogen has ceased, the liquid is found to contain a large quantity of fine white needles; when submitted to distillation, it yields the same body, which, passing over with the vapour of water and alcohol, collects in the form of white crystals upon the water in the receiver. If the residue be now allowed to cool, an additional quantity of the crystalline compound is deposited. Analysis and examination of the properties of these crystals have identified them with the substance generated by the action of sulphuretted hydrogen upon methylic aldehyde‡, to which I assigned the formula



stating at the same time that a higher molecular weight might possibly be found to belong to this sulphaldehyde of the methyl-series.

On adding strong soda-lye to the liquid containing chloride of zinc, from which the crystals have been separated, until the oxide at first precipitated is redissolved, a strongly alkaline layer collects on the surface of the solution, which may be considerably augmented by the intervention of a small quantity of alcohol. This layer was removed and separated from adhering soda by distillation. When the very volatile distillate was saturated with

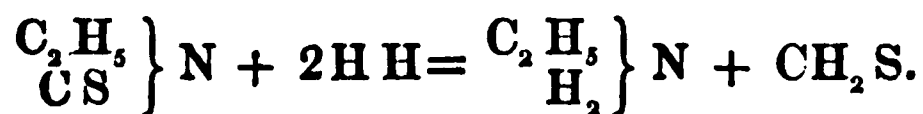
* Proceedings, vol. xvii. p. 67.

† Ann. Chem. Pharm. vol. cxxxiv. p. 7.

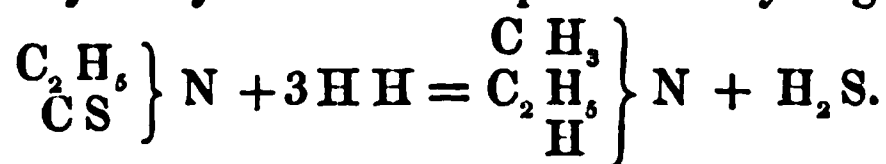
‡ Proceedings of the Royal Society, vol. xvi. p. 156.

hydrochloric acid and mixed with perchloride of platinum, the well-known hexagonal tables of the ethylamine-platinum-salt were at once deposited. The mother-liquor was found to contain a second salt, much more soluble both in water and alcohol, which was precipitated by ether. By recrystallization it was obtained in magnificent orange-red needles, which on analysis exhibited the composition of the platinum-salt of methyl-ethylamine.

The interpretation of these observations presents no difficulty. There are obviously two parallel reactions to be distinguished. In the first place (and this is doubtless the principal reaction) there are two molecules of hydrogen inserted at the place in which the two compounds of ethylic mustard-oil are joined together—this insertion giving rise to the formation, on the one hand, of ethylamine, the mother-compound of the mustard-oil, and on the other hand, of methylic sulphaldehyde, the hydrogen-derivative of bisulphide of carbon.



Or the substance, under the powerful influence of hydrogen, splits in another place; three molecules of hydrogen penetrate into the fragment of the bisulphide, and the products of this secondary and subordinate transformation are methyl-ethylamine and sulphuretted hydrogen.



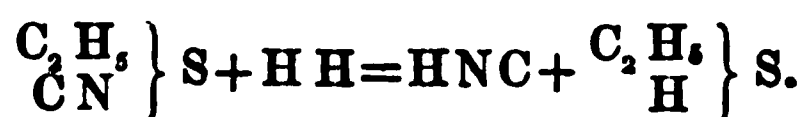
Action of Hydrogen in condicione nascendi on Sulphocyanide of Ethyl.

On treating the isomeric sulphocyanide of ethyl with zinc and hydrochloric acid, sulphuretted hydrogen is also evolved; it contains, however, so abundant an admixture of mercaptan, that the brown spot of sulphide of lead appearing upon lead-paper held over the mouth of the flask in which the reaction takes place is surrounded by a yellow ring of mercaptide of lead.

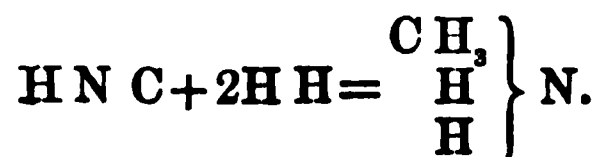
In order to examine the gases evolved, they were passed in the first place through lime-water, then through hydrate of sodium, and lastly through acetate of lead and perchloride of mercury; ultimately they were collected in a gas-holder. The lime-water remained clear; hence the gases did not contain carbonic acid; the liquid, however, was saturated with hydrocyanic acid. By the hydrate of sodium large quantities of sulphuretted hydrogen and ethyl-mercaptan were fixed; the two metallic salts, lastly, retained some ethylic mercaptan and ethylic sulphide. The gas collected in the gas-holder was transmitted once more through lime-water and sodic hydrate, and then passed over a layer of incandescent oxide of copper. Together with water, large quantities of carbonic acid were thus produced, proving that the hydrogen contained a carbonated gas, which I do not hesitate to consider marsh-gas, although verification of this assumption, by transformation of the hydrocarbon into tetrachloride, has still to be adduced.

On distilling the liquid, when the evolution of sulphuretted hydrogen has ceased, there are evolved, together with a small quantity of the latter gas, ethylic mercaptan, sulphide and, under certain conditions, even bisulphide of ethyl, these several compounds being easily recognized by their special reactions. The residue, when heated with hydrate of sodium, disengages abundant quantities of ammonia, and also an appreciable amount of methylamine.

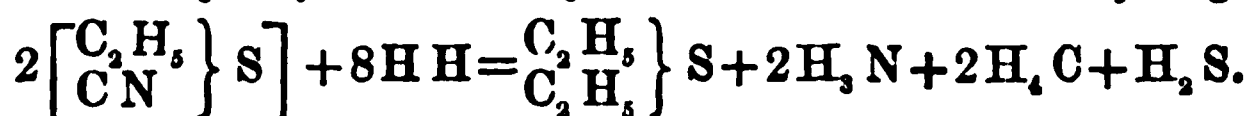
If these varied results be taken into consideration, the action of nascent hydrogen upon sulphocyanide of ethyl would appear to be a very complicated process. The principal transformation of the body is nevertheless extremely simple. Here, again, the point of junction of the two components of sulphocyanide of ethyl is the vulnerable part. A molecule of hydrogen entering at this point, between the sulphur and the carbon, the compound separates into hydrocyanic acid on the one hand and ethylic mercaptan on the other.



All the other products belong to secondary reactions. In contact with hydrogen, hydrocyanic acid is converted into methylamine.



Sulphide of ethyl, ammonia, marsh-gas, and sulphuretted hydrogen may be looked upon as resulting from a further and deeper destruction of the molecule of sulphocyanide of ethyl under the influence of hydrogen.



Action of Hydrogen in condicione nascendi upon Allylic Mustard-oil.

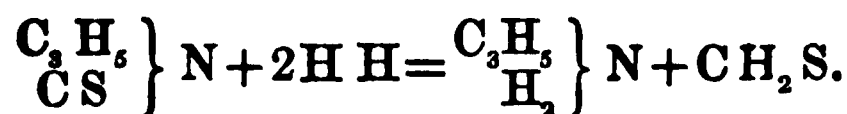
According to the experiments of M. Oeser already quoted, the mustard-oil *par excellence*, when submitted to nascent hydrogen, would appear to undergo a transformation different from that of its ethylic congener. M. Oeser represents the metamorphosis of the allyl-compound by the following equation:—



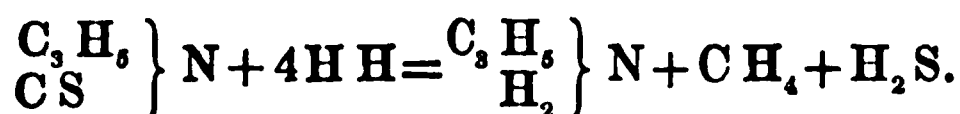
This equation, however, obviously represents no reduction process; the nascent hydrogen has no share in this reaction, which is simply accomplished under the influence of the elements of water.

To clear up this anomaly, the experiments above described were repeated in the allyl-series. On treating mustard-oil with zinc and hydrochloric acid, an abundant evolution of sulphuretted hydrogen was observed, but (under the conditions, at all events, in which I repeatedly performed this experiment) the gas did not contain a trace of carbonic acid; on the other hand, large quantities of the sulphaldhyde of the methyl-series were inva-

riably obtained. If the spirit which is employed in dissolving the mustard-oil to be reduced be dilute, a fine crystallization of the sulphaldehyde is frequently observed after the lapse of a few hours. Together with this compound allylamine is generated in large proportion. The principal reaction is thus seen to be exactly the same as with ethylic mustard-oil.

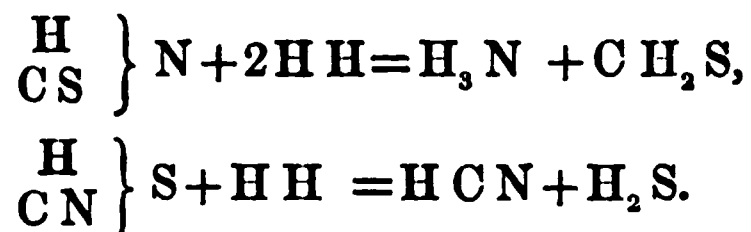


The sulphuretted hydrogen would therefore likewise belong to a secondary reaction. Vainly, however, have I endeavoured to trace in the mother-liquor of the allylamine-platinum salt the existence of the platinum compound of a second base—of methyl-allylamine for instance; though working on a rather large scale, I was unable to detect even a trace of such a compound. The origin of the sulphuretted hydrogen, however, could not be doubtful. In the gas evolved during the reaction, a large amount of a gaseous hydrocarbon (very probably marsh-gas) was present, as could be easily proved by burning the gas, after an appropriate purification, with oxide of copper.



Action of Hydrogen in condicione nascendi upon Hydrosulphocyanic Acid.

It would have been strange if in the course of these researches I had omitted to investigate the action of zinc and hydrochloric acid upon sulphocyanide of potassium. The result of this experiment could scarcely be doubtful—evolution of sulphuretted hydrogen in torrents, copious separation of sulphuretted methylic aldehyde, in the residue ammonia and methylamine. The reaction is not without interest, since the hydrosulphocyanic acid liberated by the hydrochloric acid exhibits the principal metamorphosis both of mustard-oil and the isomeric sulphocyanide of ethyl.



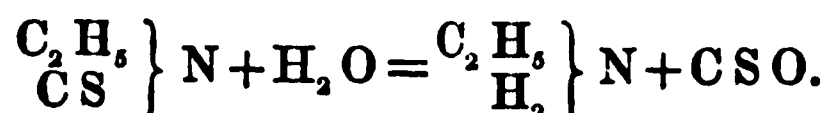
Hydrocyanic acid, it is true, is not directly observed in this case; but we meet with its hydrogen-derivative, methylamine.

Together with the behaviour of these bodies under the influence of reducing agents, I have studied the action of water and of acids upon the mustard-oils and the ethers isomeric with them.

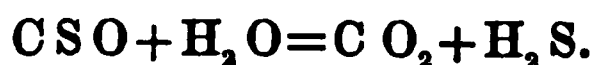
Action of Water and Hydrochloric Acid upon Ethylic Mustard-oil.

When exposed in sealed tubes together with water to a temperature of 200° for eight or ten hours, ethylic mustard-oil splits up into ethylamine, carbonic acid, and sulphuretted hydrogen. The idea naturally suggests itself that two water-molecules act in succession. Under the influence of

the first, ethylic mustard-oil would yield ethylamine and sulphoxide of carbon.



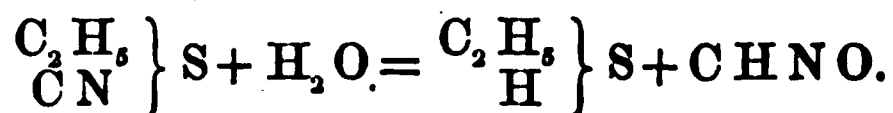
The action of the second would transform the rather unstable sulphoxide of carbon into carbonic acid and sulphuretted hydrogen.



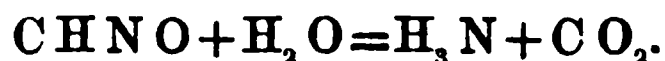
The decomposition remains essentially the same if, instead of water, hydrochloric acid be employed. The reaction, however, is very considerably accelerated; in fact an hour's digestion at 100° is sufficient to split up the mustard-oil right off into ethylamine, carbonic acid, and sulphuretted hydrogen.

Action of Water and Hydrochloric Acid upon Sulphocyanide of Ethyl.

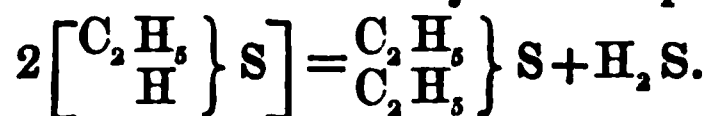
Water, even at rather high temperatures, acts but very slowly upon sulphocyanide of ethyl. Even at 200°, after several days' digestion, very appreciable quantities of the compound had remained unaltered. The reaction, as might have been expected, proceeds much more rapidly in the presence of concentrated hydrochloric acid. The ultimate products of transformation are sulphuretted hydrogen, sulphide of ethyl, carbonic acid, and ammonia. Here, again, we have by no means to deal with direct products of decomposition. Probably the compound, with the cooperation of one molecule of water, changes in the first place into ethyl-mercaptan and cyanic acid.



Under the influence of a second molecule of water, cyanic acid yields ammonia and carbonic acid.

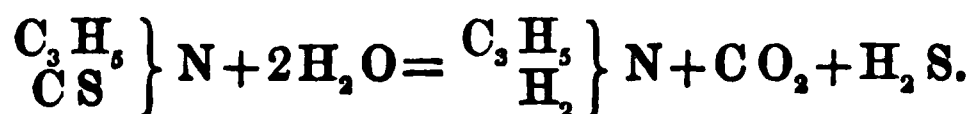


Sulphide of ethyl and sulphuretted hydrogen, lastly, have to be looked upon as products of transformation of ethylic mercaptan.



Action of Water and Hydrochloric Acid upon Allylic Mustard-oil.

Whilst engaged with these researches, I have incidentally made also some experiments upon the mustard-oil *par excellence*. As might have been expected, when submitted to the action of water at a high temperature, and more especially in the presence of hydrochloric acid, allylic mustard-oil splits up into allylamine, carbonic acid, and sulphuretted hydrogen.

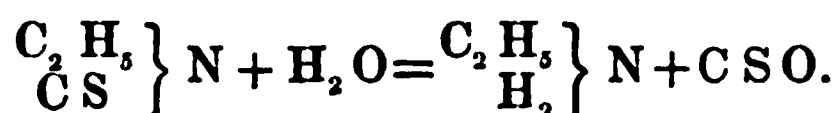


Simultaneously, however, another reaction takes place, which up to the present moment I have not yet been able to elucidate. Together with

allylamine there is formed a second liquid base having a very high boiling-point, which yields an amorphous platinum-salt. It remains behind as an oily layer, not volatilizable with the vapour of water, when the product of the action of hydrochloric acid upon mustard-oil, for the purpose of purifying the allylamine, is distilled with soda.

Action of Sulphuric Acid upon Ethylic Mustard-oil.

Dilute sulphuric acid acts like water and hydrochloric acid. Highly characteristic, however, is the behaviour of ethylic mustard-oil towards concentrated sulphuric acid. The two liquids mix with considerable evolution of heat, and after a few moments a powerful disengagement of gas takes place, which, if the reaction be promoted by the application of heat, may be increased to explosive violence. The gas evolved is inflammable, and burns with a blue flame. It has a peculiar odour, essentially different from that of bisulphide of carbon, or of sulphuretted hydrogen; from the latter it differs, moreover, by its having no action upon lead-paper. These are the characteristics of sulphoxide of carbon, lately discovered by von Than. The residue contains sulphate of ethylamine.



In contact with water, more especially in the presence of an alkali, sulphoxide of carbon is converted into carbonic acid and sulphuretted hydrogen. Treatment of ethylic mustard-oil with concentrated sulphuric acid thus enables us to arrest halfway the transformation which is accomplished under the influence of water.

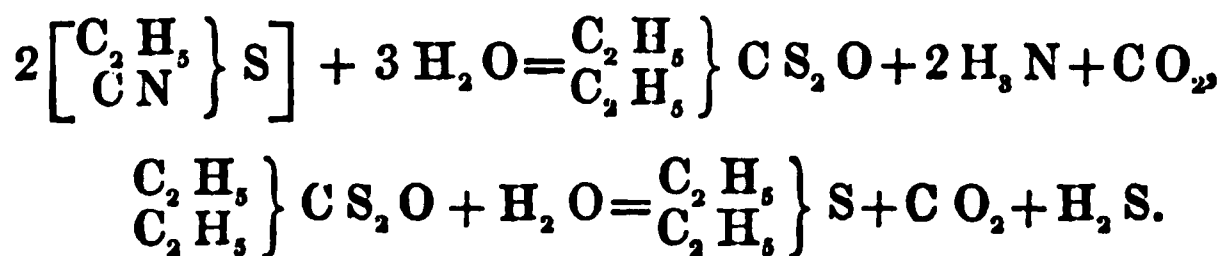
Action of Sulphuric Acid upon Sulphocyanide of Ethyl.

Dilute sulphuric acid acts but slowly upon sulphocyanide of ethyl; concentrated acid, on the other hand, attacks the compound with great energy, powerful evolution of heat and disengagement of carbonic and sulphurous acids taking place. On distilling the liquid after addition of water, sulphuretted ethereal products are volatilized; the deep-brown residue, when treated with lime, yields abundance of ammonia. In the presence of these observations, it appeared very probable that the action of sulphuric acid resembled that of water and of hydrochloric acid, and that in this case likewise the ethyl-group was eliminated in combination with sulphur.

Interesting experiments on the action of sulphuric acid upon sulphocyanide of ethyl, lately communicated to the Chemical Society of Berlin* by Messrs. Schmitt and Glutz, have indeed verified this assumption; but these experiments have proved, moreover, that the reaction, exactly as in the case of the transformation of ethylic mustard-oil, is capable of stopping at an intermediate stage, inasmuch as the above-named chemists have succeeded in isolating from the products of the reaction a compound isomeric

* Sitzungsberichte der chemischen Gesellschaft, 1868, S. 182.

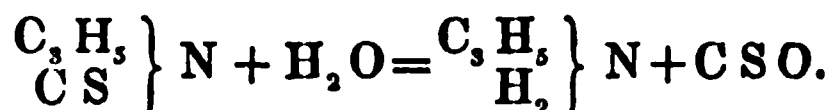
with xanthic ether. Accordingly the metamorphosis of sulphocyanide of ethyl under the influence of sulphuric acid would appear to be accomplished in the following two phases:—



It is true Messrs. Schmitt and Glutz, when submitting their ether to the action of water, obtained mercaptan, whilst, according to my observations, the products of decomposition of sulphocyanide of ethyl with hydrochloric acid are sulphide of ethyl and hydrosulphuric acid. But since two molecules of mercaptan contain the elements of one molecule of sulphide of ethyl and one molecule of sulphuretted hydrogen, the final products of decomposition of sulphocyanide of ethyl by water and by hydrochloric and sulphuric acids are virtually the same.

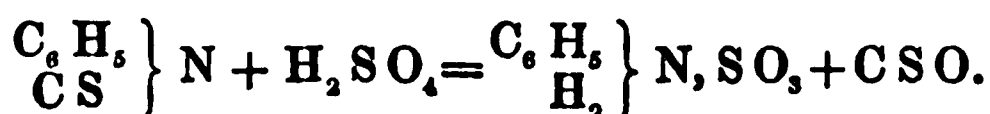
Action of Sulphuric Acid upon Allylic Mustard-oil.

Mustard-oil *par excellence*, when treated with sulphuric acid, as might have been expected, exactly imitates the behaviour of the ethyl-compound. Sulphoxide of carbon is evolved with effervescence; the residue contains sulphate of allylamine.



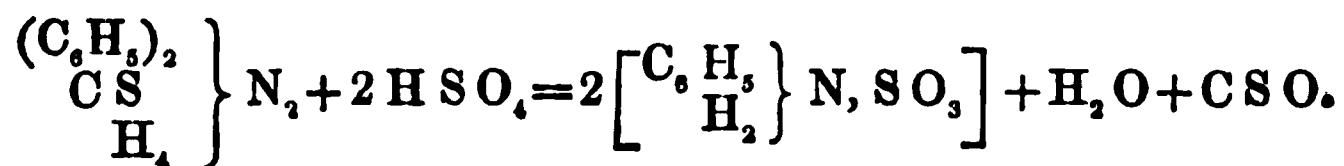
The reaction proceeds with the utmost regularity and precision. The liquid scarcely becomes coloured; mixed with water and distilled with hydrate of sodium, it yields abundance of perfectly pure allylamine. It would be difficult to imagine a more elegant and expeditious process for preparing this interesting base in a state of perfect purity. Allylamine thus obtained was identified by the analysis of the platinum-salts, the preparation of the terribly smelling allyl-formonitril, which I shall describe in another paper, and, lastly, by its retransformation into mustard-oil, according to the method described in my last paper*.

Also phenylic and tolylic mustard-oils exhibit an analogous behaviour with sulphuric acid; in these cases likewise sulphoxide of carbon is evolved; the base, however, does not remain as sulphate, but in the form of an amine-sulphate in the residue.



Even phenyl-sulphocarbamide, as well as its homologues and analogues, is changed in this sense.

* Proceedings of the Royal Society, vol. xvii. p. 67.



In the presence of an excess of sulphuric acid, the water-molecule eliminated is without influence upon sulphoxide of carbon.

Action of Nitric Acid upon Ethylic Mustard-oil.

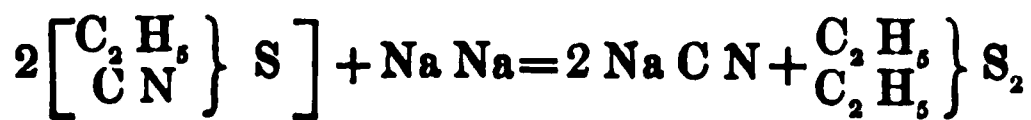
I have still to say a few words respecting the behaviour of ethylic mustard-oil with nitric acid, although the experience acquired in the several experiments I have described could not possibly leave any doubt on the nature of this reaction. Here, again, the ethyl-group separates, united with nitrogen, in the form of ethylamine, from the molecule, while the carbon and sulphur of the group C S are burnt and eliminated in the form of carbonic and sulphuric acids. The same deportment is exhibited by the homologues of ethylic mustard-oil, and also by the allyl-compound.

The products which are generated by the action of nitric acid upon sulphocyanide of ethyl and its homologues are known. According to the experiments of Muspratt, sulphocyanide of ethyl yields with nitric acid ethyl-sulphurous acid,



Accordingly there is also in this case elimination of the ethyl-group, in the form of a sulphur-compound.

In conclusion it may be stated that I have examined the action of several other chemical agents, and more especially of the alkali-metals and their hydrates, on the two classes of isomeric compounds. Most of the experiments, however, which I have made in this direction are not yet completed, and I will here only briefly allude to the elegant transformation which sulphocyanide of ethyl suffers in contact with metallic sodium. A powerful reaction ensues, cyanide of sodium and sulphide of ethyl being formed.



It affords me great pleasure to mention the energy and intelligence with which Dr. Bulk has assisted me during the performance of the experiments described in this paper. My best thanks are due to him.

III. "On the Solar Protuberances." By M. JANSSEN. In a Letter to WARREN DE LA RUE, F.R.S. Communicated by Mr. DE LA RUE. Received February 2, 1869.

"Je voulais vous écrire depuis longtemps pour vous faire part de mes travaux et vous remercier des bonnes et puissantes introductions que je vous dois. J'attendais que j'eusse quelque chose de complet à vous présenter, et j'ai été ainsi entraîné peu à peu.

“ Vous connaissez maintenant la méthode que j’ai proposé pour l’étude des protubérances, et dont Mr. Norman Lockyer avait eu l’idée, m’écrit-on, depuis deux années. J’ignorais cela, et c’est une circonstance qui a été favorable à Mr. Lockyer ; car si j’avais su qu’on travaillait sur ce sujet, naturellement j’aurais, en citant l’idée émise, fait connaître immédiatement par le télégraphe les résultats que j’obtenais dans l’Inde. Mais je ne regrette pas que Mr. Lockyer soit parvenu séparément à la confirmation de ses idées. Je trouve qu’il le méritait. Nous restons aussi indépendants l’un de l’autre.

“ Je dois vous dire que je viens de découvrir que les protubérances se rattachent au soleil par une atmosphère dont l’hydrogène forme la base, au moins générale, et qui enveloppe la photosphère. Cette atmosphère est basse, à niveau fort inégal et tourmenté ; souvent elle ne dépasse pas les saillies de la photosphère. Les protubérances ne paraissent être que des portions soulevées, projetées, détachées de cette enveloppe. J’étudie aussi les taches, sujet difficile, mais qui promet d’importantes notions sur la constitution du soleil.

“ J’aurai l’honneur, à l’issue de ces études, d’envoyer un mémoire à votre Société Royale, comme hommage rendu à sa grande et juste célébrité, et aussi comme témoignage de reconnaissance des bonnes réceptions que j’ai eues dans l’Inde et chez vous toutes les fois que j’y vais.

“ Mais, en attendant, je vous prie de vouloir bien lui communiquer les résultats dont je vous fais part ici.

“ Je suis, en ce moment, à Simla, résidence d’été du Gouverneur, où j’ai un beau ciel et 8000 de vos pieds au-dessous de moi. Je profite de ces heureuses conditions pour aborder ici toutes sortes d’études.

“ Je serai encore dans le Bengale en Mars. J’aurai donc le temps de recevoir une lettre de vous, ce qui me ferait un bien grand plaisir. Je n’ai ici aucune nouvelle scientifique d’Angleterre, et bien peu de France.”

February 11, 1869.

Dr. W. B. CARPENTER, Vice-President, in the Chair.

The following communications were read :—

- I. “ On the Structure and Development of the Skull of the Common Fowl (*Gallus domesticus*).” By W. KITCHEN PARKER, F.R.S. Received November 25, 1868.

(Abstract.)

In a former paper (Phil. Trans. 1866, vol. clvi. part 1, pp. 113–183, plates 7–15) I described the structure and development of the skull in the Ostrich tribe, and the structure of the adult skull of the Tinamou—a bird which connects the Fowls with the Ostriches, but which has an essentially struthious skull.

That paper was given as the first of a proposed series, the subsequent communications to be more special (treating of one species at a time) and carrying the study of the development of the cranium and face to much earlier stages than was practicable in the case of the struthious birds.

Several years ago Professor Huxley strongly advised me to concentrate my attention for some considerable time on the morphology of the skull of the Common Fowl; that excellent advice was at length taken, and the paper now offered is the result.

A full examination of the earlier conditions of the chick's skull has cost me much anxious labour; but my supply of embryonic birds (through the kindness of friends)* was very copious, and in time the structure of the early conditions of the skull became manifest to me.

The *earliest* modifications undergone by the embryonic head are not given in this paper: they are already well known to embryologists; and my purpose is not to describe the general development of the embryo, but merely the skeletal parts of the head.

These parts are fairly differentiated from the other tissues on the fourth day of incubation, when the head of the chick is a quarter of an inch (3 lines) in length; this in my paper is termed the "first stage." The next stage is that of the chick with a head from 4 to 5 lines in length, the third 8 to 9 lines, and so on. The ripe chick characterizes the "fifth stage;" and then I have worked out the skull of the chicken when three weeks, two months, three months, and from six to nine months old, the skull of the aged Fowl forming the "last stage."

During all this time (from their first appearance to their highly consolidated condition in old age) the skeletal parts are undergoing continual change, obliteration of almost all traces of the composite condition of the early skull being the result—except where there is a hinge, for there the parts retain perfect mobility.

Here it may be remarked that although the Fowl is only an approach to what may be called a typical Bird, yet its skull presents a much greater degree of coalescence of primary centres than might have been expected from a type which is removed so few steps from the semistruthious Tinamou, a bird which retains so many of its cranial sutures.

The multiplicity of parts in the Bird's skull at certain stages very accurately represents what is persistent in the Fish, in the Reptile, and to some degree in certain Mammals; but the skull at first is as simple as that of a Lamprey or a Shark, and, in the Bird above all other Vertebrates, reverts in adult age to its primordial simplicity—all, or nearly all, its metamorphic changes having vanished and left no trace behind them.

Although in this memoir I have no business with the Fish, yet all along I have worked at the Fish equally with the Bird, the lower type being taken as a guide through the intricacies of the higher; and here the Car-

* Dr. Murie is especially to be thanked for his most painstaking kindness in this respect.

tilaginous and the Osseous Fishes are never fairly out of sight. 'The Reptile, and especially the Lizard, has been less helpful to me, on account of its great specialization.

On the fourth day of incubation the cranial part of the notochord is two-thirds the length of the primordial skull, but it does not quite reach the pituitary body ; it lies therefore entirely in the occipito-otic region. The fore part of the skull-base extends horizontally very little in front of the pituitary space ; this arises from the fact that the "mesocephalic flexure" has turned the "horns of the trabeculæ" under the head. Thus at this stage the nasal, oral, and postoral clefts are all seen on the under surface of the head and neck of the chick. At this time the facial arches have begun to chondrify ; but only the quadrate, the Meckelian rod, and the lower thyro-hyal are really cartilaginous ; the other parts are merely tracts of thickened blastema or indifferent tissue.

In the second stage an orbito-nasal septum has been formed ; the "horns of the trabeculæ" have become the "nasal alæ," and an azygous bud of cartilage has grown downwards between them ; this is the "prænasal" or snout cartilage ; it is the *axis* of the intermaxillary region. At the commencement of this second stage the primordial skull stands on the same morphological level as that of the ripe embryo of the Sea-turtle ; at the end of this stage it has become struthious ; and now parosteal tracts (the angular, surangular, dentary, &c.) appear round the mandibular rod.

In this abstract I shall not trace the changes of the skull any further, but conclude with a few remarks on the nomenclature of certain splints, and as to the nature of the great basicranial bones.

Some years ago I found that certain birds (for instance the Emeu) possessed an additional maxillary bone on each side ; knowing that the so-called "turbinal" of the Lizard and Snake was one of the maxillary series, I set myself to find the homologies of these splints. Renaming the reptilian bones "prævomers," on account of their relation to the vomer, and supposing the feeble maxillaries of the Bird to represent them, I considered that the true maxillaries were to be found in those newly found cheek-bones of the Emeu and some other birds.

After discussion with Professor Huxley I have determined to drop the term "prævomer," and to call the supposed turbinal of the Lizard "septo-maxillary," and the additional bone in the Bird's face "postmaxillary."

In many Birds, but not in the Fowl, the "septo-maxillary" is largely represented—not, however, as a distinct osseous piece, but as an outgrowth of the true maxillary.

With regard to the basicranial bones, I have now satisfied myself that the "parasphenoid" of the Osseous Fish and the Batrachian reappears in the Bird as three osseous centres—all true "parostoses," as in the single piece of the lower types ; these three pieces are, the "rostrum" of the basisphenoid and the two "basitemporals."

These three centres rapidly coalesce to form one piece, the exact counter-

part of the Ichthyic and Batrachian bone; but just as this coalescence begins, ossification proceeds inwards from these "parostoses," and affect the overlying cartilage, the cartilage of the basisphenoidal region having no other osseous nuclei. This process of the extension inwards of ossification from a splint-bone to a cartilaginous rod or plate I have already called "osseous grafting" *.

In my former paper the basisphenoidal "rostrum" and "basitemporals" were classed with the endoskeletal bones; they will in the present paper be placed in the parosteal category, in accordance with their primordial condition.

By the careful following out of these and numerous other details I have corrected and added to my previous knowledge of the early morphological conditions of the Bird's cranium, and at the same time, I trust, have contributed to an enlarged and more accurate conception of the history and meaning of the Vertebrate skull in general.

II. "Determinations of the Dip at some of the principal Observatories in Europe by the use of an instrument borrowed from Kew Observatory." By Lieut. ELAGIN, Imperial Russian Navy Communicated by BALFOUR STEWART, LL.D. Received February 2, 1869.

Before I give a short account of the observations and the results deduced from them, I beg to express in the first place my best thanks to Dr Balfour Stewart, Director of the Kew Observatory, who, having heard of my desire to take the dip at different places, was so kind as to lend me an instrument from the Kew Observatory,—also to James Glaisher, Esq. F.R.S., &c., who furnished me with a tripod-stand, which I found to be of great use to me on some stations.

I may also remark that, having other duties to perform in obedience to instructions from the Russian Government, I could only devote a portion of my time to the observations of dip.

The instrument I had from Kew Observatory was one of Barrow's Dip Circles, furnished with two $3\frac{1}{2}$ -inch needles in the form generally used at the Observatory. The Dip-Circle used had been in use for some time at the Kew Observatory, until, it having been ascertained that one of its needles was somewhat deteriorated, it was replaced with that now in use.

Before I left Kew Observatory I was aware that one of the needles was not as good as might be desired; but as Mr. Stewart had no other circle suitable for my purpose, I considered it desirable to take this circle.

The observations were made according to the instructions of Lieut. General Sabine, given in the 'Admiralty Manual of Scientific Enquiry.'

The following Table I. shows the results of the observations with the circle from Kew; in it the name of station and the date of observation are

* See memoir "On the Shoulder-girdle and Sternum," Ray Soc. 1858, p. 10.

mentioned in the first column; in the second column is noted the particular needle used, and whether in the first series of observations the marked end or "N. Pole" was dipping, in which case it has been indicated by the word "direct;" in the case when the opposite or "S. Pole" was dipping first, it is indicated by the word "reversed." Under the head of marked end, each of the two results is that formed from the mean of four sets of observations; in one of these two results the marked end is made a north pole, and in the other it is a south pole. The headings of the remaining columns explain themselves.

TABLE I.

Station and Date.	Needle.	Marked end.		Means of the two results.	Means of separate Needles.	Means of both Needles.
		N. Pole.	S. Pole.			
1868.						
Kew Observatory (Magnetic House).						
August	d h	A ₁ direct.	67° 59'·50	68° 7'·80	68° 3'·65	} 68 2·55
"	5 0·5	"	68 12·37	67 50·12	68 1·25	
"	5 23·0	"	68 9·50	67 57·00	68 3·25	
"	7 2·0	"	68 4·12	68 0·00	68 2·08	
"	5 2·5	A ₂ direct.	68 9·40	68 3·20	68 6·20	
"	6 0·0	"	68 10·20	67 58·20	68 4·20	
} 68 5·20						
Royal Observatory, Green- wich (Magnetic Offices).						
August	7 23·5	A ₁ direct.	68 9·25	67 51·30	68 0·27	} 67 58·25
"	8 3·5	"	68 6 25	67 44·38	67 55·32	
"	9 23·0	"	68 13·12	67 49·88	68 1·50	
"	10 2·5	"	68 2·81	67 47·77	67 55·09	
"	10 3·5	A ₁ reversed.	68 4·69	67 47·77	67 56·23	
"	10 22·5	A ₁ direct.	67 59·50	67 52·06	67 55·78	
"	11 3·5	"	67 58·19	67 52·22	67 55·33	
"	11 22·0	"	68 5·56	67 49·12	67 57·34	
"	12 22·0	"	68 10·42	67 55·25	68 2·83	
"	13 0·5	"	68 6·60	67 55·10	68 0·85	
"	13 2·5	"	68 5·60	67 44·70	67 55·05	
"	14 0·5	A ₁ reversed.	68 5·11	68 0·45	68 2·93	
"	14 22·5	A ₁ direct.	68 6·59	67 49·00	67 57·84	
"	10 23·5	A ₂ direct.	68 3·50	67 55·38	67 59·44	
"	11 23·5	"	67 58·75	68 0·12	67 69·43	
"	15 0·5	"	68 2·12	67 57·19	67 59·65	
} 67 50·51						
} 67 58·88						
Norwich (Mr. Firth's gar- den, St. Giles Street).						
August	18 20	A ₁ direct.	68 30·8	68 13·4	68 22·10	} 68 16·93
"	21 3·5	"	68 17·0	68 5·6	68 11·30	
"	24 20·5	"	68 24·4	68 10·5	68 17·45	
"	21 3·5	A ₂ direct.	68 22·50	68 13·9	68 18·20	} 68 18·95
"	24 20·5	"	68 20·50	68 18·9	68 19·70	
(Mr. Gibson's garden, Bethel Street).						
August	24 23·0	A ₁ direct.	68 23·19	68 9·37	68 16·28	} 68 17·86
"	24 23·0	A ₂ direct.	68 23·12	68 15·50	68 19·31	

TABLE I. (continued).

Station and Date.	Needle.	Marked end.		Means of the two results.	Means of separate Needles.	Means of both Needles.
		N. Pole.	S. pole.			
1868.						
Brussels Observatory (Magnetic House).						
August 31 23	A ₁ direct.	67° 15.22	66° 59.12	67° 7.17	} 67 5.67	} 67 6.77
September 2 0	"	67 10.55	66 58.42	67 4.48		
" 4 6	"	67 6.70	67 4.00	67 5.35		
" 1 3	A ₂ direct.	67 5.10	67 4.45	67 4.75	} 67 7.87	
" 1 23	"	67 18.10	67 4.90	67 11.50		
" 2 4.5	"	67 14.00	67 5.67	67 9.83		
" 4 5.0	"	67 5.90	67 5.60	67 5.75		
Utrecht Meteorological Observatory (Magnetic House).						
September 9 0	A ₁ direct.	67 50.2	67 29.0	67 39.6	} 67 40.6	} 67 43.3
" 10 0	"	52.4	30.5	41.5		
" 8 2	A ₂ direct.	67 49.8	67 38.8	67 44.3	} 67 46.0	
" 8 23	"	49.8	45.4	47.6		
Vienna (Theresianum Garden, Magnetic House).						
September 19 0	A ₁ direct.	63 42.7	63 24.8	63 33.75	} 63 36.2	} 63 38.8
" 19 4	"	47.1	29.2	38.15		
" 21 0	"	44.0	29.7	36.80	} 63 41.40	
" 19 0	A ₂ direct.	63 44.3	63 40.5	63 42.40		
" 19 4	"	43.3	41.05	42.18		
" 21 0	"	42.3	36.80	39.60		
Munich Observatory (Magnetic House).						
September 29 22.5	A ₁ direct.	64 11.7	63 53.9	64 2.8	} 64 3.9	} 64 7.7
" 30 3.5	"	19.9	50.1	5.0		
" 29 3.5	A ₂ direct.	64 13.0	64 11.0	64 12.0	} 64 11.5	
" 29 22.5	"	14.9	6.9	10.9		
" 30 3.5	"	12.2	11.1	11.7		
Paris Observatory (in the garden close to the Magnetic House).						
October 14 22.5	A ₁ direct.	65 55.7	65 36.7	65 46.2	} 65 48.4	} 65 49.85
" 16 23.5	"	54.6	39.8	47.2		
" 20 1.0	"	2.2	41.7	51.95	} 65 51.3	
" 14 22.5	A ₂ direct.	65 55.1	65 45.2	65 50.20		
" 16 23.5	"	53.4	48.2	50.80		
" 20 1.0	"	56.95	48.55	52.75		
Royal Observatory, Greenwich (Magnetic Offices).						
December 3 2	A ₁ direct.	68 11.0	67 50.0	67 0.5	} 67 58.7	} 67 58.0
" 7 22	"	68 3.5	67 50.0	67 56.8		
" 3 2	A ₂ direct.	68 10.9			} 67 57.2	
" 7 22	"	68 1.1	67 54.4	67 57.2		

At the Royal Observatory, Greenwich, I took more observations with one needle than the other ; and the reason for that was, I found that this needle, A₁, gave two distinctly different positions : for instance, at times

dips were found which differed from those obtained at other times about seven minutes, whilst the other needle, A_2 , gave more uniform and satisfactory results; and this is also the reason I preferred to take the separate means for each needle, and then means of both needles, and to give to them equal weights, notwithstanding the number of observations is greater in one case than the other. The cause of needle A_1 giving different positions must be most probably in the axis of the needle, not in the agate plates; otherwise both needles would indicate the same difference.

Having given the results of my observations, I think it desirable to state the precautions I took to obtain the best results. First of all, whilst at the Royal Observatory, Greenwich, where I was for several months studying the several instruments in the magnetic department, through the kindness of the Astronomer Royal and Mr. Glaisher, I had made myself well acquainted with the necessary care in those observations; besides, I several times visited the Kew Observatory, through the kindness of Dr. Balfour Stewart, and took some observations of dip. At all times my first efforts were directed to have a firm support; next, to accurately levelling the instrument; third, to see that the agate plates were clean, that the axis of the needle was also clean and tested by the use of cork, that the needles were free from dust and damp, their ends being passed in and out of cork, and their surfaces wiped with wash-leather; and in damp weather increased attention was paid to everything; but, as a rule, observations were not made at such times; care was also had in determining the magnetic meridian corresponding, and in all cases several readings were taken in every position.

The results of observations of dip with local instruments at different places were as follows:—

Kew Observatory, monthly observations of dip with an instrument No. 33 Circle, of the same pattern I had made by Barrow; the length of the needle is about $3\frac{1}{2}$ inches. To compare No. 33 Circle with the Circle borrowed from Kew, I made simultaneous observations; the mean from six observations with two needles gave for No. 33 Circle $=68^\circ 2' 19$, and for the Circle I had from Kew $68^\circ 3' 8$, this result being $1' 6$ larger.

Royal Observatory, Greenwich.—Observations of dip are made frequently with Mr. Airy's dip instrument, described in the yearly volumes of observations at the Royal Observatory. Six needles of three different lengths are observed on the same instrument; the results derived from each separate needle seldom differ more than five minutes in the year. I took from the Royal Observatory observations the mean of the determined dip for the period from 1st July to 30th September, which was $=67^\circ 56' 15$, derived from twenty-seven observations, and nearly corresponds to the time of my observations. The dip obtained from my observations with Kew Circle was $=67^\circ 58' 88$, being $2' 73$ larger.

Brussels Observatory.—The observations of dip were made with an instrument of old English construction, which was made in the year 1828,

by the English makers Troughton and Simms; two needles about 8 inches long are observed, and the observations are made in the usual manner, in the magnetic meridian. The dip is observed at the beginning of each year, in the month of March or April; thus for the year 1868 there was one observation made with two needles the 30th of March, and the dip obtained was $67^{\circ} 11' \cdot 1$. The 5th of September Professor Quetelet's son, according to my wish, was so kind as to observe the dip, and obtained almost the same result (that is, $67^{\circ} 11' \cdot 0$), whilst from observations with the Kew Circle I obtained the dip $= 67^{\circ} 6' \cdot 77$, being $4' \cdot 2$ smaller.

Utrecht Meteorological Department.—The observations were made with an instrument not differing much from instruments of this class formerly used in England. It was constructed by Olland, a maker at Utrecht; the dip is observed every fortnight, in the middle and at the end of each month, with two needles about 8 inches in length. The results of the separate needles are very close to one another, and the dip is generally observed about 9 o'clock in the morning. Simultaneous observations were made by Mr. H. Welers Bethink and myself, each observing his own instrument. The dips obtained are as follows:—

With the Observatory instrument. $67^{\circ} 47' \cdot 7$

With the Kew Circle. $67^{\circ} 43' \cdot 3$, being $4' \cdot 4$ less.

Vienna Meteorological Department.—The Dip Circle was made by Repsold, and a description of it is given in the 'Magnetische und meteorologische Beobachtungen zu Prag bei Karl Kreil, sechster Jahrgang, vom Januar bis 31. December 1845.' The instrument is provided with eight needles, whose lengths are about 9 inches each; the axis of the needle is perforated, and can be turned round the centre of the needle through a definite angle; each dip is deduced from eight separate sets of observations, by turning each time the axis of the needle through an angle of about 45° . The separate results derived in this way differ sometimes about 1° from each other, and the means for separate needles differ in some cases about $20'$; so that the determinations of dip with this instrument are very uncertain, whilst the labour to obtain a pretty good result is very great; at the same time a single determination with one of the Barrow's Circles gives a result nearer to the truth. I must say here that the present Director of the Meteorological Institution in Vienna, Professor Yelynak, was so pleased with the instrument I had from Kew, that he asked me to order one for him of Mr. Barrow.

The mean result derived from the observations from January 1 to September 18, 1868, is $= 63^{\circ} 32' \cdot 06$; the result obtained with the Kew Circle is $63^{\circ} 38' \cdot 80$, being larger by $6' \cdot 7$.

Munich.—Regular observations of absolute dip are not made at the Observatory. The last determined dip was in 1866, in September, and was $64^{\circ} 16' \cdot 8$. The dip for the present time is deduced from the variation of horizontal force and the constant relation between it and the dip as found by Dr. Lamont from a large series of observations; according to this the

dip for September 1868 is $64^{\circ} 10' \cdot 9$. The observed dip with the Kew Circle is $64^{\circ} 7' \cdot 7$, being smaller by $3' \cdot 2$.

Paris.—The observations of dip at the Observatory are made with an instrument of Gambey regularly three times every day—that is, at 9 o'clock in the morning, at noon, and at 4 o'clock in the afternoon. This instrument gives only the variations of dip. To determine the absolute dip, a long series of simultaneous comparisons with a Dip-circle have been made. The following dip is deduced from observations with this instrument on the same days as my observations : it is $= 65^{\circ} 45' \cdot 3$; the result I obtained with the Kew Circle is $65^{\circ} 49' \cdot 85$, being $4' \cdot 5$ larger.

These were all the stations at which I was able to make satisfactory observations; but as at most of these stations comparative observations at adjoint stations had been made before and the differences found between them, there was less need to extend my observations beyond the principal observatories.

Table II. contains the dips observed at the different stations before mentioned, and the differences between the local instruments and the Circle from Kew.

TABLE II.

September 1868. Stations.	Dips observed with local instruments.	Dips observed with Circle from Kew.	Local instru- ments—Kew Circle.
Normal Observatory, Kew	68 2·19	68 3·80	—1·61
Royal Observatory, Greenwich ...	67 56·15	67 58·88	—2·73
Norwich.....		68 17·86	
Brussels Observatory	67 11·00	67 6·77	+4·23
Utrecht, Meteorol. Department ...	67 47·70	67 43·30	+4·40
Vienna, Meteorol. Institution	63 32·06	63 38·80	—6·74
Munich Observatory		64 7·70	
Paris Observatory.....	65 45·30	65 49·85	—4·55

I will now endeavour to deduce the most probable dips at each station. First I shall deduce the dip at Munich, as no observations are made there specially for dip, by taking the differences between the values I found at Munich and at every other station, and applying it to the result as found with the local instrument at each place. Thus the dip I obtained at Kew was $68^{\circ} 3' \cdot 80$, and at Munich was $64^{\circ} 7' \cdot 70$; the difference is $3^{\circ} 56' \cdot 1$; and applying this to the result as found at Kew by the Kew instrument $68^{\circ} 2' \cdot 19$, I deduce $64^{\circ} 6' \cdot 09$ as the dip for Munich ; and treating all the other stations in a similar way I find :—

Dip, from Kew.....	= 64 6·09
„ Greenwich ..	= 4·97
„ Norwich	= 7·70
„ Brussels	= 11·93
„ Utrecht	= 12·10
„ Vienna.....	= 0·96
„ Paris	= 3·15
Mean	64 6·70

And in a similar way I calculated the dips for all the stations, taking Utrecht first, because the dip found for Munich from this station gave a result differing from the mean the most of any; and then I treated Brussels in the same way, it being the next in order of discordance, and so on.

I thus formed Table III., giving the calculated dips, the observed dips with the local instruments and the Kew Circle, and the corrections for the Kew Circle.

TABLE III.

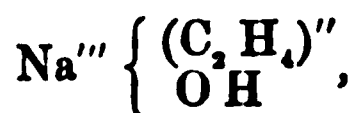
Stations.	Calculated Dips.	Dips observed with local instruments.	Dips observed with Circle from Kew.	Calcul. — Obs. with Kew Circle.
Kew	68° 1' 69	68° 2' 19	68° 3' 80	—2' 11
Greenwich ...	67 56' 84	67 56' 15	67 58' 88	—2' 04
Norwich.....	68 15' 50	67 17' 86	68 17' 86	—2' 36
Brussels	67 4' 12	67 11' 00	67 6' 77	—2' 65
Utrecht	67 41' 37	67 47' 70	67 43' 30	—1' 93
Vienna	63 36' 73	63 32' 06	63 38' 80	—2' 07
Munich	64 6' 70	64 7' 70	—1' 00
Paris	65 47' 80	65 45' 30	65 49' 85	—2' 05
			Mean	—2' 03

This Table shows that the Circle from Kew gave at all stations the dip about 2' too large; and only for Munich this difference is but 1', which shows that the calculated dip for Munich is a little too large.

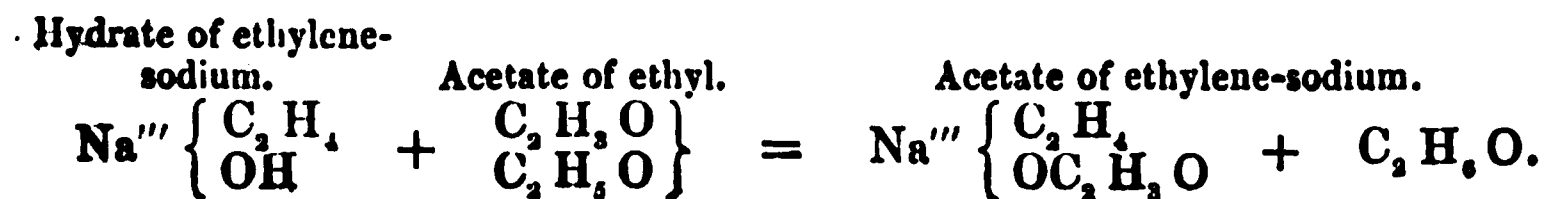
III. “On a New Class of Organo-metallic Bodies containing Sodium.” By J. ALFRED WANKLYN, Professor of Chemistry in the London Institution. Communicated by Professor E. W. BRAYLEY. Received February 6, 1869.

Up to the present time organo-metallic bodies containing ethylene in union with the metal have been often sought, but never recognized.

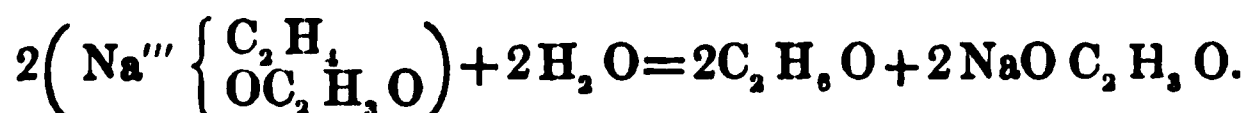
I have to announce the existence of organo-metallic compounds of ethylene with the alkali-metals. In ethylate of sodium, or at any rate in the substance which is produced by heating up to 200° C. the well-known crystals got by acting on alcohol with sodium, I see the hydrated oxide of ethylene-sodium—



which, as I have recently shown, yields alcohol and a new compound on being heated with the ethers of the fatty acids: thus



Acetate of ethylene-sodium yields alcohol and common acetate of soda on treatment with water :—



The extreme lightness of the so-called ethylate of sodium (it swims in ether) is a reason for regarding it as a compound belonging to a less condensed order of sodium-compound than ordinary sodium-compounds. The property of yielding up its olefine in the shape of alcohol when it is treated with water is a reason for assigning to the new compound given by the action of acetic ether the above formula, and shows that the olefine is associated with the alkali-metal, not with the acid.

IV. "On the Temperature of the Human Body in Health." By SYDNEY RINGER, M.D. (Lond.), Professor of Materia Medica in University College, London, and the late ANDREW PATRICK STUART. Communicated by Dr. BASTIAN. Received December 18, 1868.

(Abstract.)

These observations were conducted by the authors in order to learn with minuteness the fluctuations of the temperature in health. They were performed on persons of different ages, and were in many instances continued through the night and day.

The temperature was noted every hour, and on many occasions much more frequently.

The following subjects are discussed in this communication :—

1. The daily variation of the temperature.
2. The effects of food on the temperature.
3. The effects of cold baths on the temperature.
4. The effects of hot baths on the temperature.

From their observations and experiments the authors have drawn the following conclusions :—

The average maximum temperature of the day in persons under 25 years of age is $99^{\circ}\cdot 1$ Fahr. ; of those over 40, $98^{\circ}\cdot 8$ Fahr.

There occurs a diurnal variation of the temperature, the highest point of which is maintained between the hours of 9 A.M. and 6 P.M. At about the last-named hour the temperature slowly and continuously falls, till, between 11 P.M. and 1 A.M., the maximum depression is reached. At about 3 A.M. it again rises, and reaches very nearly its highest point by 9 A.M.

The diurnal variation in persons under 25 amounts, on an average, to $2^{\circ}\cdot 2$ Fahr. ; but in persons between 40 and 50 it is very small, the average being not greater than $0^{\circ}\cdot 87$ Fahr. ; nay, on some days no variation whatever happens. In these elderly people the temperature still further differs

from that of young persons; for in the former the diurnal fall occurs at any hour, and not, as is the case with young persons, during the hours of night.

Concerning the influence of food on the temperature of the body the authors have concluded that none of the diurnal variations is in any way caused by the food we eat.

The experiments to prove this conclusion are very numerous. Some were made with the breakfast, others with the dinner and tea; but all point to the conclusion just stated.

This important question is very fully discussed in the section devoted to it.

By cold baths both the surface of the body and the deep parts were lowered in temperature. The temperature of the surface was in some instances reduced to 88° Fahr.; but the heat so soon returned to all parts as to show that the cold bath is of very little use as a refrigerator of the body.

The cold bath produced no alteration in the time or amount of the diurnal variation. This began at the same hour, and reached the same amount as on those days when no bath was taken.

By hot-water or vapour baths the heat of the body could be raised very considerably. Thus, on some occasions, when using the general hot bath, the temperature under the tongue was noted to be between 103° and 104° Fahr., a fever temperature.

The body being heated considerably above the point at which combustion could maintain it, it was then shown with what rapidity heat may be lost, simply by radiation and evaporation. The particulars of these results are given in the paper.

The experiments tend to prove that hot baths in no way affect the diurnal variation of the temperature.

V. "Preliminary Note of Researches on Gaseous Spectra in relation to the Physical Constitution of the Sun." By EDWARD FRANKLAND, F.R.S., and J. NORMAN LOCKYER, F.R.A.S.
Received February 11, 1869.

1. For some time past we have been engaged in a careful examination of the spectra of several gases and vapours under varying conditions of pressure and temperature, with a view to throw light upon the discoveries recently made bearing upon the physical constitution of the sun.

Although the investigations are by no means yet completed, we consider it desirable to lay at once before the Royal Society several broad conclusions at which we have already arrived.

It will be recollected that one of us in a recent communication to the Royal Society pointed out the following facts:—

i. That there is a continuous envelope round the sun, and that in the

spectrum of this envelope (which has been named for accuracy of description the "chromosphere") the hydrogen line in the green corresponding with Fraunhofer's line F takes the form of an arrowhead, and widens from the upper to the lower surface of the chromosphere.

ii. That ordinarily in a prominence the F line is nearly of the same thickness as the C line.

iii. That sometimes in a prominence the F line is exceedingly brilliant, and widens out so as to present a bulbous appearance above the chromosphere.

iv. That the F line in the chromosphere, and also the C line, extend on to the spectrum of the subjacent regions and re-reverse the Fraunhofer lines.

v. That there is a line near D visible in the spectrum of the chromosphere to which there is no corresponding Fraunhofer line.

vi. That there are many bright lines visible in the ordinary solar spectrum near the sun's edge.

vii. That a new line sometimes makes its appearance in the chromosphere.

2. It became obviously, then, of primary importance—

i. To study the hydrogen spectrum very carefully under varying conditions, with the view of detecting whether or not there existed a line in the orange, and

ii. To determine the cause to which the thickening of the F line is due.

We have altogether failed to detect any line in the hydrogen spectrum in the place indicated, *i. e.* near the line D; but we have not yet completed all the experiments we had proposed to ourselves.

With regard to the thickening of the F line, we may remark that, in the paper by MM. Plücker and Hittorf, to which reference was made in the communication before alluded to, the phenomena of the expansion of the spectral lines of hydrogen are fully stated, but the cause of the phenomena is left undetermined.

We have convinced ourselves that this widening out is due to pressure, and not appreciably, if at all, to temperature *per se*.

3. Having determined, then, that the phenomena presented by the F line were phenomena depending upon and indicating varying pressures, we were in a position to determine the atmospheric pressure operating in a prominence, in which the red and green lines are nearly of equal width, and in the chromosphere, through which the green line gradually expands as the sun is approached*.

With regard to the higher prominences, we have ample evidence that the gaseous medium of which they are composed exists in a condition of *excessive* tenuity, and that at the lower surface of the chromosphere itself the pressure is very far below the pressure of the earth's atmosphere.

* Will not this enable us ultimately to determine the temperature?

The bulbous appearance of the F line before referred to may be taken to indicate violent convective currents or local generations of heat, the condition of the chromosphere being doubtless one of the most intense action.

4. We will now return for one moment to the hydrogen spectrum. We have already stated that certain proposed experiments have not been carried out. We have postponed them in consequence of a further consideration of the fact that the bright line near D has apparently no representative among the Fraunhofer lines. This fact implies that, assuming the line to be a hydrogen line, the selective absorption of the chromosphere is insufficient to reverse the spectrum.

It is to be remembered that the stratum of incandescent gas which is pierced by the line of sight along the sun's limb, the radiation from which stratum gives us the spectrum of the chromosphere, is very great compared with the radial thickness of the chromosphere itself; it would amount to something under 200,000 miles close to the limb.

Although there is another possible explanation of the non-reversal of the D line, we reserve our remarks on the subject (with which the visibility of the prominences on the sun's disk is connected) until further experiments and observations have been made.

5. We believe that the determination of the above-mentioned facts leads us necessarily to several important modifications of the received theory of the physical constitution of our central luminary—the theory we owe to Kirchhoff, who based it upon his examination of the solar spectrum. According to this hypothesis, the photosphere itself is either solid or liquid, and it is surrounded by an atmosphere composed of gases and the vapours of the substances incandescent in the photosphere.

We find, however, instead of this compound atmosphere, one which gives us nearly, or at all events mainly the spectrum of hydrogen; (it is not, however, composed necessarily of hydrogen alone; and this point is engaging our special attention;) and the tenuity of this incandescent atmosphere is such that it is extremely improbable that any considerable atmosphere, such as the corona has been imagined to indicate, lies outside it,—a view strengthened by the fact that the chromosphere bright lines present no appearance of absorption, and that its physical conditions are not statical.

With regard to the photosphere itself, so far from being either a solid surface or a liquid ocean, that it is cloudy or gaseous or both follows both from our observations and experiments. The separate prior observations of both of us have shown:—

i. That a gaseous condition of the photosphere is quite consistent with its continuous spectrum. The possibility of this condition has also been suggested by Messrs. De La Rue, Stewart, and Loewy.

ii. That the spectrum of the photosphere contains bright lines when the

limb is observed, these bright lines indicating probably an outer shell of the photosphere of a gaseous nature.

iii. That a sun-spot is a region of greater absorption.

iv. That occasionally photospheric matter appears to be injected into the chromosphere.

May not these facts indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself or extremely near to it, instead of in an extensive outer absorbing atmosphere? And is not this conclusion strengthened by the consideration that otherwise the newly discovered bright lines in the solar spectrum itself should be themselves reversed on Kirchhoff's theory? this, however, is not the case. We do not forget that the selective radiation of the chromosphere does not necessarily indicate the whole of its possible selective absorption; but our experiments lead us to believe that, were any considerable quantity of metallic vapours present, their bright spectra would not be entirely invisible in all strata of the chromosphere.

February 18, 1869.

Lieut.-General SABINE, President, in the Chair.

The Most Noble the Marquis of Salisbury and the Right Hon. Lord Houghton were admitted into the Society.

The following communications were read:—

- I. "On the Structure of Rubies, Sapphires, Diamonds, and some other Minerals." By H. C. SORBY, F.R.S., and P. J. BUTLER.
Received December 8, 1868.

[Plate VII.]

For many years Mr. Butler has had the opportunity of examining very many rubies, sapphires, and diamonds, and has taken advantage of it in forming a most interesting collection, cut and mounted as microscopical objects. He had very carefully studied the included fluid-cavities, and ascertained many curious facts. Mr. Sorby had for some time paid much attention to the microscopical structure of crystals, and published a paper* in which he showed that their microscopical characters often serve to throw much light on the origin of rocks. Mr. Butler therefore placed the whole of his collection in Mr. Sorby's hands for careful examination, and it was decided that a paper should be written by the two conjointly; and since Mr. Sorby had previously made many experiments in connexion with the expansion of liquids, as already described in a paper published in the *Philosophical Magazine*†, he took advantage of the opportunity to investi-

* *Quarterly Journal of Geol. Soc.*, 1858, vol. xiv. p. 453.

† "On the Expansion of Water and Saline Solutions at High Temperatures," August 1859, vol. xviii. p. 81.

gate the law of the expansion of the very interesting fluid met with in the cavities of sapphire.

In describing the various facts, it will be well to consider them in relation to the following general principles:—

- (1) The structure of the various minerals as mere microscopical objects.
- (2) The physical characters of the fluid-cavities, as throwing light on the origin of the minerals.
- (3) The influence of some included crystals on the structure of the surrounding mineral.

Sapphires.

By far the most interesting objects contained in sapphires are the fluid-cavities. Their occasional presence has been already noticed by Brewster*, who met with one no less than about $\frac{1}{3}$ inch long, two-thirds full of a liquid which expanded so as to fill the whole cavity when heated to 82° F. (28° C.). He thought the liquid was less mobile than that described by him in topaz, and could not see a second liquid in the cavity. Though many thousand sapphires have been examined by the authors, no such large cavity has been found; but several have been met with about $\frac{1}{10}$ inch in diameter; the greater number are far less, and some are very minute; and they seem to contain only the liquid which expands so much when warmed. The size of the included bubble varies much, according to the temperature. At the ordinary heat of a room it is sometimes equal to one-half of the capacity of the cavity, whereas in other cases the cavity is quite full. This is especially the case with the very small cavities, and is to some extent due to the forced dilatation of the liquid. But if we only take into consideration the larger cavities, the temperature required to expand the fluid so as to fill them certainly varies from 20° to 32° C. (68° to 90° F.), and this not only in different crystals, but also, to a less extent, in the same specimen. As illustrations of the form of such cavities, we refer to Plate VII. figs. 1, 2, 3, and 4, the extent to which they are magnified being shown in each case. At the ordinary temperature the bubble in the cavity shown by fig. 1 is about one-half its diameter, but disappears entirely at 30° C. By carefully measuring the size of the cavity in various positions, and comparing it with the diameter of the bubble at 0° C., it appears that the liquid expands from 100 to 152 when heated from 0° to 30° C. Fig. 2 is a tubular cavity, and shows in a very excellent manner the boiling of the liquid when it cools after having been made to expand to fill the whole space. At the ordinary temperature the liquid occupies only about half the cavity; but when heated in a water-bath to 32° C., it fills it entirely. No bubble is formed until the temperature has fallen to 31°; and then innumerable small bubbles are suddenly formed, which rise to the upper part and unite; but instead of the liquid merely contracting by further cooling, it still continues to boil for some time, as represented in the drawing. Two other large cavities

* Söchting's *Einschlüsse von Mineralien in krystallisirten Mineralien*, p. 121, who refers to *Edin. Journ. of Sc.*, vol. vi. p. 115.

contained in the same specimen also behave in the same manner, and become full and suddenly boil at almost absolutely the same temperature, as that figured. We need scarcely say that such cavities are extremely rare, and are very remarkable even when merely looked upon as microscopical objects, independently of their interest in connexion with physics. Fig. 3 is a tubular cavity of more irregular form, and is interesting on account of there being two plates of the sapphire projecting into the cavity so as to nearly divide it into three portions. At the ordinary temperature these partitions prevent the passage of the bubble from one part to the other; but by breathing on the object through a flexible tube, the slight increase of temperature expands the liquid so as to make the bubble small enough to pass into the next compartment; and a repetition of the process causes it to pass into that at the other end. Such plates projecting into the cavities are very common; and it is requisite to pay attention to this fact, since otherwise they might easily be mistaken for crystals of some other substance included in the cavity, which, if they ever occur, must be extremely rare, since no decided case has come under our notice.

In examining sections of sapphire cut in a plane more or less parallel to the principal axis of the crystal, the double refraction is so strong that two images of every object lying at any depth below the surface are seen, in such a manner as to make them very confused. This may be avoided by using polarized light without an analyzer, and arranging the plane of polarization so as to coincide with one of the axes of the crystal. High powers may then be used with perfect definition; and they show many small cavities, sometimes of most irregular forms, like fig. 4; and very often their sides are so inclined that they totally reflect transmitted light, and appear black and opaque. In some specimens most of the cavities have lost their fluid.

Besides fluid-cavities, there are many small crystals of other minerals included in sapphires, but not so many as in rubies. The most striking are small plate-like crystals, often of triangular form, with one angle very acute. They are very thin, and give the colours of thin plates; so that when viewed by reflected light they look something like the scales from a butterfly. Seen edgewise, they appear as mere black lines, and are arranged parallel to the three principal planes of the sapphire, as shown by fig. 5. These small crystals and the minute fluid-cavities cause many sapphires to appear milky by reflected, and somewhat brown by transmitted light; and being arranged in zones related to the form of the crystal, they often show, as it were, lines of growth.

Rubies.

Though the ruby and the sapphire are of course essentially the same mineral, yet their structure is in many respects as characteristically different as their colour. The number of the fluid-cavities in rubies is far less, and the larger cavities are very rare, and only contain what appears to be water or a saline aqueous solution, as is shown by the amount of expansion when

the specimen is heated to the temperature of boiling water. Those containing a similar fluid to that included in sapphires do occasionally occur; and when they are minute, they are extremely interesting, since they show the spontaneous movement of the bubbles to greater perfection than any mineral that has come under our notice. This is perhaps to some extent due to the nature of the liquid, which is more mobile than the saline aqueous solutions contained in the cavities of the quartz of granite and syenite. It is manifestly a molecular movement analogous to that seen in all matter when very minute particles are suspended in a liquid, so as to allow freedom of motion; and the rapidity of the movement is certainly dependent on the size of the particles. It is not seen to advantage if the diameter of the bubbles is more than $\frac{1}{10000}$ of an inch; but when it is about $\frac{1}{50000}$ they move to and fro in the most surprising manner, with such rapidity that the eye can scarcely follow them.

The number of small crystals of other minerals included in rubies is often very great. There must be at least four different kinds; but it would be difficult to determine what minerals they all are. Some are very well characterized octahedrons, variously modified; and, as shown by fig. 5, their planes are very generally arranged parallel to planes of the ruby, and to the small plate-like crystals already mentioned in describing sapphire. These octahedrons have no influence on polarized light, and in general form and character correspond so closely with spinel that it seems very probable that they are that mineral. For some time we thought they were angular fluid-cavities filled with liquid; but when cut across in the sections they are clearly seen to be solid, though less hard than ruby. Many of the other included crystals are of such very rounded forms that, if it were not for their action on polarized light, they might easily be mistaken for cavities filled with some fluid. Most of these rounded crystals are colourless; but some are of more or less dark orange-red colour, and are certainly not the same mineral as the colourless or the octahedral crystals; and in all probability the thin and flat are a fourth kind. Occasionally alternating plates of ruby with their axes in different positions gave rise to a beautiful series of coloured stripes when examined with polarized light.

Spinel.

The ruby spinels from Ceylon sometimes contain fluid-cavities which differ in a striking manner from those of any other mineral that has come under our notice. One of these is shown in fig. 7. They are to a great extent filled with a yellow substance, indicated by the shading, which seems to be either a solid or a very viscous liquid. It incloses transparent, sometimes well-defined cubic crystals, which have no action on polarized light; transparent, prismatic, or plate-like crystals, which strongly depolarize it; and black opaque crystals, either in larger pieces or mere grains. The rest of the cavity is in each case about one-third full of a colourless liquid, which seems to contract on the application of heat, because it passes entirely into

vapour, as occurred in some of the cavities in topaz described by Brewster. In this change it must expand about six hundred times less than when water passes into steam. Spinel also incloses crystals of several other minerals which we have not yet been able to identify.

Aquamarina.

The most striking peculiarity of this mineral is the occurrence of numbers of fluid-cavities containing two fluids and a vacuity, as shown by fig. 6.

Emerald.

Some of the specimens which we have examined are so full of fluid-cavities that they are only partially transparent. They differ entirely from those already described, and contain only one liquid, which does not sensibly expand when warmed. In all probability this is a strong saline aqueous solution, since the cavities also inclose cubic crystals, as shown by fig. 8, which dissolve on the application of heat, and recrystallize on cooling. On the whole, therefore, these cavities are very similar to those found in the quartz of some granites, and in some of the minerals found in blocks ejected from Vesuvius, as described in Mr. Sorby's paper on the microscopical structure of crystals, already referred to.

Diamond.

Few, if any, of the specimens of diamond that have come under our notice contain objects similar to those which, in the opinion of Göppert*, are evidence of its having been derived from vegetable remains, but we have been able to study to great advantage some facts which do not appear to have presented themselves to either Göppert or Brewster. We have examined twenty-one objects similar to the two described by Brewster, in his paper in the Transactions of the Geological Society†; and this has enabled us to clear up some of the difficulties to which he alludes, and has led us to propose a different explanation. He thought that the black specks, which were surrounded by a black cross when examined with polarized light, were minute cavities; but at the same time he admitted that they were so small that it was not possible to say whether they contained a fluid or were empty. Judging from what we have seen of such small examples, we consider it impossible to say whether they are cavities or inclosed crystals; but fortunately we have met with several of such a size and character that it was quite easy to see that they were crystals. Fig. 9 is a most excellent example of this fact. The form is clearly that of a crystal, and it depolarizes light very powerfully. Its refractive power must be very much less than that of diamond; for the inclined planes totally reflect the transmitted light, and thus look quite black, as shown in the figure. It is this circumstance which causes many smaller inclosed crystals to appear like mere black specks.

* "Ueber Einschlüsse im Diamant," *Natuurkundige Verhandelingen*, Haarlem, 1864.

† 2nd series, vol. iii. p. 455.

Brewster has shown that the irregular depolarizing action of diamond is analogous to that of an irregularly hardened gum; and this much interferes with the perfection of the black crosses seen round the inclosed crystals, and sometimes even neutralizes this action. Still, as a general rule, a black cross is seen; and, as described by Brewster, when examined by means of a plate of selenite which gives the blue of the first order, the tints of the sectors in the line of its principal axis are depressed in the same manner as when such a black cross is produced by the compression of glass—thus proving that the inclosed crystals have exerted a pressure on the surrounding diamond. We, however, do not imagine that the crystals have increased in size, but that probably they have prevented the uniform contraction of the diamond, which, as already mentioned, must have been very irregular, even where no such impediment was present. A few of the crystals inclosed in rubies give rise to similar black crosses, as shown by fig. 11; and we are informed by Professor Zirkel that his brother-in-law Professor Vogelsang has prepared a thin section of a specimen of partially devitrified glass, which also shows black crosses round the inclosed crystals.

Brewster suggested that this phenomenon in diamond was due to the elastic force of an inclosed gas or liquid, and compared it with what is seen in the case of some cavities in amber. We, however, find that the optical character of the crosses seen round the undoubted cavities in amber is the very reverse of that in the case of diamond, and cannot be explained by the mere mechanical action of an included elastic substance, but is similar to the change to a crystalline state which has occurred over the whole external surface, and on both sides of cracks passing from it inwards.

The optical properties, however, are not the only evidence of contraction round crystals inclosed in diamond; for actual cracks are often seen to proceed from them. These present the striped appearance shown in fig. 10, owing to more or less perfect total reflection from their waved surface. The same kind of phenomenon may be seen in sapphire, and still better in spinel, as shown by figs. 12 and 13. Sometimes there is a system of radiating cracks nearly in one plane, terminating in a transverse crack which surrounds the whole, as in fig. 12; and in other cases there are various complicated wavy cracks in different planes, as in fig. 13. There seems to be some connexion between this structure and the nature of the included minerals; for round some kinds it is very common, but round others very rare or quite absent; and it appears probable that it may be referred to unequal contraction in cooling from a high temperature; and, if so, the results would necessarily depend on a variety of circumstances. Now that attention has been directed to it, it will probably be found to be a very common peculiarity of certain classes of minerals, and serve to throw a good deal of light on their origin.

Crystals surrounded by radiating cracks on a much larger scale have

been observed by Mr. David Forbes*, and may, we think, be explained in a similar manner.

The crystals formed in blowpipe beads kept hot for some time over the lamp, also furnish good illustrations of these facts. Phosphate of zirconia is deposited in cubes from a borax bead to which much microcosmic salt has been added; and when examined with the microscope whilst cooling, cracks like those described in diamond and spinel are seen to be formed round many of the crystals, which are evidently due to the crystals contracting less than the surrounding material. On the contrary, the long prisms of borate of baryta deposited from solution in borax are seen to separate from the borax on cooling, and to be filled with transverse cracks, like those in schorl inclosed in quartz, which is clearly owing to their contracting more than the borax.

Fluid-cavities in general.

Before discussing the nature of fluid-cavities in connexion with the origin of the various minerals, we think it best to describe the remarkable properties of the liquid included in the sapphire, and to point out what it seems to be. Brewster, in his paper on the fluid-cavities in topaz†, says that the more expansible liquid contained in them expands one-fourth its size, when heated from 50° to 80° F, or thirty-one and a quarter times as much as water; and, as already stated, he found that the fluid in sapphire expands about one-half when heated to 82° F. Though this amount of expansion is very remarkable, yet, when the relative expansion at various temperatures is examined, it will be seen to be still more remarkable. Very fortunately the tubular cavity in sapphire, shown by fig. 2, is most admirably fitted for experiment. Mere inspection shows that its general diameter is very uniform; and that it is really so can be proved by causing the liquid to pass from one end to the other; for at 17½° C. the length of the column of liquid was $\frac{25}{100}$ of an inch, whether it was at the end A or B. The total effective length of the cavity is $\frac{50}{100}$.

The specimen inclosing this cavity was fastened to a piece of glass, and this was fixed in a beaker containing water, supported so that the cavity was in the focus of the microscope under a low power. The temperature was raised very slowly, and was maintained for some minutes at each particular degree at which it was thought desirable to measure the volume of the liquid; and this was usually repeated over and over again when the heat was both rising and falling, so as to obtain as accurate a result as possible. In making the measurements with the micrometer, care was taken to allow for the tapering ends of the cavity and the curved surface of the liquid. The results are given in degrees Centigrade. Though the expansion below 30° was very great, compared with that of any other known substances except liquid carbonic acid and nitrous oxide, when the

* Ed. New Phil. Journ. July 1857.

† Trans. Roy. Soc. Edin. 1824, vol. x. p. 1.

temperature rose above 30° it was so very extraordinary that it was not until after having performed the experiment over and over again that Mr. Sorby felt confidence in the results. This will not be thought surprising when we state that from 31° to 32° the apparent expansion of the liquid is no less than one-fourth of the bulk it occupies at 31° ; the length of the column increasing for that single degree from $\frac{40}{100}$ to $\frac{50}{100}$ inch. This is about 780 times as great as the expansion of water would be, and even 69 times as much as that of air and permanent gases. It was not possible to ascertain the amount of expansion above 32° C., because the cavity was quite filled at that temperature. If the expansion increase at the same increased rate, the liquid would soon occupy several times as much space; but it seems very probable that before then it would pass into the state of gas. At all events it appears as if this enormous rate of expansion indicated a close approach to a fresh physical condition. The following Table gives the results of the experiments; and it has been found, by drawing them as a curve, that their general relations indicate that there cannot be any serious error; but at the same time, considering all the circumstances, they must only be looked upon as tolerably good approximations to the truth.

Temperature.	Volume.
0° C.....	100
$17\frac{1}{2}$	109
20	113
25	122
28	130
29	139
30	150
31	174
32	217

The apparent expansion of the liquid is doubtless to some extent increased by the condensation of the gas, as the space occupied by it is diminished. When in the highly expanded condition this liquid appears to be remarkably elastic. Berthelot has shown, in his paper on forced dilatation*, that the force with which liquids adhere to the interior of a glass tube is sufficient to prevent their contraction to the normal volume, if they have been heated so as to expand and quite fill the tube, and then cooled to a temperature below that requisite to fill it. This fact must always be borne in mind in studying fluid-cavities, and explains why the bubbles, as it were, hesitate to return, and then make their appearance with a sudden start. Such a forced dilatation is very remarkable in the case described; for though it was requisite to raise the temperature to 32° C. to fill the cavity, no vacuity was formed until it fell to 31° ; and therefore it seems as if the force of cohesion were sufficient to stretch it to considerably

* *Annales de Chimie* sér. 3. t. xxx. p. 232.

more than its normal bulk, even perhaps to the extent of one-fifth or one-fourth. Moreover, in the case shown in fig. 1., the liquid expanded so as to fill the cavity at about $30^{\circ}\text{C}.$; and yet it can be heated up to 42° without bursting it, though, even if the expansion did not continue to increase, and were the same for each degree as from 31° to 32° , the normal volume would be about four times that of the cavity,—which in any case seems only to be explained by supposing that its elasticity is most remarkably great, more like that of a gas than of a liquid. There was no decided evidence of its passing into a gaseous state, as does occur when cavities contain a less amount of liquid.

Simmler * has shown that the physical properties of the liquid in topaz, as observed by Brewster, agree more nearly with those of liquid carbonic acid than with those of any other known substance. Dana, in his 'Mineralogy' (5th edition, 1868, p. 761), calls it *Brewsterlinite*, and says that its composition is unknown. The facts at Simmler's command were not in all respects satisfactory—since the amount of expansion given by Brewster was from 10° to $26^{\circ}\cdot7\text{C}.$, whereas that of liquid carbonic acid observed by Thilorier was from 0° to 30° , and, as shown above, the expansion increases so much as the temperature rises that the average rate for 1° is very indefinite. The only reliable method is therefore to compare the expansion between equal degrees of temperature. According to Thilorier† liquid carbonic acid, when heated from 0° to 30° , expands from 100 to 145. One of the experiments described above showed that the liquid in sapphire expands from 100 to 152; and the other from 100 to 150, which is the most reliable. This agrees so closely with the expansion of liquid carbonic acid, that the difference might easily be due to a slight error in the thermometers. The expansion of ordinary liquids is not to be compared with it, nor is that of liquid sulphurous acid. Dr. Frankland has kindly ascertained this fact, with special reference to the case in question, and found that from 0° to $32^{\circ}\text{C}.$ the expansion was only from 100 to 104·36 instead of to 217.

According to Andréeff‡ the expansion of liquid nitrous oxide is not much inferior to that of liquid carbonic acid, being, from 15° to 20° , ·00872 for each degree, which differs decidedly from that of the liquid in sapphires. The occurrence of nitrous oxide in minerals is also so very much more improbable, that, on the whole, it seems as if we should be justified in concluding provisionally that it is liquid carbonic acid, which, like water, should therefore be classed amongst natural liquid mineral substances.

Brewster has shown§ that when cavities in topaz contain less than one-third of their volume of the expansible liquid, it does not expand when heated, but passes entirely into the state of a compressed vapour. Un-

* Pogg. Ann. vol. cv. p. 460.

† Gmelin's Handbook of Chemistry, Cavendish Society's Translation, vol. i. p. 225.

‡ Liebig's Ann. vol. cx. p. 1.

§ Trans Roy. Soc. Edin. vol. x. p. 25.

fortunately he does not state the temperature at which this occurs, nor does he seem to have tried to ascertain the exact limit of the volume, which must, however, lie between one-half and one-third. Cagniard-Latour* found that when ether and other liquids sealed up in small strong tubes, with a certain space left empty, were heated, they expanded very much, and suddenly passed into the state of vapour. The temperature, pressure, and volume at which this change took place varied very considerably. Ether expanded to nearly double its volume, and passed into vapour at about 200° C., with an elastic force of 37 or 38 atmospheres. Alcohol expanded to about three times its volume, and passed into vapour at about 260° C., with an elastic force of 119 atmospheres; whereas water appeared to expand to nearly four times its volume, and required a temperature near that at which zinc melts (328° C., Daniel). When in this highly expanded state, the liquids were very mobile, and seemed much more compressible than under other circumstances; for they did not burst the tube, if too much had been sealed up in it, until after their normal volume would have been decidedly greater than its capacity. No one could fail to see that these phenomena have much in common with what occurs at a lower temperature in the case of the liquid inclosed in sapphire, and that they are of great importance in connexion with the origin of fluid-cavities. Since they become full of liquid at a comparatively low temperature, it was not unreasonable to suppose that the minerals in which they occur must have been formed where the heat was scarcely above that of the atmosphere; but these facts seem to show that the occurrence of such fluid-cavities is quite reconcilable with a very high temperature; for it is obvious that if, at a great depth below the surface, heated, highly compressed *gaseous* carbonic acid were inclosed in growing crystals, it might condense on cooling so as to more or less completely fill the cavities with the *liquid* acid.

If the same principles could be applied in the case of water, we should be led to infer that it could not exist in a liquid state at a higher temperature than that of dull redness, corresponding closely with what Mr. Sorby deduced from the fluid-cavities in some volcanic rocks. In that case, according to Cagniard-Latour, the liquid when condensed would occupy only one-fourth part of the cavity, and it would scarcely be likely to contain any fixed salt in solution; whereas the fluid-cavities in the minerals of ejected blocks are often two-thirds full of what seems to have been a supersaturated solution of alkaline chlorides. The phenomena now under consideration should certainly be borne in mind in studying volcanic action; and it is possible that some cavities now containing water may have been formed by the inclosure of very highly compressed steam. In some cases the requisite pressure would be enormous, and other facts seem to show that it was more generally caught up in a liquid state.

The cavities in emerald are very interesting in connexion with this subject, and also furnish strong evidence against the opinion that the liquid was not

* Ann. de Chimie, 1822, t. xxi. pp. 127 & 178; t. xxii. p. 410.

present when the crystals were formed, but penetrated into the fluid-cavities at a subsequent period, and either filled vacant spaces, or removed and replaced the material of glass cavities, as suggested by Vogelsang *. In the specimens which we have examined, each of the cavities contains what is no doubt an aqueous saline solution, and, as shown by fig. 8, one or more cubic crystals, probably chloride of potassium, which dissolve on the application of heat, and are deposited again on cooling. These cavities are thus analogous to those met with in the quartz of some granite, and in the minerals of blocks ejected from Vesuvius; and it seems difficult, if not impossible, to explain them except by supposing that a strong saline solution was caught up by the mineral at the time of its formation. In some cases the amount of such saline matter is so great in comparison to the liquid, that a high temperature would be requisite to make it all dissolve. It also seems probable that, if water could penetrate into such crystals, it would soon be lost when they were kept dry. This certainly occurs in some soluble salts, especially those containing combined water, and in some minerals of loose texture; but we have never seen evidence of it when fluid-cavities are completely inclosed in hard and dense substances like quartz or emerald. Though in some instances the size of the bubbles does not bear a uniform relation to that of the cavities, yet in many cases the general proportion is very similar in each specimen; and the exceptions can easily be explained by supposing that occasionally small bubbles of gas were caught up along with the water, or that there was some variation in either temperature or pressure during the growth of the crystal; all of which conditions were discussed in Mr. Sorby's paper already referred to.

We have not had the opportunity of studying many examples of cavities which contain two fluids, probably water and liquid carbonic acid, and therefore forbear to say much about them. According to Brewster† the temperature at which those in topaz become full corresponds very closely with what we have observed in the case of sapphire, so that the carbonic acid might have been inclosed either as a highly dilated liquid, or as a highly compressed gas; but since the other liquid has deposited crystals which dissolve on the application of heat‡, it seems most probable that the water was caught up in a liquid state, sometimes perhaps holding a considerable amount of carbonic acid in solution as a gas.

On the whole, therefore, the various facts described in this paper seem to show that ruby, sapphire, spinel, and emerald were formed at a moderately high temperature, under so great a pressure that water might be present in a liquid state. The whole structure of diamond is so peculiar that it can scarcely be looked upon as positive evidence of a high temperature, though not at all opposed to that supposition. The absence of fluid-cavities containing water or a saline solution does not by any means prove that water

* Philosophie der Geologie und mikroskopische Gesteinsstudien, (Bonn, 1867) pp. 155, 196.

† Trans. Roy. Soc. Edin. vol. x. p. 1 *et seq.*

‡ See Brewster's paper, Phil. Mag. 1847, vol. xxxi. p. 497.

was entirely absent, because the fact of its becoming inclosed in crystals depends so much on their nature. At the same time the occurrence of fluid-cavities containing what seems to be merely liquid carbonic acid is scarcely reconcilable with the presence of more than a very little water in either a liquid or gaseous form. We may here say that we do not agree with those authors who maintain that the curved or irregular form of the fluid-cavities is proof of the minerals having been in a soft state, since analogous facts are seen in the case of crystals deposited from solution.

EXPLANATION OF PLATE VII.

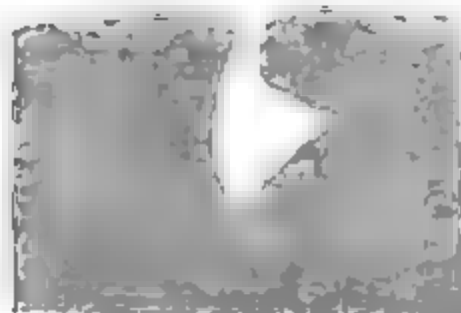
- Figs 1. & 2. Fluid-cavities in sapphires ; magnified 20 linear.
 Fig. 3. Fluid-cavity in sapphire, partially divided by plates of sapphire ; mag. 50.
 Fig. 4. Branched fluid-cavity in sapphire ; mag. 50.
 Fig. 5. Crystal of spinel ? inclosed in ruby ; mag. 50.
 Fig. 6. Cavity in aquamarina, with two fluids ; mag. 150.
 Fig. 7. Cavity in ruby spinel ; mag. 100.
 Fig. 8. Fluid-cavity in emerald, with soluble crystals ; mag. 200.
 Fig. 9. Crystal inclosed in diamond, surrounded by a black cross, as seen with polarized light ; mag. 100.
 Fig. 10. Crystal inclosed in diamond, with a crack proceeding from it ; mag. 100.
 Fig. 11. Crystal inclosed in ruby, surrounded by a black cross, seen by polarized light ; mag. 75.
 Figs. 12 & 13. Crystals in ruby spinel, surrounded by various cracks ; mag. 50.

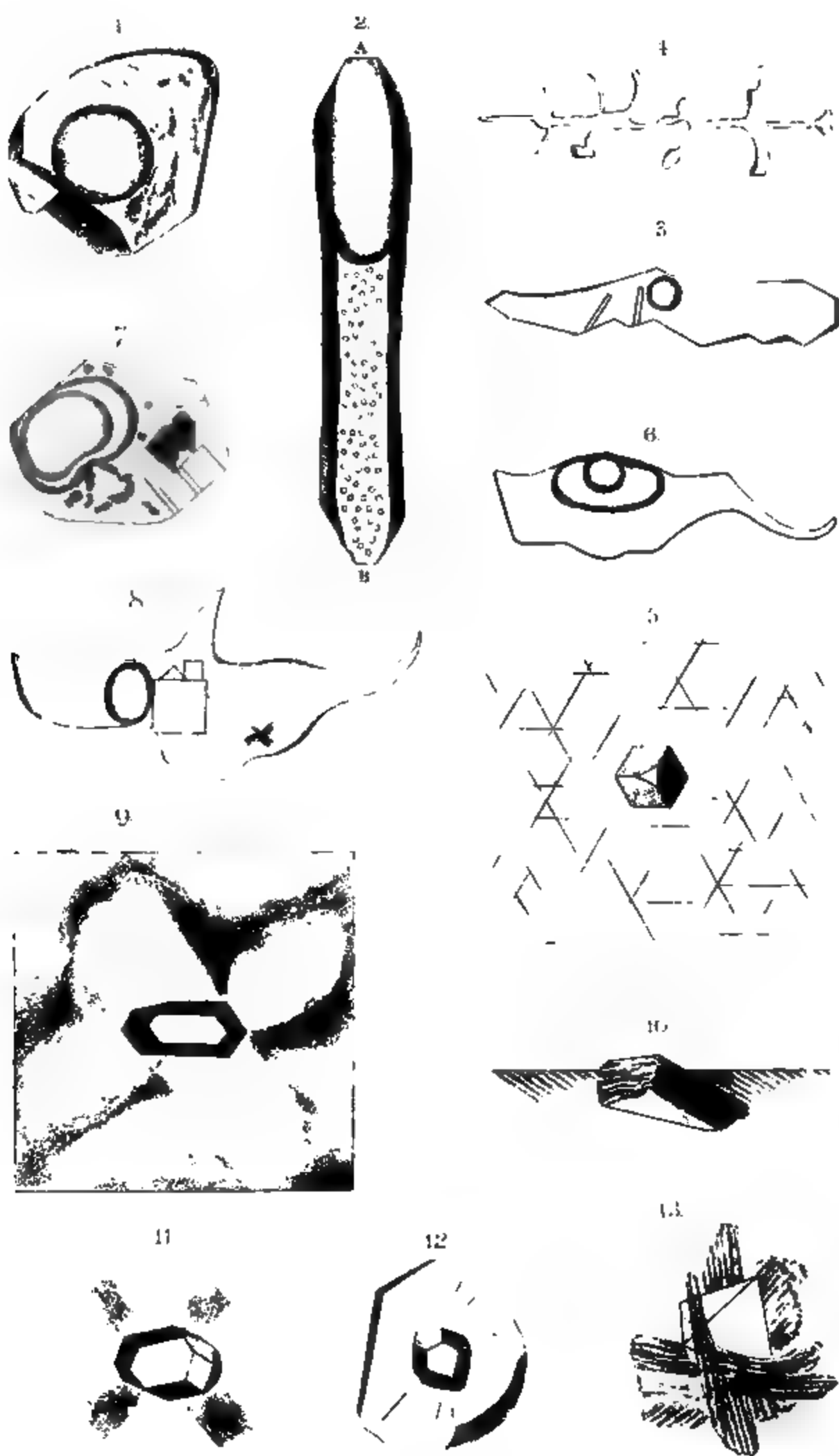
II. "Note on a Method of viewing the Solar Prominences without an Eclipse." By WILLIAM HUGGINS, F.R.S. Received February 16, 1869.

Last Saturday, February 13, I succeeded in seeing a solar prominence so as to distinguish its form. A spectroscope was used ; a narrow slit was inserted after the train of prisms before the object-glass of the little telescope. This slit limited the light entering the telescope to that of the refrangibility of the part of the spectrum immediately about the bright line coincident with C.

The slit of the spectroscope was then widened sufficiently to admit the form of the prominence to be seen. The spectrum then became so impure that the prominence could not be distinguished.

A great part of the light of the refrangibilities removed far from that of C was then absorbed by a piece of deep ruby glass. The prominence was then distinctly perceived, something of this form.







A more detailed account is not now given, as I think I shall be able to modify the method so as to make the outline of these objects more easily visible.

February 25, 1869.

Capt. RICHARDS, R.N., Vice-President, in the Chair.

The following communications were read :—

- I. "Additional Observations of Southern Nebulæ." In a Letter to Professor STOKES, Sec. R.S., by Lieut. J. HERSCHEL, R.E. Communicated by Prof. STOKES. Received January 4, 1869.

Bangalore, Dec. 1, 1868.

DEAR SIR,—I have the pleasure to subjoin a few additions to my former list of Southern Nebulæ spectroscopically examined.

The observations extend from the 24th October to the 20th November.

I will first enumerate those of which no trace of a spectrum of any kind has been detected, and which, I can with confidence state, have no other than a continuous spectrum. I am enabled to do this for the following reason—that even when the jaws of the slit were entirely removed, so as to command a perfectly free field of view (in which stellar spectra were frequently recognized), no light from these objects was visible. That no doubt might remain as to the justice of this conclusion, three faint planetary nebulæ were looked at in the same way, and were more or less easily recognized as spots of light in the spectroscopic field. It is much to be regretted that I did not long ago make the experiment; had I done so I should unquestionably have saved myself many tedious hours lost in vain searching. The following may safely be erased from a list of nebulæ to be examined for evidence of a "linear" character :—

Nos. 4757. "Very bright; pretty small."

27. "Very bright; very large."

67. "Very bright; large."

*162. "Globular cluster; bright; large."

163. "Very bright; large."

339. "Bright; large."

*342. "Very bright; pretty large."

361. "Very bright; very large."

369. "Bright; pretty large."

†544. "Very bright; very large."

604. "Very bright; pretty large."

†610. "Very bright; large."

* These were not looked at in the way described above (without a slit), but are nevertheless included, because it is certain they have no visible spectrum.

† The same remark applies to these; they were *twice* examined.

The brackets denote that the objects are so near each other that one observation sufficed for both.

304 Lieut. Herschel's *Observations of Southern Nebulae*. [Feb. 25,

709. "Very bright; small."
 721. "Bright; large."
 731. "Very remarkable; very bright; very large; barely resolvable."
 741. } "Globular cluster; bright; pretty large."
 744. } "Globular cluster; very bright; pretty large."
 746. "Bright; pretty small."
 748. } "Globular cluster; very bright; pretty large; partially resolved."
 750. } "Very bright; pretty large."
 747. } "Considerably bright; pretty small."
 752. } "Very bright; large."
 755. "Bright; pretty small."
 767. "Very bright; large."
 808. "Globular cluster; bright; considerably large; partially resolved."
 823. } "Bright; very large."
 822. } "Pretty bright; pretty large."
 828. "Very bright; pretty small."
 1009. "Very bright; very large; partially resolved."
 1288. "Globular cluster; bright; pretty large."

The following were not spectroscopically examined, only because they appeared too faint in the telescope :—

- Nos. 279. "Bright; small."
 811. "Bright; large."
 813. } "Bright; large."
 815. } "Pretty bright; round."
 824. "Very bright; very large."
 916. "Very bright; large."

The following were "not identified" :—

- Nos. 73. "Very bright; small." [Not seen on three different nights, with clear sky.]
 243. "Bright; small." Several small indistinct objects in the field.
 273. "Very bright; small." Not found: two independent settings gave the same field.
 411. "Bright; small." [No remark.]
 670. "Very bright; round." No such object. No. 685 precedes by $2\frac{1}{2}$ min., and has the same N.P.D.; it appears as described, very bright in the middle, and has a small star involved.
 769. "Globular cluster; very bright." Not found on two nights.

Checked *AR* by No. 767; "very bright; large." In searching for it, found a nebula which agreed well with No. 766, "pretty faint; small."

1401. "Very bright; small." Not recognized; twice.

I come now to those of which the spectrum has been recognized.

The following have *continuous* spectra:—

- Nos. 138. "Very remarkable; extremely bright; extremely large." A fine object, and certainly very bright; but the spectrum was recognized with great difficulty (through the slit).
600. "Very bright; pretty large; partially resolved." Spectrum continuous.
685. "Globular cluster; very bright; pretty large; round; easily resolvable." Spectrum continuous—readily.
697. } "Very bright; considerably large." Spectrum continuous
—without difficulty.
698. } "Pretty bright; pretty small." ?
715. "Very bright; pretty small." Spectrum continuous—barely visible.
748. "Globular cluster; very bright; pretty large; round; partially resolved." Spectrum continuous.
1061. "Globular cluster; remarkable; very bright; very large; round; well resolved." Spectrum continuous—bright.
1076. "Very bright; large; round; barely resolvable." Spectrum continuous.
4687. "Remarkable; globular cluster; bright; large; stars." Spectrum continuous—bright, almost stellar in middle.

Lastly, I am able to report that one globular cluster proves to be of the same character as the "planetary" nebulae, viz.:—

No. 826. "Globular cluster; very bright; small; round; barely resolvable (IV. 26)." Spectrum "linear." This object shows one principal, one secondary, and one very faint line in the usual places. It also shows an undoubted continuous spectrum, principally (but not only) on the more refrangible side. This is visible even when the slit is very narrow. The following measurements were taken—that of *D* by a spirit flame before the object-glass:—

$$\left. \begin{array}{l} \text{Prin. line} = 5.17 \text{ (D} = 3.02) \\ \text{,,} \quad \quad = 5.19 \text{ (D} = 3.02) \end{array} \right\} \text{ or } D + 2.16.$$

No. 1225. "A planetary nebula; pretty bright; very small; very little extended; barely resolvable"? No spectrum of this planetary nebula had been obtained in April. It was now recognized instantly, and without the smallest doubt, as "linear," or at least apparently monochromatic, in the *open* field of the

spectroscope. The position of the line has not been measured.

No. 1565. "A planetary nebula; pretty bright; pretty small; extremely little extended; barely resolvable." The "linear" character of the spectrum of this object has been already recognized; but it was again examined, as a test of the advantage of removing the slit. It is a considerably larger and less bright object than No. 1225, and situated in a magnificent cluster of stellar points. In the open field of the spectroscope it appeared as a similar faint patch of light, in the midst of an infinity of streaks. Nothing could have been more conclusive as a test.

No. 1185. "Remarkable; very bright; very large; round; with tail; much brighter in middle, a star of 8.9 magnitude." A neighbour of the great nebula of Orion. Examined *with* the slit repeatedly. On the first occasion the spectrum was described as "linear, but faintly seen; not certainly seen in presence of the central star. When the latter is put out, the spectrum becomes broadly continuous, with monochromatic light across it." On the next it was, "To-night I could trace no lines. There is ample light, but it is not 'linear,' though certainly confined principally to the neighbourhood of the position of the ordinary lines. The spectrum in any case occupies nearly the width of the field, and is not much less in length." On a third occasion I noted that I could "barely trace any lines, while there is a broad patch of spectral light on either side, which is certainly not due to the stellar centre." The trace of lines is confirmed on a fourth occasion.

I think I am justified in saying that we have here a nebula of a class or description intermediate between those which show a clear continuous spectrum only, and those which show bright lines only. Not that the apparent character of these two extremes is necessarily absolute; it is far more probable that the non-appearance in either, of the distinguishing characteristic of the other, is relative only. Indeed there are not wanting instances of nebulae whose place in a series would be short of the latter extreme. For instance,—

No. 826, *supra*, and

No. 4964. "Extremely remarkable; a planetary nebula; very bright; pretty small; round; blue." Presented "a continuous spectrum and a fourth line (besides the three usual ones); the first strongly suspected, the last less so." The fourth line I find has been noted by Mr. Huggins.

And the great nebula of Orion appears to be of the same order. I have examined this nebula repeatedly of late, because on the first occasion of

looking at No. 1185 I had appended a remark that "the principal nebula shows a great deal of continuous light on this side," an observation which seemed to require confirmation. The following extracts from my notebook must speak for themselves:—

No. 1179. The great Nebula of Orion. Oct. 25. "A fourth line, almost beyond question: measured twice with reference to principal line, $7.3 - 5.05 = 2.25$, $7.6 - 5.06 = 2.54$, mean 2.4 ; continuous spectrum suspected, but, owing to moonlight and low altitude, there was no conviction." Nov. 7. "Previous observation confirmed. The fourth line is a fact. The diffused light, which also is certainly visible, to the extent of rendering the edges of the field visible beyond the immediate neighbourhood of the lines, can only be a continuous spectrum." Nov. 9. "Fourth line, $7.9 - 5.1 = 2.8$, very rough. I am satisfied that there is a continuous spectrum, though I am not *certain* it may not be dispersed stellar light." Ditto, later: "I have no longer any hesitation as to the continuous spectrum." Nov. 10. "Fourth line, 7.92 , 7.88 , 7.91 , $-5.09 = 2.81^*$. Continuous spectrum distinctly ending coincidently with the bright lines at the edge of the bright part of the nebula."

There is nothing very remarkable in the presence either of an additional line or of a continuous spectrum; but as this nebula has been examined very carefully in England without the detection of either†, it appeared necessary to put both beyond question; taken, too, in connexion with the very different character of its near neighbour, 1185, and with others in which the relative intensity of the two kinds of spectra varies in degree, it appears to break down, to a considerable extent, the barrier between "gaseous" and "solid" nebulous matter, and to lead towards the inference that condensation is in a more or less advanced stage in all nebulae, and in the vast majority of cases, including all "clusters," has become complete.

I am sorry to say that I shall be unable to prosecute these observations for some months, as my survey duties require my presence elsewhere. In the meanwhile I should be glad to learn whether the course I have been pursuing appears a desirable one to continue, now that so large a number of the southern nebulae have been tested, or whether a reexamination would be preferred.

I remain, dear Sir, yours truly,

J. HERSCHEL.

* Adopting $D + 2.19$ as the position of the principal line

gives $D + 5.00$ " " fourth line.

Kirchhoff's $272.1 = D + 4.80$ } ; hence the fourth line is about midway between these.
 " $285.5 = D + 5.25$ }

† [A faint continuous spectrum appears to have been seen with Lord Rosse's great telescope. See a paper by Lord Oxmantown "On the Great Nebula in Orion," *Philosophical Transactions* for 1868, p. 72.—G. G. S.]

II. "Note on the Separation of the Isomeric Amylic Alcohols formed by Fermentation." By ERNEST T. CHAPMAN and MILES H. SMITH. Communicated by Prof. E. W. BRAYLEY. Received January 14, 1869.

At present we are acquainted with two amylic alcohols formed by fermentation. They were discovered by Pasteur, who observed that different specimens of amylic alcohol caused a ray of polarized light to rotate to different degrees. He succeeded in devising a separation of these alcohols, which consisted in converting them into sulphamylates of barium and recrystallizing these salts. The one alcohol is without action on polarized light, and the other rotates it. This method of separation is beset with great practical difficulties, and has, we believe, only once been repeated, viz. by Mr. Pedler. He gives no detailed account of the separation, but gives some of the leading properties of the alcohols. He found that the rotating alcohol caused a ray of polarized light to rotate 17° with a column of 500 millims. of liquid.

The following are some examples of the rotations effected by eleven different samples of amylic alcohol in a column of 385 millims. For comparison with Pedler's number, the observed numbers have been reduced in the second column to observations on 500 millims.:—

Designation of specimen.	Rotation observed on column of 385 millims.	Reduced to observations on 500 millims.
1.	3.5	4.55
2.	3.7	4.81
3.	4	5.2
4.	3.7	4.81
5.	4.7	6.11
6.	4	5.2
7.	3.5	4.55
8.	2.7	3.51
9.	5	6.5
10.	4	5.2
11.	3.8	4.94
Pedler's rotating alcohol.....		17.0

If Pedler's number be absolutely correct, it follows that these specimens of amylic alcohol contained from 15.9 per cent. as a minimum, to 38.2 as a maximum of the rotating alcohol. The boiling-points of the whole of the samples lay between $131^{\circ}.5$ and 133° .

We have effected the separation of these alcohols more simply. If soda, potash, chloride of calcium, or, apparently, any salt easily soluble in amylic alcohol be dissolved in that alcohol at the boiling-point, and the saturated solution be distilled, the non-rotating alcohol will be to a great extent retained and the rotating alcohol distils off. The substance which appears to lend itself most conveniently to this operation is caustic soda.

Amylic alcohol is boiled with excess of caustic soda; when saturated,

the hot solution is decanted into a flask and distilled from an oil-bath, the temperature of which may be allowed to rise to 200° . The alcohol distils off at first readily, after a while with greater difficulty; finally the contents of the distilling flask solidify, and it becomes extremely difficult to drive over any more amylic alcohol. On now adding water to the contents of the flask and again distilling, amylic alcohol comes over of about half the rotating power of the alcohol employed. If the power of rotation be very small, the reduction is considerably greater; thus, operating on an alcohol rotating $1^{\circ}3$ on the 385 millims., by one operation we have reduced it to $0\cdot3$. By a sufficient number of repetitions of the process, it is possible to effect a separation of the alcohols, and very easy to obtain considerable quantities of the non-rotating alcohol quite pure. No valerianic acid is formed; and the soda-solution remaining in the flask after the operation is completed is barely coloured.

The separation of the alcohols may also be effected by dissolving metallic sodium in amylic alcohol, and distilling, &c., as above described, the resulting solution of amylate of soda in amylic alcohol. The process appears to present no point of advantage over that with caustic soda.

We shall shortly publish a detailed account of differences in structure of these alcohols, together with a description of some of their principal derivatives.

III. "Note on the Heat of the Stars." By WILLIAM HUGGINS, F.R.S. Received February 18, 1869.

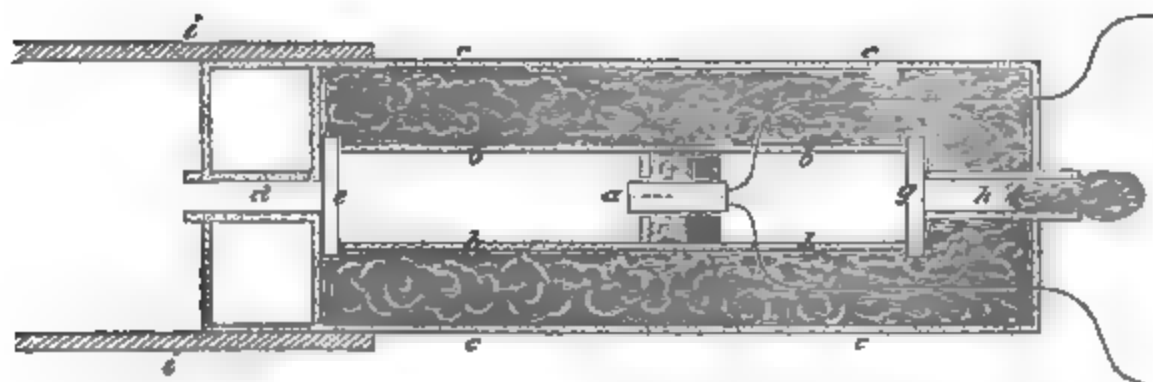
In the summer of 1866 it occurred to me that the heat received on the earth from the stars might possibly be more easily detected than the solar heat reflected from the moon. Mr. Becker (of Messrs. Elliott Brothers) prepared for me several thermopiles, and a very sensitive galvanometer. Towards the close of that year, and during the early part of 1867, I made numerous observations on the moon, and on three or four fixed stars. I succeeded in obtaining trustworthy indications of stellar heat in the case of the stars Sirius, Pollux, and Regulus, though I was not able to make any quantitative estimate of their calorific power.

I had the intention of making these observations more complete, and of extending them to other stars. I have refrained hitherto from making them known; I find, however, that I cannot hope to take up these researches again for some months, and therefore venture to submit the observations in their present incomplete form.

An astatic galvanometer was used, over the upper needle of which a small concave mirror was fixed, by which the image of the flame of a lamp could be thrown upon a scale placed at some distance. Usually, however, I preferred to observe the needle directly by means of a lens so placed that the divisions on the card were magnified, and could be read by the observer when at a little distance from the instrument. The sensitiveness of the

instrument was made as great as possible by a very careful adjustment from time to time of the magnetic power of the needles. The extreme delicacy of the instrument was found to be more permanently preserved when the needles were placed at right angles to the magnetic meridian during the time that the instrument was not in use. The great sensitiveness of this instrument was shown by the needles turning through 90° when two pieces of wire of different kinds of copper were held between the finger and thumb. For the stars, the images of which in the telescope are points of light, the thermopiles consisted of one or of two pairs of elements; a large pile, containing twenty-four pairs of elements, was also used for the moon. A few of the later observations were made with a pile of which the elements consist of alloys of bismuth and antimony.

The thermopile was attached to a refractor of eight inches aperture. I considered that though some of the heat-rays would not be transmitted by the glass, yet the more uniform temperature of the air within the telescope, and some other circumstances, would make the difficulty of preserving the pile from extraneous influences less formidable than if a reflector were used.



The pile *a* was placed within a tube of cardboard, *b*; this was enclosed in a much larger tube formed of sheets of brown paper pasted over each other, *c*. The space between the two tubes was filled with cotton-wool. At about 5 inches in front of the surface of the pile, a glass plate (*e*) was placed for the purpose of intercepting any heat that might be radiated from the inside of the telescope. This glass plate was protected by a double tube of cardboard, the inner one of which (*d*) was about half an inch in diameter. The back of the pile was protected in a similar way by a glass plate (*g*). The small inner tube (*h*) beyond the plate was kept plugged with cotton-wool; this plug was removed when it was required to warm the back of the pile, which was done by allowing the heat radiated from a candle-flame to pass through the tube to the pile. The apparatus was kept at a distance of about 2 inches from the brass tube by which it was attached to the telescope by three pieces of wood (*i*), for the purpose of cutting off as much as possible any connexion by conduction with the tube of the telescope.

The wires connecting the pile with the galvanometer, which had to be

placed at some distance to preserve it from the influence of the ironwork of the telescope, were covered with gutta percha, over which cotton-wool was placed, and the whole wrapped round with strips of brown paper. The binding-screws of the galvanometer were enclosed in a small cylinder of sheet gutta percha, and filled with cotton-wool. These precautions were necessary, as the approach of the hand to one of the binding-screws, or even the impact upon it of the cooler air entering the observatory, was sufficient to produce a deviation of the needle greater than was to be expected from the stars.

The apparatus was fixed to the telescope so that the surface of the thermopile would be at the focal point of the object-glass. The apparatus was allowed to remain attached to the telescope for hours, or sometimes for days, the wires being in connexion with the galvanometer, until the heat had become uniformly distributed within the apparatus containing the pile, and the needle remained at zero, or was steadily deflected to the extent of a degree or two from zero.

When observations were to be made, the shutter of the dome was opened, and the telescope, by means of the finder, was directed to a part of the sky near the star to be examined where there were no bright stars. In this state of things the needle was watched, and if in four or five minutes no deviation of the needle had taken place, then by means of the finder the telescope was moved the small distance necessary to bring the image of the star exactly upon the face of the pile, which could be ascertained by the position of the star as seen in the finder. The image of the star was kept upon the small pile by means of the clock-motion attached to the telescope. The needle was then watched during five minutes or longer; almost always the needle began to move as soon as the image of the star fell upon it. The telescope was then moved, so as to direct it again to the sky near the star. Generally in one or two minutes the needle began to return towards its original position.

In a similar manner twelve to twenty observations of the same star were made. These observations were repeated on other nights.

The mean of a number of observations of Sirius, which did not differ greatly from each other, gives a deflection of the needle of 2° .

The observations of Pollux $1\frac{1}{2}^{\circ}$.

No effect was produced on the needle by Castor.

Regulus gave a deflection of 3° .

In one observation Arcturus deflected the needle 3° in 15 minutes.

The observations of the full moon were not accordant. On one night a sensible effect was shown by the needle; but at another time the indications of heat were excessively small, and not sufficiently uniform to be trustworthy.

It should be stated that several times anomalous indications were observed, which were not traced to the disturbing cause.

The results are not strictly comparable, as it is not certain that the

sensitiveness of the galvanometer was exactly the same in all the observations, still it was probably not greatly different.

Observations of the heat of the stars, if strictly comparable, might be of value, in connexion with the spectra of their light, to help us to determine the condition of the matter from which the light was emitted in different stars.

I hope at a future time to resume this inquiry with a larger telescope, and to obtain some approximate value of the quantity of heat received at the earth from the brighter stars.

IV. "On the Fracture of Brittle and Viscous Solids by 'Shearing.'"

By Sir W. THOMSON, F.R.S. Received January 2, 1869.

On recently visiting Mr. Kirkaldy's testing works, the Grove, Southwark, I was much struck with the appearances presented by some specimens of iron and steel round bars which had been broken by torsion. Some of them were broken right across, as nearly as may be in a plane perpendicular to the axis of the bar. On examining these I perceived that they had all yielded through a great degree to distortion before having broken. I therefore looked for bars of hardened steel which had been tested similarly, and found many beautiful specimens in Mr. Kirkaldy's museum. These, without exception, showed complicated surfaces of fracture, which were such as to demonstrate, as part of the whole effect in each case, a spiral fissure round the circumference of the cylinder at an angle of about 45° to the length. This is just what is to be expected when we consider that if $A B D C$ (fig. 1) represent an infinitesimal square on the surface of a round bar with its sides $A C$ and $B D$ parallel to the axis of the cylinder, before torsion, and $A B D' C'$ the figure into which this square becomes distorted just before rupture, the diagonal $A D$ has become elongated to the length $A D'$, and the diagonal $B C$ has become contracted to the length $B C'$, and that therefore there must be maximum tension every-

Fig. 1.

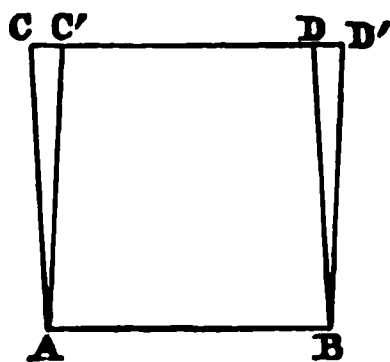
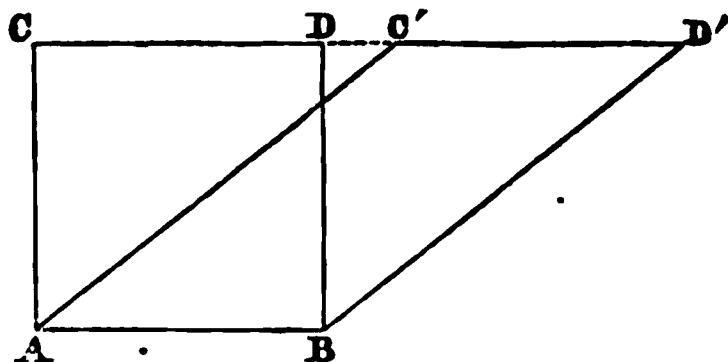


Fig. 2.



where, across the spiral of which $B C'$ is an infinitely short portion. But the specimens are remarkable as showing in softer or more viscous solids a tendency to break parallel to the surfaces of "shearing" $A B$, $C D$, rather than in surfaces inclined to these at an angle of 45° . Through the kindness of Mr. Kirkaldy, his specimens of both kinds are now exhibited

to the Royal Society. On a smaller scale I have made experiments on round bars of brittle sealing-wax, hardened steel, similar steel tempered to various degrees of softness, brass, copper, lead.

Sealing-wax and hard steel bars exhibited the spiral fracture. All the other bars, without exception, broke as Mr. Kirkaldy's soft steel bars, right across, in a plane perpendicular to the axis of the bar. These experiments were conducted by Mr. Walter Deed and Mr. Adam Logan in the Physical Laboratory of the University of Glasgow; and specimens of the bars exhibiting the two kinds of fracture are sent to the Royal Society along with this statement. I also send photographs exhibiting the spiral fracture of a hard steel cylinder, and the "shearing" fracture of a lead cylinder by torsion.

These experiments demonstrate that continued "shearing" parallel to one set of planes, of a viscous solid, develops in it a tendency to break more easily parallel to these planes than in other directions, or that a viscous solid, at first isotropic, acquires "cleavage-planes" parallel to the planes of shearing. Thus, if CD and AB (fig. 2) represent in section two sides of a cube of a viscous solid, and if, by "shearing" parallel to these planes, CD be brought to the position $C'D'$, relatively to AB supposed to remain at rest, and if this process be continued until the material breaks, it breaks parallel to AB and $C'D'$.

The appearances presented by the specimens in Mr. Kirkaldy's museum attracted my attention by their bearing on an old controversy regarding Forbes's theory of glaciers. Forbes had maintained that the continued shearing motion which his observations had proved in glaciers, must tend to tear them by fissures parallel to the surfaces of "shearing." The correctness of this view for a viscous solid mass, such as snow becoming kneaded into a glacier, or the substance of a formed glacier as it works its way down a valley, or a mass of *débris* of glacier-ice, reforming as a glacier after disintegration by an obstacle, seems strongly confirmed by the experiments on the softer metals described above. Hopkins had argued against this view, that, according to the theory of elastic solids, as stated above, and represented by the first diagram, the fracture ought to be at an angle of 45° to the surfaces of "shearing." There can be no doubt of the truth of Hopkins's principle *for an isotropic elastic solid, so brittle as to break by shearing before it has become distorted through more than a very small angle*; and it is illustrated in the experiments on brittle sealing-wax and hardened steel which I have described. The various specimens of fractured elastic solids now exhibited to the Society may be looked upon with some interest, if only as illustrating the correctness of each of the two seemingly discrepant propositions of those two distinguished men.

V. Note by Professor CAYLEY on his Memoir "On the Conditions for the Existence of Three Equal Roots, or of Two Pairs of Equal Roots, of a Binary Quartic or Quintic." Received February 20, 1869.

The title is a misnomer ; I have in fact, in regard to the quintic, considered *not* (as according to the title and introductory paragraph I should have done) the twofold relations belonging to the root-systems 311 and 221 respectively, but the threefold relations belonging to the root-systems 41 and 32 respectively. The word "quadric," p. 582, line 1, should be read "cubic." The proper title is, "On the Conditions for the Existence of certain Systems of Equal Roots of a Binary Quartic or Quintic."

March 4, 1869.

Lieut.-General SABINE, President, in the Chair.

In accordance with the Statutes, the names of the Candidates for election into the Society were read as follows :—

Sir Samuel White Baker, M.A.	John Robinson M'Clean, C.E.
William Baker, C.E.	George Matthey.
John J. Bigsby, M.D.	St. George Mivart.
Francis T. Buckland, M.A.	Prof. Alfred Newton, M.A.
George William Callender, F.R.C.S.	Captain Sherard Osborn, R.N., C.B.
Charles Chambers.	Oliver Pemberton.
Walter Dickson, M.D.	Charles Bland Radcliffe, M.D.
Henry Dircks.	William Henry Ransom, M.D.
Sir George Floyd Duckett, Bart.	Theophilus Redwood, Ph.D.
William Esson, M.A.	John Russell Reynolds, M.D.
Alexander Fleming, M.D.	Vice-Admiral Sir Robert Spencer Robinson, K.C.B.
Prof. George Carey Foster, B.A.	Major James Francis Tennant, R.E.
Peter Le Neve Foster, M.A.	Edward Thomas.
William Froude, M.A., C.E.	Prof. Wyville Thomson, LL.D.
Edward Headlam Greenhow, M.D.	Col. Henry Edward Landor Thuillier, R.A.
William Withey Gull, M.D.	Cromwell Fleetwood Varley, C.E.
G. B. Halford, M.D.	Augustus Voelcker, Ph.D.
Townshend Monckton Hall.	Edward Walker, M.A.
Edmund Thomas Higgins, M.R.C.S.	George Charles Wallich, M.D.
Charles Horne.	Henry Wilde.
James Jago, M.D.	Samuel Wilks, M.D.
George Johnson, M.D.	
J. Norman Lockyer.	
James Atkinson Longridge, C.E.	

I. "Appendix to the Description of the Great Melbourne Telescope."

By T. R. ROBINSON, D.D., F.R.S., &c. Received February 10, 1869.

(Abstract.)

Since this paper was read the author has made several observations of the quantity of light transmitted by object-glasses, and determined the index of absorption in various specimens of glass. The results of some of these are in accordance with the opinion expressed in the paper ; but others present a difference which is very satisfactory as indicating a surprising progress in the manufacture of optical glass. The observations were made by means of Zöllner's photometer.

The following results were obtained for the intensity of the light transmitted by a variety of object-glasses :—

Description.	Aperture.	Focus.	Intensity.
	in.	in.	
a. Triple object-glass	2·75	48	0·5497
b. Double	3·80	63	0·5962
c. Double	3·25	48	0·6567
d. Double	6·50	96	0·6772
e. Double	5·50	...	0·7928
f. Double, inner surface cemented.....	5·00	...	0·8739
g. Double, cemented	12·00	224	0·8408
h. Double	3·20	...	0·7393

Of the above, *a* belongs to the Armagh Observatory ; it is by one of the Dollonds, older than 1790, and is probably one of their first attempts at a triple combination. *b* is the original object-glass of the Armagh circle ; it was made by Tulley about 1828 ; the crown is greenish, and is supposed to be English ; the flint is believed to have been from Daguet. *c* was made for the author by Tulley in 1838 ; its glass is French, the crown is greenish. *d* is by Cauchoix ; the crown is greenish, and has probably a high *n*, but its mean thickness is only 0·39. *e* is by Messrs. Cooke ; the glass is Chance's. *f* is by Grubb, the glass Chance's : the very high transmission of this lens is in part due to the cementing of the adjacent surfaces, which, while it makes more difficult the correction of spherical aberration, removes almost entirely the reflection at a surface of crown and one of flint : the factor for this = 0·9036 ; and if the *I* be multiplied by this, we obtain 0·7806, nearly that of *e*, the difference being due to the reflection at the film of cement. *g* is also by Grubb, and cemented ; the glass is by Chance. *h* is by Fraunhofer.

On examining this Table the progressive increase in the light of the object-glasses is evident. The first two, which may be considered good specimens of the early achromatics, have less illuminating-power than the Herschelian reflector. A great advance was made by Guinand and those who followed in his steps ; and a still greater one by Chance, whose glass is nearly perfect as to colour and transparency.

The same inference follows from the author's measure of the index of

2 A 2

absorption, n . The specimens examined were, with two exceptions, prisms; and this form is very convenient. If a ray is incident on an isosceles prism parallel to its base, it emerges parallel to itself after undergoing total internal reflection at the base; and the length of the path of the light within the glass, and the loss by the two reflections, are easily calculated from the known angle and refractive index. The mean index used in the calculation was that of the line E.

The results are given in the following Table, in which are introduced those given in the paper that they may be referred to at once; and there is added to them one found in Bouguer's 'Traité d'Optique,' which seems trustworthy.

Description.	n
1. Prism, originally Captain Kater's	0·1829
2. French plate, Mr. Grubb	0·1728
3. London plate, Mr. Grubb	0·2140
4. Two of same, Mr. Grubb	0·1446
5. Prism, Mr. Grubb	0·0617
6. Bouguer's glass	0·1895
7. Gassiot's prisms	0·6209
8. Prism by Dubosq, flint	0·1504
9. Prism by Merz, flint	0·1089
10. Prism by Merz, crown	0·0858
11. Prism by Merz, flint	0·1065
12. Prism by Grubb	0·0218
13. Cylinder of crown	0·0272
14. Cylinder of flint	0·0090

No. 1 was shown to the author in 1830 by Captain Kater, as the *chef-d'œuvre* of the Glass-Committee; he used it as the small speculum of his Newtonian. Afterwards it came into the possession of the late Lord Rosse, who made the above measures with Bunsen's photometer in 1848. It is English plate, greenish.

Nos. 2, 3, 4, 5 were measured by Mr. Grubb in 1857. No. 5 was a prism of 90° . He does not remember its history; but evidently it was of Chance's glass.

No. 6 is described by Bouguer as "glace," 3 Paris inches thick. It was probably that of St. Gobain, which has probably not varied in composition, and its μ has been used in the computation.

No. 7 consists of two prisms of 60° , which Mr. Gassiot, with his wonted kindness, intrusted to the author for some inquiries about the improvement of the spectroscope. They are by Merz, of glass which seems nearly identical with Faraday's dense glass, having a specific gravity of 5·1, and a mean $\mu = 1·7664$. It is very pellucid, but, like its prototype, has a yellowish tinge, probably given by the large proportion of lead. As Merz does not polish the base or ends of his prisms, the usual method could not be employed; but the prisms were put together with the angles opposed, and a drop of olive-oil between, and the reflections allowed for. The great

absorption is remarkable, and apparently cannot be explained by the colour of the glass.

No. 8 is of 60° ; its μ for $E=1.620$. It is free from colour, and an evident improvement on the earlier ones.

No. 9, a prism of 90° , was given to the author by Dr. Lloyd for a small mirror in the Newtonian form of the Armagh 15-inch reflector; its μ for $E=1.6188$.

No. 10, of 90° , was obtained by the late Lord Rosse to be similarly used in his 3-feet Newtonian; its μ for $E=1.5321$.

No. 11, of 60° , obtained at Munich in 1837. For these measures the ends were polished flat; its μ for $E=1.6405$.

These three show considerable progress, and an object-glass made of such materials would have a great power of transmission, though much behind the following.

No. 12 is of 90° . Its glass is from Chance; its μ for $E=1.6216$.

No. 13 is a cylinder 2.2 inches in diameter, and 4.3 long, which Mr. Grubb obtained from Messrs. Chance for these measures; its μ for $E=1.5200$.

No. 14 is a cylinder got at the same time, 2.1 inches in diameter and 4.4 long; its μ for $E=1.6126$; the ends of both are polished flat, and they are of wonderful transparency.

If, as there is good ground for hoping, Messrs. Chance shall succeed in manufacturing large disks of the same perfection as these two cylinders, the author's comparison of the achromatic and the reflector must be considerably modified.

Assuming $n=.02$, he calculates that the aperture of an achromatic, of focal length equal to 18 times the aperture, equivalent to a 4-feet Newtonian, is 35.435 inches. This aperture would be diminished if the process of cementing were found applicable to lenses of such magnitude.

The author concludes with suggesting that, as very slight variations in the manufacture of glass seem to make great changes in its absorptive power, it would be prudent to examine the value of n in the disks intended for lenses of any importance. This could be done by polishing a couple of facets on their edges, and need not involve the sacrifice of many minutes.

II. "Note on the Formation and Phenomena of Clouds." By JOHN TYNDALL, LL.D., F.R.S. Received January 25, 1869.

It is well known that when a receiver filled with ordinary undried air is exhausted, a cloudiness, due to the precipitation of the aqueous vapour diffused in the air, is produced by the first few strokes of the pump. It is, as might be expected, possible to produce clouds in this way with the vapours of other liquids than water.

In the course of the experiments on the chemical action of light which have been already communicated in abstract to the Royal Society, I had frequent occasion to observe the precipitation of such clouds in the experimental tubes employed; indeed several days at a time have been devoted

solely to the generation and examination of clouds formed by the sudden dilatation of the air in the experimental tubes.

The clouds were generated in two ways: one mode consisted in opening the passage between the filled experimental tube and the air-pump, and then simply dilating the air by working the pump. In the other, the experimental tube was connected with a vessel of suitable size, the passage between which and the experimental tube could be closed by a stopcock. This vessel was first exhausted; on turning the cock the air rushed from the experimental tube into the vessel, the precipitation of a cloud within the tube being a consequence of the transfer. Instead of a special vessel, the cylinders of the air-pump itself were usually employed for this purpose.

It was found possible, by shutting off the residue of air and vapour after each act of precipitation, and again exhausting the cylinders of the pump, to obtain with some substances, and without refilling the experimental tube, fifteen or twenty clouds in succession.

The clouds thus precipitated differed from each other in luminous energy, some shedding forth a mild white light, others flashing out with sudden and surprising brilliancy. This difference of action is, of course, to be referred to the different reflective energies of the particles of the clouds, which were produced by substances of very different refractive indices.

Different clouds, moreover, possess very different degrees of stability; some melt away rapidly, while others linger for minutes in the experimental tube, resting upon its bottom as they dissolve like a heap of snow. The particles of other clouds are trailed through the experimental tube as if they were moving through a viscous medium.

Nothing can exceed the splendour of the diffraction-phenomena exhibited by some of these clouds; the colours are best seen by looking along the experimental tube from a point above it, the face being turned towards the source of illumination. The differential motions introduced by friction against the interior surface of the tube often cause the colours to arrange themselves in distinct layers.

The difference in texture exhibited by different clouds caused me to look a little more closely than I had previously done into the mechanism of cloud-formation. A certain expansion is necessary to bring down the cloud; the moment before precipitation the mass of cooling air and vapour may be regarded as divided into a number of polyhedra, the particles along the bounding surfaces of which move in opposite directions when precipitation actually sets in. Every cloud-particle has consumed a polyhedron of vapour in its formation; and it is manifest that the size of the particle must depend, not only on the size of the vapour polyhedron, but also on the relation of the density of the vapour to that of its liquid. If the vapour were light, and the liquid heavy, other things being equal, the cloud-particle would be smaller than if the vapour were heavy and the liquid light. There would evidently be more shrinkage in the one case than in the other: these considerations were found valid throughout the

experiments; the case of toluol may be taken as representative of a great number of others. The specific gravity of this liquid is 0·85, that of water being unity; the specific gravity of its vapour is 3·26, that of aqueous vapour being 0·6. Now, as the size of the cloud-particle is directly proportional to the specific gravity of the vapour, and inversely proportional to the specific gravity of the liquid, an easy calculation proves that, assuming the size of the vapour polyhedra in both cases to be the same, the size of the particle of toluol cloud must be more than six times that of the particle of aqueous cloud. It is probably impossible to test this question with numerical accuracy; but the comparative coarseness of the toluol cloud is strikingly manifest to the naked eye. The case is, as I have said, representative.

In fact, aqueous vapour is without a parallel in these particulars; it is not only the lightest of all vapours, in the common acceptation of that term, but the lightest of all gases except hydrogen and ammonia. To this circumstance the soft and tender beauty of the clouds of our atmosphere is mainly to be ascribed.

The *sphericity* of the cloud-particles may be immediately inferred from their deportment under the luminous beams. The light which they shed when spherical is *continuous*: but clouds may also be precipitated in solid flakes; and then the incessant sparkling of the cloud shows that its particles are *plates*, and not spheres. Some portions of the same cloud may be composed of spherical particles, others of flakes, the difference being at once manifested through the *calmness* of the one portion of the cloud, and the *uneasiness* of the other. The sparkling of such flakes reminded me of the plates of mica in the River Rhone at its entrance into the lake of Geneva, when shone upon by a strong sun.

III. "On the Behaviour of Thermometers in a Vacuum." By
BENJAMIN LOEWY, F.R.A.S. Communicated by Prof. STOKES,
Sec. R.S. Received January 8, 1869.

1. In the year 1828 General Sabine made a series of pendulum-experiments* in a receiver from which the air was exhausted, and observed incidentally that on the pump being worked the thermometer in the receiver fell about 7-tenths of a degree of Fahrenheit's scale when the pressure was reduced to 7 inches, while the converse took place when the air was re-admitted. He ascribed this effect to the removal of the pressure of the atmosphere on the exterior of the bulb and tube of the thermometer; and to ascertain whether this explanation was correct the following experiment was made:—A thermometer being immersed in pounded ice and placed on the brass plate of an air-pump, the mercury coincided exactly with the division of 32°; it was then covered with a receiver, and the air withdrawn; the thermometer fell as the pump was worked, and when the

* Published in the Philosophical Transactions, 1829, part 1.

gauge indicated a pressure of half an inch the mercury stood at $31^{\circ}25$; on readmitting the air it rose again to 32° . The experiment was repeated, with precisely similar results; and a correction was ultimately adopted, corresponding to the varying pressures in the receiver, in order to reduce the pendulum-experiments to the true temperature at which they were made.

2. It was generally admitted that this apparent fall of the mercury arose from a change in the capacity of the interior of the thermometer; and the physicists, especially the pendulum-experimenters who followed in General Sabine's steps, never neglected this correction when their object was to discuss the results of experiments made in a vacuum, and in the reduction of which the temperature entered as an element.

In the pendulum-experiments which were made at the Kew Observatory in connexion with the Great Trigonometrical Survey of India (*vide* Proceedings of the Royal Society, No. 78, 1865), the thermometers used were, before the discussion of the observations, subjected to independent experiments, to determine their "vacuum-correction," which was found nearly the same for each of the two thermometers employed, viz. $0^{\circ}43$. In these experiments the two thermometers were suspended, together with another (the latter enclosed in a sealed glass tube, and hence surrounded by air), in the receiver, and their readings taken some time after the exhaustion, sufficient to equalize its effect upon all three thermometers, bearing in mind the fact that the thermometer in the glass case would take a somewhat longer time for showing changes of temperature than those without such an enclosure. The arrangement of the experiments was precisely the same as that originally adopted by General Sabine; and the precaution taken as regards the time of reading the different thermometers left no doubt on my mind that the observed difference of $0^{\circ}43$, by which amount the thermometers exposed to the effect of exhaustion were in every experiment found to read *less* than that enclosed in a glass tube, gave the required vacuum-correction in this particular case. It is also clear that in this method of carrying on the experiment the refrigeration due to the work done by the expanding air during the process of exhaustion will affect all thermometers alike, and that consequently the *residual difference* must be due to other causes.

3. One point, however, was overlooked in these experiments, viz. to wait a number of hours and then to take another series of readings, in order to determine whether the effect of the removal of the atmosphere upon the capacity of a thermometer was only transient or permanent. Professor Oscar Meyer, in Breslau, was the first to call attention* to this question. While making some experiments on the internal friction of gases, he found that the primary effect of the exhaustion upon a thermometer was quite in accordance with the observations of General Sabine, but that after some time (for the thermometer employed by him, after about half an hour) this effect entirely disappeared. Captain Basevi, who

* *Vide* Poggendorff's 'Annalen,' vol. cxxv. p. 411.

has charge of the pendulum-experiments in India, communicated to me that the results of some experiments made by him strengthened Professor Meyer's conclusion, and caused him grave doubts as to the necessity of applying the vacuum-correction in pendulum-experiments, one swing often lasting in such experiments from five to eight hours.

4. It appeared to me that there were various sources of error in the experiments previously made. The only experiments which seem conclusive are those made by General Sabine with thermometers placed in ice; but we are not informed in the account of these experiments how long each of them lasted, probably because there was no reason to regard the element of time as of importance.

In the experiments made by comparing the thermometers with one enclosed in a glass tube and surrounded by air, it is obvious that the thermometers under comparison are throughout under different circumstances as regards their sensitiveness, and that this difficulty cannot be entirely overcome by allowing some time for the equalization of the original effect of the exhaustion. Again, it is questionable whether the glass tube which surrounds the thermometer which must be considered the standard of comparison has not, during the process of being closed up before the blow-pipe, been so heated that the remaining air, instead of representing the pressure of a whole atmosphere, is really of a much less density. Further, there is the question of time, raised by Professor Meyer and Captain Basevi.

In Professor Meyer's experiments one thermometer was placed within the receiver, and another suspended outside, on the exterior of the receiver itself. He found that exhaustion lowered the reading of the former by from one-half to one degree of the centesimal scale, but that after about half an hour both thermometers agreed again: the readmission of air caused the thermometer in the receiver to rise by the same quantity by which it had previously fallen; but after the lapse of some time the two thermometers read again alike. This lowering of the mercury on evacuation, and rising on readmission of the air, ceased almost entirely when the thermometer was introduced into the receiver immersed in dehydrated glycerine. From these observations Professor Meyer concludes that it is solely the mechanical labour of the air during expansion or compression which produces these fluctuations, and that they do not depend on the varying pressure upon the bulb of the thermometer. This conclusion may be correct as far as the particular thermometer is concerned which Professor Meyer employed, for it will be seen in the sequel that certain thermometers really behave exceptionally; but it will also appear on examining the experiments given in Table II. that two thermometers, under precisely the same external circumstances, and in close juxtaposition, often differ in their readings by half a degree of Fahrenheit's scale, and even more, without any assignable cause. We may obviously infer from this that two thermometers, arranged as in Professor Meyer's experiments, are not strictly comparable when small differences of temperature have to be ascertained.

5. In order to decide the question of the "vacuum-correction" by avoiding the above indicated sources of error, I had three pairs of thermometers made, each pair of equal shape and size as regards bulb and tube, but these pairs differing in this respect among themselves. These six thermometers were, in the manner which is shown in the annexed figure for one pair of them, enclosed in glass cases, which terminated in narrow tubes of about 5 inches in length. One case with its thermometer was left open at the top (A), while the other (A') with the corresponding thermometer was closed by a rapid puff of the blowpipe, without the possibility of heating the enclosed air and thus diminishing the pressure upon the enclosed thermometer.

There were thus subjected to experiment six thermometers, of three different forms, as may be seen from the following description of them:—

- (1) A* (No. 6700), *Spherical bulb*, diameter of bulb $\frac{1}{2}$ inch, length of stem 13 inches, enclosed in *open* case.
- (2) A' (No. 6701), *Spherical bulb*, diameter of bulb $\frac{1}{2}$ inch, length of stem 13 inches, enclosed in *shut* case.
- (3) B (No. 6703), *Cylindrical bulb*, $1\frac{1}{10}$ inch long, $\frac{3}{10}$ inch wide, length of stem 15 inches, enclosed in *open* case.
- (4) B' (No. 6702), *Cylindrical bulb*, $1\frac{1}{10}$ inch long, $\frac{3}{10}$ inch wide, length of stem 15 inches, enclosed in *shut* case.
- (5) C (No. 6704), *Spherical bulb*, diameter of bulb $\frac{3}{4}$ inch, length of stem 27 inches, enclosed in *open* case.
- (6) C' (No. 6982), *Spherical bulb*, diameter of bulb $\frac{3}{4}$ inch, length of stem 27 inches, enclosed in *shut* case.



The thermometers A, A', B, B' represent the usual form and size of these instruments, while those marked C, C' are unusually large, and would hardly be employed except for special purposes. The former had each degree divided into five parts, hence reading by estimation to $\frac{1}{50}$ of a degree, while the latter had each degree divided into ten parts, each of which occupied about the space of one degree in the common form; $\frac{1}{100}$ of a degree of Fahrenheit's scale could thus be read with the utmost accuracy.

6. The thermometers and the receiver employed in these observations were made by Mr. L. Casella, who took the greatest interest in the purpose of the experiments, and consequently took especial care to make the instruments as perfect as possible.

* These letters are the same as those used in the succeeding Table of Experiments to designate the different thermometers.

The thermometers were tested before putting them into the glass cases by comparing them from three to three degrees with the Kew standard, taking a great number of readings by two independent observers for this purpose. From this comparison and by interpolation, the following Table of corrections for every degree over the range of temperature during the experiments was constructed. It will not only prove that the utmost precaution was taken to ensure the experiments against errors inherent in the instruments employed, but will also show the excellency of the thermometers and the degree of accuracy now obtained by eminent makers in the construction of these instruments.

TABLE I. Corrections to be applied to the Readings of the Thermometers.

N.B. The corrections are in all cases subtractive.

Thermo- meters.	40°	41°	42°	43°	44°	45°	46°	47°	48°	49°	50°	51°	52°	53°	54°	55°	56°	57°	58°	59°	60°
No. 6700.	12	12	12	12	11	13	16	19	18	17	16	16	17	17	16	16	17	19	20	17	15
No. 6701.	13	13	13	12	12	15	19	22	21	19	18	17	15	14	15	15	17	19	20	19	18
No. 6702.	13	13	11	09	08	09	10	11	11	11	11	11	11	11	11	12	13	14	15	13	11
No. 6703.	09	09	10	12	13	16	18	20	19	18	18	16	13	10	08	07	08	09	10	10	09
No. 6704.	09	09	10	10	11	13	16	19	17	15	13	13	12	12	12	12	12	12			
No. 6982.	24	24	23	22	21	23	24	25	25	25	25	25	26	27	28	28	30	32			

7. The thermometers were placed in the receiver, arranged close to each other on a board fixed to a support, the four smaller thermometers on one side, the two larger ones on the other; and the manner of proceeding with each experiment was the following. Before pumping, all the thermometers were twice read in rapid succession; after exhausting the receiver to between one and two inches of pressure (a manipulation which generally lasted about ten minutes), two or more readings were again taken to determine the "immediate effect of exhaustion" on each thermometer. After an interval of several hours the thermometers were supposed to have assumed the surrounding temperature, and two readings were now taken for the "residual effect of exhaustion." The whole apparatus was then left undisturbed for nearly a whole day, when another set of readings were taken, and the apparatus was refilled. After readmission of the air the temperature shown by the instruments was immediately registered to find the heating-effect upon them of the inrush of air.

The readings, both for the comparison of the instruments and during the experiments themselves, were taken alternately by Mr. Thomas Baker, Assistant at the Kew Observatory, and myself. By the kind permission of Mr. Balfour Stewart, Superintendent of the Kew Observatory, I was enabled to avail myself of the obliging assistance of Mr. Baker and his great experience in thermometric experiments. I take this opportunity of expressing to both these gentlemen my gratitude for the aid given to me in the pursuit of this inquiry.

8. In the following Table I give the results of six experiments which were made for my purpose, leaving their discussion for the next paragraphs. A number of experiments made previously to these here given, in the large Kew receiver, had to be rejected; for the apparatus has leaked latterly to a considerable amount during a day, causing a feeble but con-

TABLE II. Experiments for determining the Vacuum-correction of Thermometers.

No. of experiment.	Designation of thermometers.	Corrected means of readings before exhaustion.	Means of readings immediately after exhaustion.	Pressure in the receiver.	Interval between the last and next reading.	Means of readings after interval.	Pressure in the receiver.	Interval between the last and next reading.	Means of readings after interval.	Pressure in the receiver.	Interval between the last and next reading.	Means of readings after interval.	Pressure in the receiver.	Interval between the last and next reading.	Means of readings after reading.
I.	A	52.40	50.56	3.07	3 15	50.64	3.16	18 37	51.47	3.50	53.44
	A'	52.47	51.56	51.02	51.79	53.03
	B	52.56	50.79	50.45	51.39	53.08
	B'	52.60	51.55	50.92	51.71	52.99
	C	51.82	50.21	50.63	51.32	52.79
	C'	51.48	50.14	50.57	51.32	52.33
II.	A	53.63	51.77	2.14	3 40	53.45	2.20	20 15	48.97	2.47	50.55
	A'	53.34	52.64	53.85	49.45	50.07
	B	53.17	51.62	53.26	49.09	50.83
	B'	53.24	52.45	53.74	49.45	50.31
	C	52.94	51.42	53.33	49.21	50.43
	C'	52.50	51.32	53.28	49.27	49.95
III.	A	50.94	49.15	1.63	2 20	51.37	1.74	20 5	47.10	1.90	48.74
	A'	50.93	49.96	51.68	47.40	48.02
	B	50.78	49.13	51.10	46.97	48.78
	B'	50.79	49.84	51.58	47.32	48.20
	C	50.67	49.08	51.10	46.93	48.23
	C'	50.25	48.94	51.04	46.96	47.64
IV.	A	49.53	47.78	1.72	3 10	49.92	1.80	19 55	51.25	2.16	52.72
	A'	49.20	48.29	50.23	51.54	52.10
	B	48.99	47.42	49.71	51.09	52.86
	B'	49.08	48.11	50.15	51.47	52.23
	C	48.80	47.28	49.65	50.93	52.25
	C'	48.26	47.03	49.59	50.93	51.61
V.	A	55.03	52.64	1.16	0 35	52.68	1.16	18 15	47.55	1.43	48.96
	A'	54.86	53.69	53.20	48.05	48.56
	B	54.63	52.68	53.70	47.86	49.55
	B'	54.77	53.55	53.16	48.16	48.84
	C	54.58	52.72	52.90	47.73	48.88
	C'	54.09	52.61	52.82	47.93	48.48
VI.	A	50.34	48.24	0.91	0 25	49.33	0.91	20 0	42.40	1.10	24 50	46.88	1.44	..	48.58
	A'	50.16	49.20	49.72	42.90	47.13	47.80
	B	50.04	48.37	49.12	42.48	46.70	48.78
	B'	50.12	49.10	49.45	42.94	47.06	48.00
	C	49.87	48.19	49.06	42.60	46.65	47.84
	C'	49.40	48.11	48.89	42.59	46.48	47.23

stant inrush of air, which vitiated the ultimate results. Only those experiments are here given and discussed which were made in a smaller receiver expressly constructed for my purposes by Mr. Casella.

In this Table the corrected means of the individual observations are given, while a larger Table, embodying also the latter, has been deposited with the Royal Society for future reference. It is seen from this larger Table that the average amount of error in these observations is not more than about two-hundredths of a degree of Fahrenheit. In a very few cases only, where the thermal effect was not quite completed when the readings were taken, errors of about one-tenth of a degree occur; care, however, was taken in these solitary cases to ascertain the completion of the effect by the more close agreement of a new series of observations.

9. A glance at the preceding Table will at once show that the immediate effect of exhaustion is a fall, that of readmission of air a rise of all thermometers, and that there is at once a difference in the behaviour between the thermometers A', B', C', which are still surrounded by air, and A, B, C, which are in a vacuum. But this difference is also observable to a certain extent when the receiver is refilled, and when, as regards external pressure, all thermometers are in the same condition; hence this *immediate* difference must have another cause than the supposed change in the capacity of the instruments; at any rate if a permanent difference is found afterwards in consequence of such a change, it must be included in that difference which shows itself immediately. The cause of the latter is obvious. The thermometers in closed cases lag a little behind when they are affected by such sudden fluctuations as those produced in these experiments, and they assume, as the experiments have shown, the normal temperature a little later.

The following Table gives the immediate fall and rise of all thermometers, observed respectively on evacuating and refilling the receiver, and the immediate mean difference between the differently placed thermometers. It exhibits a very close agreement between the effect of exhaustion and that of readmission of air; but its more important practical purpose is to show that *an error of nearly two degrees of Fahrenheit is made in thermometer-readings in a receiver immediately after exhaustion or readmission of air.*

Immediate effect of exhausting the Receiver.

		Thermometers falling.					
		A.	A'.	B.	B'.	C.	C'.
Experiment	I.	1.84	0.91	1.77	1.05	1.61	1.34
	II.	1.86	0.70	1.55	0.79	1.52	1.18
	III.	1.79	0.97	1.65	0.95	1.59	1.31
	IV.	1.75	0.91	1.57	0.97	1.52	1.23
	V.	2.39	1.17	1.95	1.22	1.86	1.48
	VI.	2.10	0.96	1.67	1.02	1.68	1.29
Means		1.95	0.94	1.69	1.00	1.63	1.30
Differences immediately observable		1.01		0.69		0.33	

Immediate effect of refilling the Receiver.

Thermometers rising.

		A.	A'.	B.	B'.	C.	C'.
Experiment	I.	1.97	1.24	1.69	1.28	1.47	1.01
"	II.	1.58	0.62	1.74	0.86	1.22	0.68
"	III.	1.64	0.62	1.81	0.88	1.30	0.68
"	IV.	1.47	0.56	1.77	0.76	1.20	0.68
"	V.	1.41	0.51	1.69	0.68	1.15	0.55
"	VI.	1.64	0.67	2.08	0.94	1.38	0.75
Means	1.62	0.70	1.63	0.90	1.29	0.73
Differences immediately observable	0.92		0.73		0.56	

10. Now if this immediate difference would entirely disappear after some time (say, after a number of hours, or a whole day), or would become so small as to be within the limits of experimental errors, the question whether a vacuum-correction is necessary would have to be negatived. We may presume that after some time the refrigeration caused by the exhaustion disappears, and that the thermometers are then solely or chiefly influenced by the temperature of the surrounding air; if then a difference still appears in the behaviour of the thermometers, this permanent difference must obviously be caused by something independent of the temperature, and its source must be looked for in a change of the instruments themselves.

The thermometers C, C' (that is, those with unusually large spherical bulbs and long stems) differed in their behaviour entirely from the others of common form and size; they will be spoken of afterwards.

The thermometers A, A', B, B' exhibited, on the contrary, as the following Table shows, constant differences when read from three hours to as long as two days after exhaustion. A, A', B, B' signify in this Table the *readings* of the corresponding thermometers taken so long a time after exhaustion as to exclude all possibility of introducing the effect of it. The Table gives only the differences, the readings themselves are given in Table II., with the times at which they were taken.

		A' - A.	B' - B.
Experiment	I.	0.38	0.47
"	II.	0.32	0.32
"	III.	0.40	0.48
"	IV.	0.48	0.36
"	V.	0.31	0.48
"	VI.	0.30	0.35
"	VII.	0.31	0.44
"	VIII.	0.29	0.38
"	IX.	0.52	0.46
"	X.	0.50	0.30
"	XI.	0.21	0.33
"	XII.	0.50	0.46
"	XIII.	0.25	0.36
Mean difference	0.37	0.40

11. These readings tell all the same thing, and taken separately agree closely with the mean result. If the result, found by another method, for the thermometers tested for the vacuum-correction during the pendulum-experiments for the Indian Survey, which gave $0^{\circ}\cdot43$, is added, it may be stated, as first result of these experiments, *that a thermometer of common form and size will, if used in the vacuum of a receiver, require an additive correction of four-tenths of a degree of Fahrenheit's scale, provided that no readings are taken until the immediate effect of exhaustion, which amounts to nearly two degrees, is equalized.*

12. The two large thermometers gave the following differences:—As some of them are in an opposite direction, I denote the expected differences by the sign +, and those on the wrong side by —.

		C'—C.			C'—C.	
Experiment	I.	—0°·06	Experiment	IV.....	—0°·06
			0°·00			0°·00
„	II.	—0°·05	„	V.....	—0°·08
			+0°·06			+0°·20
„	III.	—0°·06	„	VI.....	—0°·17
			+0°·03			—0°·01

These results only strengthen the validity of the others; for obviously we have, in regard to these large thermometers, in fact no other difference but that arising from experimental errors, local currents, &c.

The first explanation of this behaviour that suggested itself was, that the thermometer which was supposed to be surrounded by air, had some flaw in the glass envelope, which allowed the air to escape during the pumping, so that there was really no difference of condition between it and its companion thermometer. A most careful examination of the case did not lead to the discovery of such a cause of leakage; and as the thermometer in the closed case lagged behind the other in the same manner as the other thermometers in a similar condition did, I can only come to the conclusion that thermometers with large bulbs and stems really behave differently, or that the permanent effect of exhaustion is imperceptible.

[With a view of determining whether the exceptional behaviour of the large thermometers could be accounted for by greater strength of their glass bulbs, Professor Stokes kindly suggested to me a comparison of the relative thickness of the glass of the bulbs by placing on it a very minute opaque dot, and measuring the apparent distance of the dot from its reflected image by a lens. I found the following results:—

1st. The thickness of the glass varies not inconsiderably in different parts of the bulb of one and the same thermometer.

2nd. The thickness of the glass in the bulbs of the large thermometers was, on the average, twice that of the small spherical bulbs.

3rd. The thermometers with cylindrical bulbs had nearly three times the thickness of those with small spherical bulbs; this thickness, however, was considerably less at the base of the cylinder.

The behaviour of the large thermometers may thus be referred to their greater strength ; but it also appears that in thermometers with cylindrical bulbs great strength will not obviate the necessity of a vacuum-correction. —Added February 27th.]

13. In order to test the accuracy of the preceding results, the *closed* cases of the thermometers were opened ; hence all instruments were in the same condition when the receiver was exhausted. The result was the following :—

Thermometers.	A.	A'.	B.	B'.	C.	C'.
Corrected mean of readings before exhaustion	56·24	56·03	56·23	56·08	55·17	55·23
Corrected mean of readings after exhaustion	53·65	54·02	54·24	54·42	53·00	53·03
Corrected mean of readings after an interval of 26 ^h 15 ^m	52·56	52·27	52·47	52·49	51·87	51·85
Differences	—0·29		+0·02		—0·02	

that is, the difference shown is either inappreciable, or due to accidental causes.

14. These experiments have sufficiently established the fact that in vacuum-experiments due attention must be given to the causes which influence the thermometers employed in the receiver, and that in delicate experiments an independent determination of the vacuum-correction is indispensable.

No new explanation of the cause of the permanent fall in a vacuum has suggested itself during the experiments. General Sabine's original explanation, that the removal of the atmospheric pressure alters the capacity of the thermometer, is probably the most correct, especially when it is considered that the only objection ever raised against it, that of time reproducing the original state of the instrument, has been proved groundless by these experiments.

In conclusion I have to thank the President and Council of the Royal Society for defraying the expenses incurred in these experiments.

IV. "Account of the Building in progress of erection at Melbourne for the Great Telescope." In a Letter addressed to the President of the Royal Society by Mr. R. J. ELLERY, of the Observatory, Melbourne. Communicated by the President. Received February 27, 1869.

Observatory, Melbourne, Jan. 4, 1869.

MY DEAR SIR,—The telescope has at length arrived, and we are now very busy getting it erected ; for nothing could be done towards it till the great machine itself came to hand. It will be nearly two months before it can be fairly tried, when a spacious rectangular building and its travelling roof will be completed.

Mr. Le Sueur arrived nearly two months before the telescope, having

come by the overland mail, and the ship carrying the telescope making an unusually long passage.

The principal or more delicate portions of the instrument came out in good order: the specula are still in thin coats of varnish, and their surfaces appear in perfect good order. Some of the large castings and portions of the gearing had got rusted, but not to an injurious extent. The piers were completed on New Year's morning, and form a magnificent piece of masonry, the stone employed being the grey basalt, so common here (called "blue stone"), in blocks from one to three tons in weight each. The building we have finally decided upon is of stuccoed brickwork 80 feet long by 40 wide. Forty in length is taken up by the telescope-room, which is covered by a ridged roof of iron travelling on rails on the walls, and moves back on the other 40 feet of the building, leaving the telescope in the open air. The back 40 feet is covered by a fixed roof lower than the moveable one, and will contain a polishing- and engine-room, a capacious laboratory, and an office for observer. The cost of piers, building, and roof will be about £1700. The Government, with hard economy in all other directions, have still acted very liberally about this work; and I only trust the telescope itself will turn out all that is expected of it. The micrometer and spectrum-apparatus have not arrived yet.

Our magnetographs do their work smoothly and satisfactorily. The photography has become a part of the routine of the Observatory now. I have been anxiously awaiting the arrival of the baro- and thermographs, and we look for them every day, although I have had no advices of their having been shipped. I suppose you will have seen Mr. Verdon long before this reaches you.

I remain, my dear Sir,

*Major-General Sabine,
Royal Society, London.*

Yours faithfully,
ROBERT J. ELLERY.

March 11, 1869.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

- I. "Contributions to the Fossil Flora of North Greenland, being a Description of the Plants collected by Mr. Edward Whymper during the Summer of 1867." By Prof. OSWALD HEER, of Zurich. Communicated by Prof. G. G. STOKES, Sec. R.S.

(Abstract.)

The author stated that the examination of the fossil plant-remains which had been at various times brought to Europe from North Greenland by M'Clintock, Inglefield, Colomb, and others, as well as by Mr. Olrik, formerly Inspector of North Greenland, the results of which were pub-

lished in his work, '*Flora Fossilis Arctica*,' had led him to certain conclusions, the verification of which, by means of additional material, became very important.

Accordingly Mr. R. H. Scott had applied to the British Association at the Nottingham Meeting in 1866 for a grant of money towards the expenses of an expedition to Greenland. A sum of money was voted to a Committee, consisting of Dr. Hooker, Sir W. Trevelyan, Dr. E. Perceval Wright, and Mr. E. Whymper, with Mr. Scott as Secretary. This grant was subsequently most liberally augmented by the Government-Grant Committee of the Royal Society. The condition laid down by both of these bodies was that a complete series of specimens should be deposited in the British Museum.

Mr. Scott being unable to go to Greenland himself, Mr. Whymper, who had, previously to the nomination of the Committee, formed the plan of travelling in Greenland, undertook to visit the shores of the Waigat, and to carry out the wishes of the Committee, if his time would permit him; and grants of money were accordingly intrusted to him conditionally. Mr. Whymper took with him Mr. Robert Brown, to assist in the collection of the specimens; and the party ultimately arrived in Greenland on the 16th of June, 1867.

Prof. Heer then gives extracts from Mr. Whymper's Report, submitted by the Committee to the British Association in August last, and also from notes furnished to him by Mr. Brown. From these statements a considerable amount of information as to the geology of the district is derived.

All the specimens which had been previously brought to Europe, with the exception of a few brought by Dr. Lyall, had been found at a place called Atanekerdruk, on the mainland of Greenland, in lat. 70° or thereabouts. Dr. Lyall's specimens were found on Disco Island, at the opposite side of the Waigat Strait from Atanekerdruk. Mr. Whymper accordingly, having engaged a number of natives as labourers, went to Atanekerdruk in the first instance, reaching it on the 22nd of August. The party remained at the spot for some days, and made a large collection of specimens. The plant-beds are reported to be on a hill, at a height of nearly 1200 feet above the sea, and the deposit is limited in extent. Details of the different beds observed are contained in the paper.

Professor Heer observes that the statements of Messrs. Whymper and Brown confirm the accounts of Olrik and Inglefield respecting the stratification of the coal-deposits and plant-beds of Atanekerdruk. They show that there is a considerable succession of sedimentary strata, pierced by volcanic rocks which form the summits of the mountains. Fossil plants occur in all the beds; but the Siderite and Limonite contain them in the greatest abundance and in the best state of preservation. In fact the slabs from these beds are quite covered with specimens, lying in every direction.

With the vegetable remains two land-insects were discovered. Of the plants many species were inhabitants of marshy or moory ground, viz. *Phragmites*, *Sparganium*, *Taxodium*, and *Menyanthes*, which are all indicative of a freshwater deposit, as is also a *Cyclas*, of which mollusk one valve was found. These facts, taken together with the absence of marine forms, show the deposit at Atanekerdruk to have been a strictly freshwater formation.

After completing the examination of the mainland at this point, the party crossed the Waigat, and landed on Disco Island, where they found plant-remains at two localities, Ujararsusuk and Kudliset.

Coal-seams are exposed at several points on the east and south coasts of Disco ; but no specimens showing impressions of leaves, like those of Atanekerdruk, had ever been brought to Europe, except those obtained by Dr. Lyall. However, Sir C. Giesecke, in his MS. journal, of which a copy is in the possession of the Royal Dublin Society, mentions that he had noticed such impressions.

The coal has been worked at several points, if the rough operations which have been carried on deserve the name of workings. It is at present only obtained at the one spot, Ujararsusuk. The coal occurs interstratified with sandstones and shales, which rest on trap. The fossils were discovered among the débris brought down by the streams, and were traced up to a bed of brown sandstone about 100 feet above the sea.

At Kudliset, the deposits are very similar to those just described ; and there also the fossils were found, in the first instance, in a torrent-bed.

The shores of the Waigat were examined for some distance to the northwards, on each side of the strait, without any fresh discoveries being made, and the party returned to Atanekerdruk.

Mr. Whymper, in concluding his report, says that the success of the expedition has been "primarily due to the invaluable information given by Herr C. S. M. Olrik, the Director of the Greenland Trade. Scarcely less are our thanks due to Herr K. Smith, the present Inspector of North Greenland, and to Herr Anderson, of Ritenbenk. Both of these gentlemen gave much assistance at considerable personal trouble ; and without their assistance it would have been almost impossible to obtain the collections."

The general conclusion to be drawn from the accounts of the succession of strata &c. is that, on both sides of the Waigat, the sedimentary rocks are covered by Miocene deposits pierced by volcanic rocks, which appear in places as thick beds of basalt and trap.

In his summary of the botanical results of the expedition, the author announces the identification of fourteen species from Disco Island, among which *Platanus Guillelmæ* (Göpp.) and *Sequoia Couttsiæ* (Hr.) are the most common. Of *Magnolia Inglefieldi*, a species originally identified by means of leaves found at Atanekerdruk, two cones were found in the Disco beds, thus corroborating the previous determination, and proving to us that

this splendid evergreen ripened its fruit so far north as the parallel of 70°.

Seven out of the Disco species occur also at Atanekrdluk ; and eight agree with those of the Lower Miocene of Europe. The age of the deposit is accordingly well ascertained.

The collection from Atanekrdluk contains 73 species, of which 25 are new to Greenland. Some of these are known European forms, especially *Smilax grandifolia*, which, at the Miocene epoch, occurred over the whole of Europe. Of *Sequoia Langsdorffii*, as was to be expected, abundant evidence has been accumulated, showing how favourable the conditions of climate and soil were to its growth.

Among the most interesting specimens are the flowers and fruit of a chestnut, the latter in a very perfect condition. The discovery of these proves to us that the deposits in which they are found were formed at different seasons, in spring as well as in autumn.

The Miocene plants discovered in Greenland have now reached the number of 137 species, and those of the Arctic Miocene Flora 194. Of the Greenland species 46, or exactly one-third, agree with those of the Miocene deposits of Europe. The determination of the age of the beds as Lower Miocene has accordingly been confirmed.

Four of the species agree with those of Bovey Tracey, among them *Sequoia Couttsiæ*, the commonest tree in the latter locality.

In concluding the first part of his paper, the author offers a *résumé* of the grounds on which the determinations of the species have been based.

Seventeen species are represented by the leaves and organs of fructification among the Greenland specimens.

Ten species are only represented by leaves in Greenland ; but their organs of fructification occur elsewhere.

Seventeen species of those of which only leaves are found exhibit, however, such marked characteristics, that there can be no doubt about their identification.

Five Cryptogams have been satisfactorily recognized.

Accordingly, though it must be allowed that the systematic position of many of the plants from North Greenland is still uncertain, yet the considerable number of absolutely identified species which can be produced enables us to form a clear idea of the Miocene Flora of North Greenland.

The second part of the paper contains the specific descriptions of the various forms.

The collection, consisting of some 300 specimens, has been deposited in the British Museum.

- II. "On the Specific Heat and other physical properties of Aqueous Mixtures and Solutions." By A. DUPRÉ, Ph.D., Lecturer on Chemistry at the Westminster Hospital, and F. J. M. PAGE. Communicated by C. BROOKE, F.R.S. Received February 4, 1869.

(Abstract.)

PART I.

Mixtures of Ethylic Alcohol and Water.

Section 1. *Specific Heat.*

For the methods employed in estimating the specific heat of these mixtures, see a former abstract, 'Proceedings of the Royal Society,' vol. xvi. p. 336.

In the present paper the authors give the specific heat of an additional number of mixtures, so as to complete the series for every 10 per cent. from water to absolute alcohol.

The following Table gives the mean of the results obtained in all experiments, details of seventy-four of which are given :—

Percentage of alcohol, by weight.	Specific heat found.	Specific heat calculated.	Difference.
5	101.502
10	103.576	96.043	+ 7.533
20	104.362	92.086	12.276
30	102.602	88.129	14.473
40	96.805	84.172	12.633
45	94.192	82.193	11.999
50	90.633	80.215	10.418
60	84.332	76.258	8.074
70	78.445	72.301	6.144
80	71.690	68.344	3.346
90	65.764	64.387	1.377
100	60.430

Section 2. *Heat produced by the mixing of Alcohol and Water.*

This was estimated as follows :—The liquid which formed the smallest portion of the mixture was sealed up in a thin glass bulb ; this was then introduced into the calorimeter, the glass bulb was broken, the mixture formed, and the rise in the temperature of the calorimeter observed.

The units of heat evolved in the formation of 5 grms. of each mixture were thus calculated, and found to be—

10 per cent. spirit 26.6850	50 per cent. spirit 35.5850
20 " " 43.9545	60 " " 27.2620
30 " " 47.9800	70 " " 18.8200
40 " " 44.8630	80 " " 12.4775
45 " " 38.8095	90 " " 7.7025

Section 3. *Boiling-points.*

A small flask was taken ; into this 100 cub. centims. of the mixture was

334 Messrs. Dupré and Page on the Physical Properties [Mar. 11,

introduced, and the mouth of the flask closed by a doubly perforated cork. Into one of these perforations a thermometer was introduced, into the other a bent tube, dipping beneath the surface of the liquid in the flask, and connected at its other extremity with a Liebig condenser. This tube had a lateral opening (inside the flask) just beneath the cork; by means of this the vapour escaped to the condenser, and trickled back into the flask after being condensed. Thus the composition of the mixture was retained as uniform as possible. Thus estimated, the barometer standing at 744·4 millims., the boiling-points are given in the following Table.

Percentage of alcohol, by weight.	Boiling-point observed.	Boiling-point calculated *.	Difference.
0	99·4
10	90·98	97·25	—6·27
20	86·50	95·10	—8·60
30	84·01	92·95	—8·94
40	82·52	90·90	—8·38
45	81·99	89·72	—7·73
50	81·33	88·60	—7·27
60	80·47	86·50	—6·03
70	79·61	84·35	—4·74
80	78·84	82·20	—3·36
90	78·01	80·05	—2·04
100	77·89

Section 4. Capillary Attraction.

This was estimated by carefully observing the heights to which the several mixtures rose in a capillary tube 0·584 millim. in diameter.

These heights were measured by means of a telescope and a millimetre-scale etched on a glass rod. This glass rod was fixed to the capillary tube, and terminated at its lower extremity in a point, which was made just to touch the surface of the liquid.

Several precautions were necessary to render the measurements accurate. The results are contained in the following Table :—

Percentage of alcohol, by weight.	Height, assuming water = 100 millims.	Relative molecular attraction.	Height calculated.	Difference.
0	100·00	100·00	100·00
10	69·17	68·07	93·11	—25·04
20	56·43	54·83	86·22	—31·39
30	48·19	46·15	79·34	—33·19
40	45·30	42·56	72·45	—29·89
45	43·74	40·64	69·00	—28·36
50	42·93	39·43	65·56	—26·13
60	42·30	37·89	58·68	—20·79
70	41·76	36·42	51·79	—15·37
80	41·29	35·03	44·90	— 9·87
90	40·54	33·35	38·02	— 4·67
100	39·21	31·13	31·13

The third column gives the length of a column of water equal in weight

* Calculated on the assumption that the alcohol and water in a mixture have an influence on the boiling-point of the mixture proportional to their respective weights.

to the thread of alcoholic mixture contained in the second column, and gives, therefore, a measure of the relative strength of the molecular attraction in the various mixtures.

The experiments were made at a temperature of 16° C.

Section 5. *Rate of Expansion.*

This was determined by estimating the specific gravity of the different mixtures at the temperatures 10° C., 15°·5 C., 20° C.

The specific-gravity bottle has two necks; into one was fitted a thermometer with a long bulb, whilst the other ended in a capillary tube.

This bottle was placed in a water-bath, whose temperature was under perfect control, and thus the specific gravity could be accurately estimated at the above-named temperatures.

Section 6. *Compressibility.*

This property was estimated by an apparatus similar to the one employed by Regnault and Grassi, but of simpler construction.

The piezometer was of glass; pressure was applied to the inside and outside by forcing air into the apparatus by means of a small pump; 0·000002 was always added as a correction for the compressibility of the piezometer.

The two following Tables give the results obtained in Sections 5 and 6.

Percentage of alcohol, by weight.	Volume at 10° C.	Volume at 20° C., found.	Volume at 20° C., calculated.	Difference.
0	100	100·154	100·154
10	100	100·212	100·272	—·060
20	100	100·405	100·386	+·019
30	100	100·632	100·498	+·134
40	100	100·783	100·601	+·182
45	100	100·827	100·652	+·175
50	100	100·868	100·700	+·168
59·77	100	100·914	100·789	+·125
69·73	100	100·980	100·874	+·106
79·81	100	101·020	100·954	+·066
89·89	100	101·052	101·034	+·018
100·00	100	101·088	101·088

Percentage of alcohol, by weight.	Compressibility for one atmosphere, found.	Compressibility for one atmosphere, calculated.	Difference.
0	0·00004774	0·00004774
10	0·00004351	0·00005387	0·00001036
20	0·00003911	0·00005998	0·00002087
30	0·00003902	0·00006584	0·00002682
40	0·00004347	0·00007118	0·00002771
45	0·00004608	0·00007366	0·00002758
50	0·00004878	0·00007600	0·00002722
59·77	0·00005620	0·00008029	0·00002409
69·73	0·00006159	0·00008426	0·00002267
78·81	0·00006942	0·00008775	0·00001833
89·89	0·00007950	0·00009140	0·00001190
100·00	0·00009349	0·00009349

Weight of water contained in the piezometer 114.9727 grms.

In conclusion the authors confine themselves to pointing out certain relations which connect the various physical properties examined.

These properties may be divided into two classes, according as they reach a maximum deviation from the theoretical mean at 30 per cent. or 40 per cent.; each of these is divided into two subclasses, one containing those properties in which the numbers found are above those calculated, and the other containing those in which they are below.

Class I.

Subclass *a*. Specific heat.

Heat produced by mixing.

„ *b*. Boiling-point.

Capillary attraction.

Class II.

Subclass *c*. Rate of expansion.

„ *d*. Compressibility.

Other characters, examined by previous investigators, are :—

1. *Vapour-tension* : this falls under Class I. Subclass *b*.

2. *Specific Gravity*.

3. *Index of Refraction*.

The two latter form a new class, coming to a maximum deviation from their theoretical value at 45 per cent.

In subclass *a*, specific heat—by reference to the Tables given, it will be seen that the first addition of alcohol to water (though alcohol has a specific heat much lower than that of water) produces mixtures which have a higher specific heat than water, and that a mixture containing between 30 and 40 per cent. alcohol has the same specific heat as water.

Similarly alcohol, though much more compressible than water, yet, when added to it, forms mixtures less compressible than water; so that a mixture containing between 45 and 50 per cent. alcohol has the same compressibility as water.

The rate of expansion is remarkable, as, starting from water, it at first is below the theoretical value, then rises; at 17 to 18 per cent. the rate of expansion is identical with the calculated expansion; for all mixtures stronger than this, the rate of expansion is constantly above that calculated.

The whole of the physical characters of mixtures of alcohol and water come to a maximum deviation from their theoretical values somewhere between 30 per cent. and 45 per cent. alcohol by weight. The 30 per cent. nearly corresponds to the formula $C_2H_6O + 6OH_2$ (=29.87 per cent.);

the 45 per cent. has approximately the formula $C_2 H_6 O + 3 O H_2$ (=46 per cent.).

Some of the physical properties examined seem to be especially connected with each other; these are:—

1. Specific heat and heat produced by mixing; for by dividing the number of units of heat evolved by 5 grammes of any mixture by 3·411, the elevation of the specific heat of such mixture above the theoretical specific heat is obtained.
2. Boiling-point and capillary attraction; by dividing the depression of the capillary attraction by 3·6, the depression of the boiling-point is obtained.

Deville & Hoek have shown the specific gravity and index of refraction to be connected with each other (Ann. de Chim. et de Physique, 3rd ser. vol. v. Pogg. Ann. vol. cxii.).

Whether the relations thus established between the various physical properties of alcoholic mixtures hold good with other similar substances, or whether these mixtures form a singular exception, must be decided by further research.

March 18, 1869.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President,
in the Chair.

The following communications were read:—

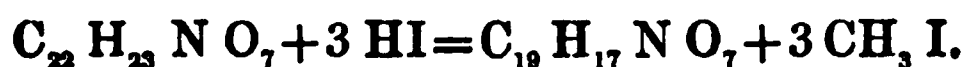
- I. “Researches into the Chemical Constitution of Narcotine, and of its Products of Decomposition.”—Part III. By A. MATTHIESSEN, F.R.S., Lecturer on Chemistry in St. Bartholomew’s Hospital. Received February 18, 1869.

(Abstract.)

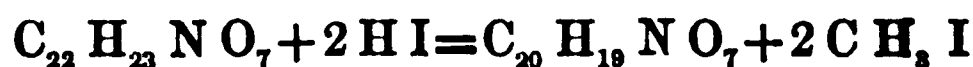
In this part the preparation is described of two new bases derived from narcotine.

1. *On the Action of Hydriodic Acid on Narcotine.*—When narcotine is heated with fuming hydriodic acid, iodide of methyl is evolved, and on investigating the residue it is found to consist of the iodide of a new base.

In two experiments made with 50 grms. of narcotine, 45·7 and 46·2 grms. of iodide of methyl, and in a third experiment with 100 grms. of narcotine, 91·8 grms. of iodide of methyl, were obtained, 51·5 grms. and 103·1 grms. being the theoretical quantity required for the following reaction:—



If the reaction



took place, the theoretical quantity of iodide would only be 34.3 grms. and 68.7 respectively.

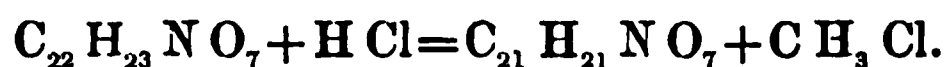
The endeavours to obtain the base in a state fit for analysis have been fruitless, owing to its oxidizing rapidly when exposed to the air; to establish its composition, the chloride was analyzed, and led to the following result:—



The base itself is, when newly precipitated, nearly white, but as soon as it is exposed to the air it becomes almost black; it is soluble in carbonate of sodium, caustic soda, potash or ammonia, slightly soluble in hot alcohol, quite insoluble in ether, and nearly so in water. All endeavours to obtain it or its salts in a crystalline state have hitherto failed.

The base may be called normal narcotine, or, shorter, nornarcotine, as it contains, in all probability, normal meconin combined with cotarnimide.

2. *On the Action of Hydrochloric Acid on Narcotine.*—When narcotine is heated with hydrochloric acid for about two hours, chloride of methyl is evolved, and on examining the residue it will be found to contain the chloride of a new base. The reaction which takes place is simply that one atom of methyl in the narcotine is replaced by one of hydrogen; thus:—



The pure base forms a white amorphous powder, almost insoluble in water and ether, very soluble in alcohol. Its salts, like those of the other bases derived from narcotine, are, as far as they have been prepared, amorphous. The base may be called dimethyl-normal-narcotine, or, shorter, dimethyl-nor-narcotine.

In the annexed Table the properties and reactions of the narcotine bases are given side by side.

Neither of the above bases has any marked physiological effects; for in working with them, as well as in taking grain doses, no ill effects have been observed. It is worthy of notice that the taste of the chlorides varies so markedly by the replacement of one atom of methyl by one of hydrogen.

II. "Researches into the Chemical Constitution of Narcotine, and of its Products of Decomposition."—Part IV. By AUGUSTUS MATTHIESSEN, F.R.S., Lecturer on Chemistry in St. Bartholomew's Hospital, and C. R. A. WRIGHT, B.Sc. London. Received February 18, 1869.

(Abstract.)

In Section I. of this memoir some new reactions of narcotine are described.

A. When narcotine is submitted to the action of water, either boiling in open vessels or at temperatures above 100° C. in sealed tubes, it splits up into meconin and cotarnine.



The splitting up of narcotine under the influence of heated water may explain the occurrence of meconin in opium-residues, as probably the small amount of meconin always found there is simply due to the partial decomposition of the narcotine during the processes of extraction of morphia.

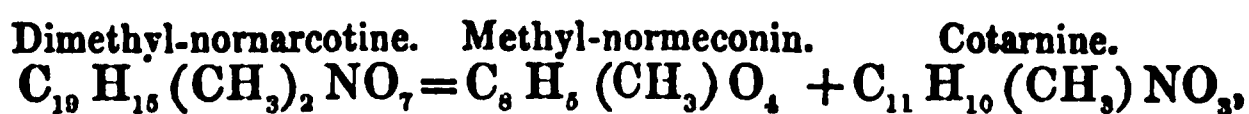
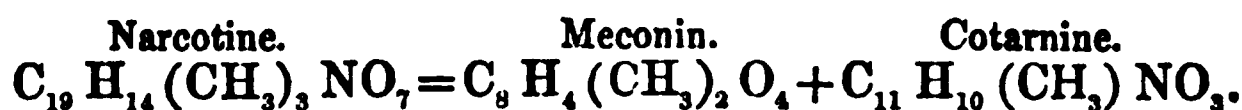
B. Narcotine heated *per se* to a little above 200° splits up as above into meconin and cotarnine, the latter being immediately decomposed at that temperature.

C. When hydrochlorate of narcotine is heated along with ferric chloride solution, the latter is reduced and the narcotine converted into opianic acid and cotarnine.



Section II. treats of the decompositions of the narcotine-bases.

A. Dimethyl-nornarcotine, when heated to above 100° C. with water in sealed tubes, undergoes decomposition: from the corresponding narcotine reaction it would seem that this decomposition might take place in either of two ways:—

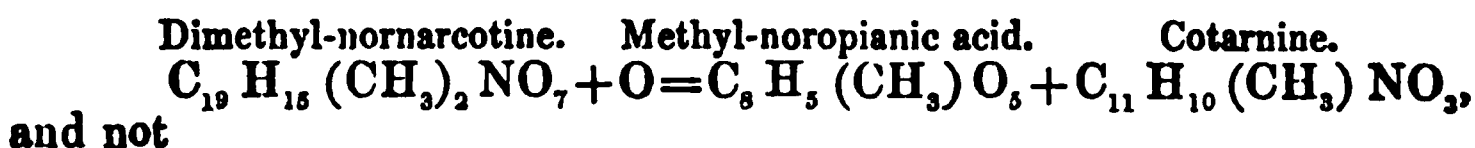


or



Of these the former reaction is apparently the one which thus takes place.

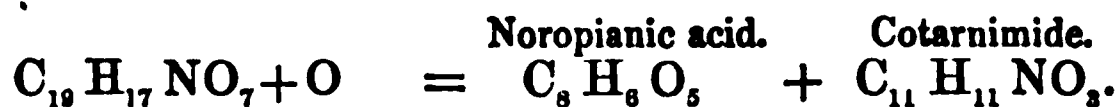
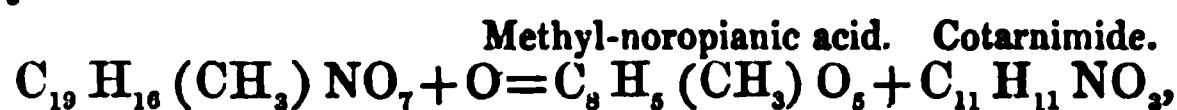
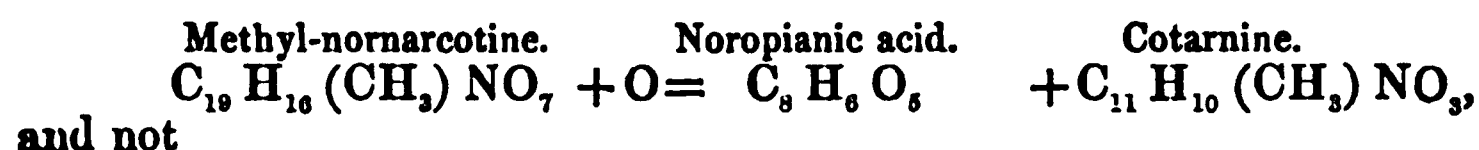
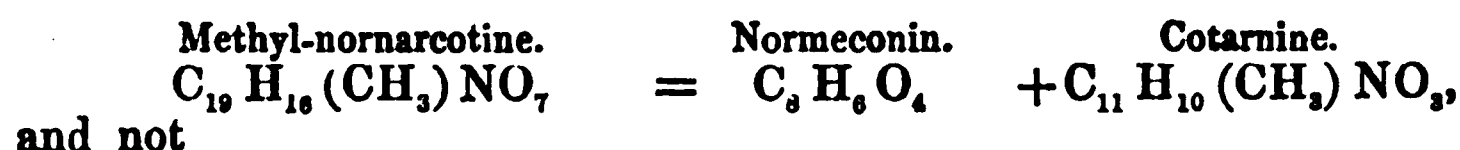
This conclusion is borne out by the fact that, when treated with ferric or platinic chloride, the hydrochlorate of dimethyl-nornarcotine forms methyl-noropianic acid and cotarnine, and not opianic acid and cotarnimide.



and not

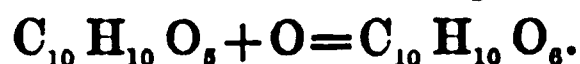


B. From reasons given in the memoir, the reactions of methyl-nornarcotine and nornarcotine with heated water and oxidizing agents are as follows :—



Section III. contains some miscellaneous observations on opianic acid, meconin, and hemipinic acid.

A. Opianic acid treated with sulphuric acid and dilute solution of bichromate of potassium becomes oxidized to hemipinic acid.



When heated a few degrees above its melting-point, opianic acid loses water and yields a substance crystallizable from hot alcohol, differing in properties from opianic acid, and apparently containing $\text{C}_{40}\text{H}_{38}\text{O}_{19}$, being formed thus :—



B. All attempts to oxidize meconin to opianic or hemipinic acid were failures.

Nitrous acid gas passed into melted meconin caused the formation of nitromeconin, identical with that got by the action of nitric acid, each sample, however, giving rather different qualitative reactions from those usually ascribed to this substance.

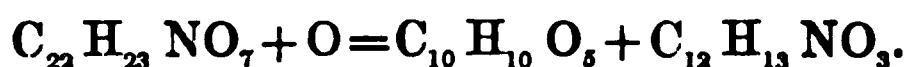
C. Hemipinic acid, when heated to 170° , loses water and becomes an anhydride, $\text{C}_{10}\text{H}_8\text{O}_5$, which may be crystallized unaltered from absolute alcohol, but when treated with ordinary spirit of 90 per cent. alcohol forms ethyl-hemipinic acid, $\text{C}_{10}\text{H}_9(\text{C}_2\text{H}_5)\text{O}_5$.

Résumé of results obtained in the four portions of this research.*

(1) It has been shown from the analyses of various samples of narcotine derived from various sources, that narcotine has always the same composition, viz. $\text{C}_{22}\text{H}_{23}\text{NO}_7$ (vol. xii. p. 501).

* Parts I. & II. by Professor G. C. Foster and one of us, Proc. Roy. Soc. vol. xi. p. 55; xii. p. 501; xvi. p. 39. Part III. Proc. Roy. Soc. vol. xvii. p. 337. Part IV. vol. xvii. p. 340.

(2) As stated by former observers, narcotine under the influence of oxidizing agents splits up into opianic acid and cotarnine.



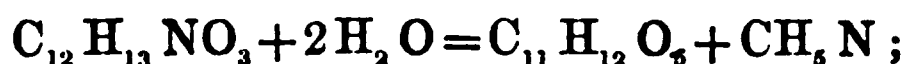
(3) When heated to a little above 200° *per se*, or for a considerable time in contact with water, narcotine splits up into meconin and cotarnine (vol. xvii. p. 340).



(4) When narcotine is heated with excess of hydrochloric acid for a short time (about two hours), chloride of methyl is formed, and one atom of H substituted for CH_3 in the narcotine; if heated for a long time (some days), two atoms of H are substituted for two of CH_3 ; when heated with fuming hydriodic acid, iodide of methyl is formed in such quantities as to prove that three atoms of H are substituted for three of CH_3 . A series of homologous bases is thus formed, whose decompositions are analogous to those of narcotine.

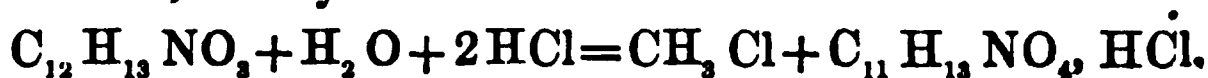
(5) Cotarnine has been shown to have the formula $C_{12}H_{13}NO_3$, and not $C_{13}H_{13}NO_3$, and is capable of crystallizing with half a molecule, and with a whole molecule, of water of crystallization.

(6) When cotarnine is heated with dilute nitric acid, under certain not clearly understood circumstances, cotarnic acid and methylamine is produced,



with strong nitric acid, as stated by previous observers, apophyllic acid is produced; other oxidizing agents give no definite results (vol. xi. p. 59).

(7) When cotarnine is heated with strong hydrochloric acid, chloride of methyl is formed, and hydrochlorate of cotarnamic acid.

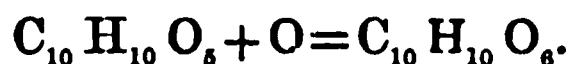


Hydriodic acid produces a similar reaction, only *one* equivalent of CH_3 being removed for one of cotarnine (vol. xii. p. 503).

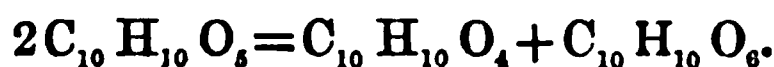
(8) Opianic acid under the influence of nascent hydrogen (as when treated with sodium-amalgam or zinc and sulphuric acid) is reduced to meconin (vol. xii. p. 503).



(9) Opianic acid heated with bichromate of potassium and dilute sulphuric acid becomes oxidized to hemipinic acid (vol. xvii. p. 341).

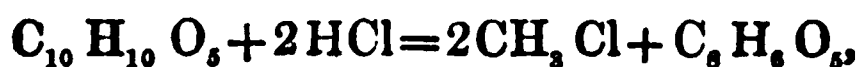


(10) Opianic acid heated with caustic potash splits up into meconin and hemipinic acid (vol. xi. p. 57).



(11) Opianic acid heated with excess of hydrochloric acid forms chloride of methyl, hydrogen being substituted for CH_3 in the opianic acid: it appears probable that two distinct substances are thus produced, noropianic acid and methyl-noropianic acid—the former by substitution of H_2 for

$(\text{CH}_3)_2$, and the latter by substitution of H for CH_3 ; only the latter has been isolated in a pure state, the former decomposing spontaneously.

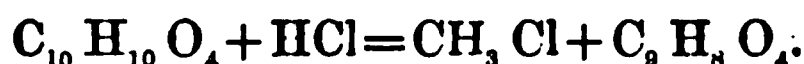


Hydriodic acid apparently produces similar decompositions.

Like opianic acid, methyl-noropianic acid is monobasic (vol. xvi. p. 39).

(12) All experiments to oxidize meconin to opianic acid or hemipinic acid or any other product have proved failures.

(13) Meconin treated with excess of hydrochloric or hydriodic acid forms chloride or iodide of methyl, and a body derived from meconin by substitution of H for CH_3 , methyl-normeconin.



Attempts to procure (hypothetical) normeconin by substituting H₂ for $(\text{CH}_3)_2$ did not yield anything capable of isolation in a pure state (vol. xvi. p. 39).

(14) Hemipinic acid treated with various reducing agents has in no case been reduced to opianic acid or meconin; nor have experiments to form opianic acid by the union of hemipinic acid and meconin been successful; nor has hemipinic acid been oxidized to any other compound.

(15) When hemipinic acid is heated with excess of hydrochloric acid, chloride of methyl and carbonic acid are formed, together with a new acid, methyl-hypogallic acid, in accordance with the following equation:—



When heated with hydriodic acid, hypogallic acid is found, together with iodide of methyl and carbonic acid: thus,



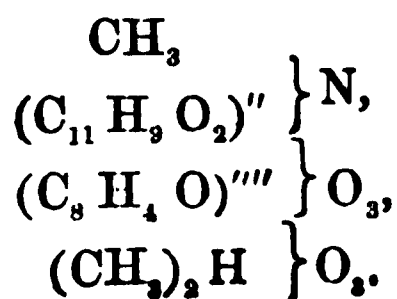
(16) The observations of Anderson, that hemipinic acid is bibasic, have been confirmed, and an anhydride obtained by simple desiccation (vol. xvii. p. 341).



Methyl-hypogallic acid, however, is monobasic (vol. xvi. p. 40).

(17) Hemipinic acid is capable of crystallizing with different amounts of water of crystallization, crystals with half a molecule, with a whole molecule, and with two molecules of water having been obtained (vol. xvi. p. 40).

(18) All the reactions of narcotine and of its products of decomposition may be satisfactorily accounted for by the following rational formula:—



III. "On the Corrections of Bouvard's Elements of Jupiter and Saturn (Paris, 1821)." By HUGH BREEN, formerly of the Royal Observatory, Greenwich. Communicated by Professor G. G. STOKES, Sec. R. S. Received December 17, 1868.

The Tables of Jupiter and Saturn which have been used for some years past in the computations of the 'Berliner Jahrbuch' and 'Nautical Almanac,' differ more from observation than is consistent with the present requirements of astronomy; and, moreover, abundant means for the correction of Bouvard's 'Elements' exist in the publication of the Greenwich Planetary Observations, 1750-1835, and the annual volumes issued from the Royal Observatory since 1836. The present work, which has been undertaken for this purpose, is based exclusively on the Greenwich Observations, 1750-1865.

Each mean group of observations in the Greenwich Planetary Reductions &c. gives the mean error of the planet's tabular geocentric place, with its equivalent in terms of the heliocentric errors of the earth and planet; but in the present investigation the places of Carlini's Solar Tables, which have been used throughout the whole period (with the exception of 1864 and 1865), have been accepted without alteration; for Jupiter and Saturn the factors of the earth's heliocentric errors are so small, that the difference of Carlini's Solar Tables from the recent investigations of Leverrier may be neglected.

The coefficients of the errors of the elements in heliocentric longitude and radius vector, for different values of the mean anomaly, are calculated in the usual way; and the formation of the equations of condition is effected by their multiplication by the printed factors of the heliocentric errors of the planet in the Greenwich Observations. A weight is assigned to each equation of condition, dependent on the number of observations in the group, and the relation of the geocentric and heliocentric errors. The equations thus, multiplied by the weights, are then solved by the method of least squares. The results are given in the following Table:—

Jupiter.

1750, October 29, to 1771, July 14.

$$\delta a = - 0.000331873.$$

$$\delta e = + 0.00000123252.$$

$$\delta t = - 4''.284354.$$

$$\delta \pi = - 22''.36544.$$

$$\delta I = - 0''.311.$$

$$\delta N = + 99''.1819 \text{ (neglecting } \delta l \text{ as insensible).}$$

δa is the error of the planet's semiaxis major, δe is the error of the eccentricity, δt is the error of the epoch of the mean longitude, and $\delta \pi$ is the error of the longitude of the perihelion, δI is the error of the inclination, and δN is the error of the longitude of the node.

1772, August 31, to 1810, January 9.

$$\begin{aligned}\delta a &= - 0.000181527. \\ \delta e &= - 0.00000211230. \\ \delta t &= - 1''.50080. \\ \delta \pi &= - 41''.7566. \\ \delta I &= - 0''.561. \\ \delta N &= + 24''.911.\end{aligned}$$

1811, February 12, to 1839, May 30.

$$\begin{aligned}\delta a &= - 0.0000355943. \\ \delta e &= + 0.00000126876. \\ \delta t &= - 2''.94891. \\ \delta \pi &= - 58''.9578. \\ \delta I &= - 1''.433. \\ \delta N &= - 72''.0634.\end{aligned}$$

1840, January 18, to 1865, August 8.

$$\begin{aligned}\delta a &= - 0.000166480. \\ \delta e &= - 0.00000677360. \\ \delta t &= - 4''.88982. \\ \delta \pi &= - 77''.3245. \\ \delta I &= - 1''.668. \\ \delta N &= - 118''.266.\end{aligned}$$

Saturn.

The tabular results of the 'Nautical Almanac' and 'Berlin Ephemeris' have been reduced to the value of the mass of Jupiter adopted in the Greenwich Planetary Reductions, 1750–1830; and the equations are formed as before mentioned.

1751, February 19, to 1783, September 28.

$$\begin{aligned}\delta a &= + 0.00048429. \\ \delta e &= - 0.000035957. \\ \delta t &= - 7''.86558. \\ \delta \pi &= + 214''.9774. \\ \delta I &= - 10''.7538. \\ \delta N &= - 157''.156.\end{aligned}$$

1784, July 12, to 1814, July 19.

$$\begin{aligned}\delta a &= + 0.0000371094. \\ \delta e &= - 0.00000436038. \\ \delta t &= - 4''.38974. \\ \delta \pi &= + 121''.9323. \\ \delta I &= - 9''.046. \\ \delta N &= + 107''.67.\end{aligned}$$

1815, July 29, to 1839, July 13.

$$\begin{aligned}
 \delta a &= + 0.00081572. \\
 \delta e &= + 0.000000334917. \\
 \delta t &= - 6''.71499. \\
 \delta \pi &= + 40''.71125. \\
 \delta I &= - 10''.418. \\
 \delta N &= + 95''.207.
 \end{aligned}$$

1840, March 9, to 1865, June 9.

$$\begin{aligned}
 \delta a &= + 0.00076325. \\
 \delta e &= + 0.0000286012. \\
 \delta t &= - 2''.89008. \\
 \delta \pi &= - 3''.47275. \\
 \delta I &= - 11''.233. \\
 \delta N &= + 38''.16.
 \end{aligned}$$

IV. "On the Structure of the Red Blood-corpuscle of Oviparous Vertebrata." By WILLIAM S. SAVORY, F.R.S. Received February 20, 1869.

The red blood-cell has been perhaps more frequently and fully examined than any other animal structure; certainly none has evoked such various and even contradictory opinions of its nature. But without attempting here any history of these, it may be shortly said that amongst the conclusions now, and for a long time past, generally accepted, a chief one is that a fundamental distinction exists between the red corpuscle of Mammalia and that of the other vertebrate classes—that the red cell of the oviparous vertebrata possesses a nucleus which is not to be found in the corpuscle of the other class. This great distinction between the classes has of late years been over and over again laid down in the strongest and most unqualified terms.

But I venture to ask for a still further examination of this important subject.

As the oviparous red cell is commonly seen, there can be no doubt whatever about the existence of a "nucleus" in its interior. It is too striking an object to escape any eye; but I submit that its existence is due to the circumstances under which the corpuscle is seen, and the mode in which it is prepared for examination. I think it can be shown that the so-called nucleus is the result of the changes which the substance of the corpuscle undergoes after death (and which are usually hastened and exaggerated by exposure), and the disturbance to which it is subjected in being mounted for the microscope. When a drop of blood is prepared for examination, little or no attention is given to the few seconds, more or less, which are consumed in the manipulation. It is usually either pressed or spread out on the glass slip, and

often mixed with water or some other fluid. But it is possible to place blood-cells under the microscope for examination so quickly, and with such slight disturbance, that they may be satisfactorily examined before the nuclei have begun to form. They may then be shown to be absolutely structureless throughout; and, moreover, as the examination is continued the gradual formation of the nuclei can be traced. The chief points to be attended to are—to mount a drop of blood as quickly as possible, to avoid as much as possible any exposure to air, to avoid as much as practicable contact of any foreign substance with the drop, or any disturbance of it.

After many trials of various plans, I find that the following will often succeed sufficiently well. Having the microscope, and everything else which is required, conveniently arranged for immediate use, an assistant secures the animal which is to furnish the blood (say, a frog or a newt), in such a way that the operator may cleanly divide some superficial vessel, as the femoral or humeral artery. He then instantly touches the drop of blood which exudes with the under surface of the glass which is to be used as the cover, immediately places this very lightly upon the slide, and has the whole under the microscope with the least possible delay. Thus for several seconds the blood-cells may be seen without any trace of nuclei; then, as the observation is continued, these gradually, but at first very faintly, appear; and the study of their formation affords strong proof of their absence from the living cells.

The “nucleus” first appears as an indistinct shadowy substance, usually, but not always, about the centre of the cell. The outline of it can hardly, for some seconds, be defined; but it gradually grows more distinct. Often some small portion of the edge appears clear before the rest. At the same time the nucleus is seen to be paler than the surrounding substance. Synchronously with this change—and this is noteworthy—the outline of the corpuscle (the “cell-wall”) becomes broader and darker. What was at first a mere edge of homogeneous substance, becomes at length a dark border sharply defined from the coloured matter within. Thus a corpuscle, at first absolutely structureless, homogeneous throughout, is seen gradually to be resolved into central substance or nucleus, external layer or cell-wall, and an intermediate, coloured though very transparent, substance. But—and this is significant—these changes are not always thus fully carried out. It not seldom happens that the nucleus does not appear as a central well-defined regularly oval mass. Sometimes it never forms so as to be clearly traced in outline, but remains as an irregular shapeless mass, in its greater portion very obscure. Sometimes only a small part, if any, of an edge can be recognized, most of it appearing to blend indefinitely with the rest of the cell-substance. Sometimes it happens that in many corpuscles the formation of a nucleus does not proceed even so far as this. No distinct separation of substance can anywhere be seen, but shadows, more or less deep, here and there indicate that there is greater aggregation of matter at some parts than at others. Occasionally some of the cells

present throughout a granular aspect. I have almost invariably observed, too, a relation between the distinctness of the nucleus and of the cell-wall. When the nucleus is well defined, the cell-wall is strongly marked; when one is confused, the other is usually fainter. This, however, does not apply to colour; on the contrary, when the nucleus is least coloured it contrasts most strongly with the surrounding cell. As a rule, the wall of the cell is more strongly marked than the nucleus.

It will of course be said that the nuclei are present all the while, but are at first concealed by the surrounding substance—the contents of the cell. Thus the fact has been accounted for, that the nuclei are not so obvious at first as they subsequently become. But I think a careful comparison of cells will show that those in which a nucleus may be traced are not more transparent than others which are structureless; and, moreover, when one cell overlaps another, the lower one is seen through the upper clearly enough to show that the substance of these cells is sufficiently transparent to allow of a nucleus being discerned if it exists. When a nucleus is fully formed, it hides that portion of the outline of a cell which lies beneath it. How is it, then, if the nucleus is present from the first, that the portion of the cell over which it subsequently appears is, for a while, plainly seen?

The success of the observation is of course influenced by numerous circumstances. The rate at which the nuclei form in the corpuscles varies in different animals. I have usually found that in the common frog they are more prone to form than in many other animals—quicker than in most fishes, or even than in some birds. But this does not seem always to depend upon their larger size; for in the common newt the cells, which are larger than those of the frog, remain, as I have noticed, for a longer period without any appearance of nuclei. But even in the frog it can be satisfactorily demonstrated that the corpuscle is structureless.

I have found, too, that the observation succeeds best with the blood of animals which are healthy and vigorous. Thus the first observations upon fresh animals are usually the most satisfactory. After they have been repeatedly wounded or have lost much blood, the cells are more prone to undergo the changes which result in the production of nuclei.

Again, the formation of nuclei may be hastened, and their appearance rendered more distinct at last, by various reagents. Acids and many other reagents are well known to have this effect. The addition of a small quantity of water acts in the same way, but less energetically. It hastens the appearance of an indistinct nucleus, but interferes with the formation of a well-defined mass, so that, after the addition of water, neither the outline of the cell nor of the nucleus becomes so strongly marked as it often does without it. Exposure to air also promotes their formation; indeed, as a rule, the nuclei form best under simple exposure. Any disturbance of the drop, as by moving the point of a needle in it, certainly hastens the change; and perhaps it is influenced by temperature.

Sometimes, when the drop of blood has been skilfully mounted, the majority of cells will remain for a long while without any trace of nucleus; but, again, in almost every specimen, the nucleus in some few of the cells, particularly in those nearest the edges, begins to appear so rapidly that it is hardly possible to run over the whole field without finding some cells with an equivocal appearance.

It would follow, of course, from these observations that, if the living blood were examined in the vessels, the corpuscle would show no trace of any distinction of parts; and this is so. Indeed, in my earlier observations*, before I had learnt to mount a drop of blood for observation in a satisfactory manner, I examined, at some length, blood in the vessels of the most transparent parts I could select; and several observations on the web and lung of the frog and elsewhere were satisfactory. But still, when the cells were thus somewhat obscured by intervening membrane, one could not generally feel sure that the observation was so clear and complete, but that a faintly marked nucleus might escape detection. While, therefore, the result of observations on blood-cells in the vessels fully accords with the description I have given, I do not think that the demonstration of the fact, that while living they have no nucleus, can be made so plain and unequivocal as when they are removed from the vessels.

The question naturally arises, Why, then, does not a nucleus form in the mammalian corpuscle? But while it is accepted that the great majority of these corpuscles exhibit no nuclei after death, excellent observers still affirm their occasional existence; and I am convinced that an indistinct, imperfectly formed "nucleus" is often seen; and the shadowy substance seen in many of the smaller oviparous cells after they have been mounted for some time is very like that seen under similar circumstances in some of the corpuscles of Mammalia. Many, too, affirm that these corpuscles do not exhibit that distinction of wall and contents which is generally described. It appears to me that this difference of opinion depends on the changes they are prone to undergo. How far the absence of a distinctly defined "nucleus" after death depends on their smaller size I am not prepared to say.

Many questions of course follow. For example, how far is this separation of the substance of a homogeneous† corpuscle into nucleus, cell-membrane, and contents to be compared to the coagulation of the blood? and how do the agents which are known to influence the one process affect the other? A still further and more important question is, How are these changes in the corpuscles, and in the blood around them, related? But in this paper I propose to go no further than the statement that the

* Made many years ago. Other observers have been unable to detect a nucleus in the living cells within the vessels.

† By the word homogeneous I do not mean to affirm that the substance of the corpuscle is of equal consistence throughout. The central may be the softest part of it. But I regard the corpuscle, in its whole substance, as "having the same nature."

V. "Spectroscopic Observations of the Sun
NORMAN LOCKYER, F.R.A.S. - COMMUN-
LAND, F.R.S. Received March 4, 1869.

Since my second paper under the above title was read before the Royal Society, the weather has been unfavourable to an almost unprecedented degree; and, as a consequence, the observations I have been enabled to make during the last few days have been much smaller than I had hoped it would be.

Fortunately, however, the time has not been wholly wasted of the weather; for, by the kindness of Dr. Frankland, I have had the interim to familiarize myself at the Royal College of Chemistry with the spectra of gases and vapours under previously untried conditions; in addition to the results already communicated to the Royal Society by Dr. Frankland and myself, the experience I have gained at the Royal College of Chemistry has guided me greatly in my observations of the Sun.

In my former paper it was stated that a diligent search for the third line of hydrogen in the spectrum of the chromosphere had been made with success. When, however, Dr. Frankland and I were enabled to observe that the pressure in the chromosphere even was small, it was found that the out of the hydrogen-lines was due in the main, if not wholly, to the pressure. I determined to seek for it again under better atmospheric conditions, and I succeeded after some failures. The position of the third line of Kirchhoff's scale. It is generally excessively faint, and is required to see it than is necessary in the case of the other lines. At least haze in the sky puts it out altogether.

Hence, then, with the exception of the bright yellow lines of the spectrum of the chromosphere, the

Dr. Frankland and myself have pointed out that, although the chromosphere and the prominences give out the spectrum of hydrogen, it does not follow that they are composed merely of that substance : supposing others to be mixed up with hydrogen, we might presume that they would be indicated by their selective absorption near the sun's limb. In this case the spectrum of the limb would contain additional Fraunhofer lines. I have pursued this investigation to some extent, with, at present, negative results ; but I find that special instrumental appliances are necessary to settle the question, and these are now being constructed.

If we assume, as already suggested by Dr. Frankland and myself, that no other extensive atmosphere besides the chromosphere overlies the photosphere, the darkening of the limb being due to the general absorption of the chromosphere, it will follow :—

- I. That an additional selective absorption near the limb is extremely probable.
- II. That the hydrogen Fraunhofer lines indicating the absorption of the outer shell of the chromosphere will vary somewhat in thickness : this I find to be the case to a certain extent.
- III. That it is not probable that the prominences will be visible on the sun's disk.

In connexion with the probable chromospheric darkening of the limb, an observation of a spot on February 20th is of importance. The spot observed was near the limb, and the absorption was much greater than anything I had seen before ; so great, in fact, was the *general* absorption, that the several lines could only be distinguished with difficulty, except in the very brightest region. I ascribe this to the greater length of the absorbing medium in the spot itself in the line of sight, when the spot is observed near the limb, than when it is observed in the centre of the disk—another indication of the great general absorbing power of a comparatively thin layer, on rays passing through it obliquely.

I now come to the selective absorption in a spot. I have commenced a map of the spot-spectrum, which, however, will require some time to complete. In the interim, I may state that the result of my work up to the present time in this direction has been to add magnesium and barium to the material (sodium) to which I referred in my paper in 1866, No. I. of the present series ; and I no longer regard a spot simply as a cavity, but as a place in which principally the vapours of sodium, barium, and magnesium (owing to a downrush) occupy a lower position than they do ordinarily in the photosphere.

I do not make this assertion merely on the strength of the lines observed to be thickest in the spot-spectrum, but also upon the following observations on the chromosphere made on the 21st and 28th ultimo.

On both these days the brilliancy of the F line taught me that something unusual was going on ; so I swept along the spectrum to see if any materials were being injected into the chromosphere.

On the 21st I caught a trace of magnesium ; but it was late in the day, and I was compelled to cease observing by houses hiding the sun.

On the 28th I was more fortunate. If anything, the evidences of intense action were stronger than on the 21st, and after one glance at the F line I turned at once to the magnesium lines. I saw them appearing short and faint at the base of the chromosphere. My work on the spots led me to imagine that I should find sodium-vapour associated with the magnesium ; and on turning from *b* to D I found this to be the case. I afterwards reversed barium in the same way. The spectrum of the chromosphere seemed to be full of lines, and I do not think the three substances I have named accounted for all of them. The observation was one of excessive delicacy, as the lines were short and *very thin*. The prominence was a small one, about twice the usual height of the chromosphere ; but the hydrogen lines towered high above those due to the newly injected materials. The lines of magnesium extended perhaps one-sixth of the height of the F line, barium a little less, and sodium least of all.

We have, then, the following facts :—

- I. The lines of sodium, magnesium, and barium, when observed in a spot, are thicker than their usual Fraunhofer lines.
- II. The lines of sodium, magnesium, and barium, when observed in the chromosphere, are thinner than their usual Fraunhofer lines.

A series of experiments bearing upon these observations is now in progress at the College of Chemistry, and will form the subject of a communication from Dr. Frankland and myself. I may at once, however, remark that we have here additional evidence of a fact I asserted in 1865 on telescopic evidence—the fact, namely, that a spot is the seat of a downrush, a downrush to a region, as we now know, where the selective absorption of the upper strata is different from what it would be (and, indeed, is elsewhere) at a higher level.

Messrs. De La Rue, Stewart, and Loewy, who brought forward the theory of a downrush about the same time as my observations were made in 1865, at once suggested as one advantage of this explanation that all the gradations of darkness, from the faculæ to the central umbra, are thus supposed to be due to the same cause, namely, the presence to a greater or less extent of a relatively cooler absorbing atmosphere. This I think is now spectroscopically established ; we have, in fact, two causes for the darkening of a spot :—

- I. The general absorption of the chromosphere, thicker here than elsewhere, as the spot is a cavity.
- II. The greater selective absorption of the lower sodium, barium, magnesium stratum, the surface of its last layer being below the ordinary level.

Messrs. De La Rue, Stewart, and Loewy also suggested, in their ‘*Researches on Solar Physics*,’ that if the photosphere of the sun be the plane of condensation of gaseous matter, the plane may be found to be subject to

periodical elevations and depressions, and that at the epoch of minimum sun-spot-frequency the plane might be uplifted very high in the solar atmosphere, so that there was comparatively little cold absorbing atmosphere above it, and therefore great difficulty in forming a spot.

This suggestion is one of great value ; and, as I pointed out in my previous paper, its accuracy can fortunately now be tested. It may happen, however, that in similar periodical fluctuations the chromosphere may be carried up and down with the photosphere ; and I have already evidence that possibly such a state of things may have occurred since 1860, for I do not find the C and F Fraunhofer lines of the same relative thickness as they were in that year*. I am waiting to make observations with the large Steinheil spectroscope before I consider this question settled. But the well-known great thickness of the F line in Sirius and other stars will point out the excessive importance of such observations as a method of ascertaining not only the physical constitution, but the actual pressures of the outer limits of stellar atmospheres, and of the same atmosphere at different epochs. And when other spectra have been studied as we have now studied hydrogen, additional means of continuing similar researches will be at our command ; indeed a somewhat careful examination of the spectra of the different classes of stars, as defined by Father Secchi, leads me to believe that several broad conclusions are not far to seek ; and I hope soon to lay them before the Royal Society.

For some time past I have been engaged in endeavouring to obtain a sight of the prominences, by using a very rapidly oscillating slit ; but although I believe this method will eventually succeed, the spectroscope I employ does not allow me to apply it under sufficiently good conditions, and I am not at present satisfied with the results I have obtained.

Hearing, however, from Mr. De La Rue, on February 27th, that Mr. Huggins had succeeded in anticipating me by using absorbing media and a wide slit (the description forwarded to me is short and vague), it immediately struck me, as possibly it has struck Mr. Huggins, that the wide slit is quite sufficient without any absorptive media ; and during the last few days I have been perfectly enchanted with the sight which my spectroscope has revealed to me. The solar and atmospheric spectra being hidden, and the image of the wide slit alone being visible, the telescope or slit is moved slowly, and the strange shadow-forms flit past. Here one is reminded, by the fleecy, infinitely delicate cloud-films, of an English hedgerow with luxuriant elms ; here of a densely intertwined tropical forest, the intimately interwoven branches threading in all directions, the prominences generally expanding as they mount upwards, and changing slowly, indeed almost imperceptibly. By this method the smallest details of the pro-

* I have learnt, after handing this paper in to the Royal Society, that in Ångström's Map the C and F lines are nearly of the same breadth : this I had gathered from observations made with my own spectroscope.

minences and of the chromosphere itself are rendered perfectly visible and easy of observation.

ADDENDUM.—Received March 17, 1869.

Since the foregoing paper was written, I have had, thanks to the somewhat better weather, some favourable opportunities for continuing two of the lines of research more especially alluded to in it; I refer to the method I had adopted for viewing the prominences, and to the injection of sodium, magnesium, &c. into the chromosphere.

With regard to seeing the prominences, I find that, when the sky is free from haze, the views I obtain of them are so perfect that I have not thought it worth while to remount the oscillating slit. I am, however, collecting red and green and violet glass, of the required absorptions, to construct a rapidly revolving wheel, in which the percentages of light of each colour may be regulated. In this way I think it possible that we may in time be able to see the prominences as they really are seen in an eclipse, with the additional advantage that we shall be able to see the sun at the same time, and test the connexion or otherwise between the prominences and the surface-phenomena.

Although I find it generally best for sketching-purposes to have the open slit in a radial direction, I have lately placed it at a tangent to the limb, in order to study the general outline of the chromosphere, which in a previous communication I stated to be pretty uniform, while M. Janssen has characterized it as "*à niveau fort inégal et tourmenté.*" My opinion is now that perhaps the mean of these two descriptions is, as usual, nearer the truth, unless the surface changes its character to a large extent from time to time. I find, too, that in different parts the outline varies: here it is undulating and billowy; there it is ragged to a degree, flames, as it were, darting out of the general surface, and forming a ragged, fleecy, interwoven outline, which in places is nearly even for some distance, and, like the billowy surface, becomes excessively uneven in the neighbourhood of a prominence.

According to my present limited experience of these exquisitely beautiful solar appendages, it is generally possible to see the whole of their structure; but sometimes they are of such dimensions along the line of sight that they appear to be much denser than usual; and as there is no longer under these circumstances any background to the central portion, only the details of the margins can be observed, in addition to the varying brightnesses.

Moreover it does not at all follow that the largest prominences are those in which the intensest action, or the most rapid change, is going on,—the action as visible to us being generally confined to the regions just in, or above, the chromosphere, the changes arising from violent uprush or rapid dissipation, the uprush and dissipation representing the birth and death of a prominence. As a rule, the attachment to the chromosphere

is narrow and is not often single ; higher up, the stems, so to speak, intertwine, and the prominence expands and soars upward until it is lost in delicate filaments, which are carried away in floating masses.

Since last October, up to the time of trying the method of using the open slit, I had obtained evidence of considerable changes in the prominences from day to day. With the open slit it is at once evident that changes on the small scale are continually going on ; it was only on the 14th inst. that I observed any change at all comparable in magnitude and rapidity to those already observed by M. Janssen.

About 9^h 45^m on that day, with a tangential slit I observed a fine dense prominence near the sun's equator, on the eastern limb. I tried to sketch it with the slit in this direction ; but its border was so full of detail, and the atmospheric conditions were so unfavourable, that I gave up the attempt in despair. I turned the instrument round 90° and narrowed the slit, and my attention was at once taken by the F line ; a single look at it taught me that an injection into the chromosphere and intense action were taking place. These phenomena I will refer to subsequently.

At 10^h 50^m, when the action was slackening, I opened the slit ; I saw at once that the dense appearance had all disappeared, and cloud-like filaments had taken its place. The first sketch, embracing an irregular prominence with a long perfectly straight one, which I called A, was finished at 11^h 5^m, the height of the prominence being 1' 5", or about 27,000 miles. I left the Observatory for a few minutes ; and on returning, at 11^h 15^m, I was astonished to find that part of the prominence A had entirely disappeared ; not even the slightest rack appeared in its place : whether it was entirely dissipated, or whether parts of it had been wafted towards the other part, I do not know, although I think the latter explanation the more probable one, as the other part had increased.

We now come to the other attendant phenomena. First, as to the F line. In my second paper, under the above title, I stated that the F line widens as the sun is approached, and that sometimes the bright line seems to extend on to the sun itself, sometimes on one side of the F line, sometimes on the other.

Dr. Frankland and myself have pointed out, as a result of a long series of experiments, that the widening out is due to pressure, and apparently not to temperature *per se* ; the F line near the vacuum-point is thin, and it widens out on both sides (I do not say to the same extent) as the pressure is increased. Now, in the absence of any disturbing cause, it would appear that when the wider line shows itself on the sun on one side of the F line, it should at the same time show itself on the other ; this, however, *it does not always do*. I have now additional evidence to adduce on this point, and this time in the prominence line itself, off the sun. In the prominence to which I have referred, the F bright line underwent the most strange contortions, as if there were some disturbing cause which varied the refrangibility of the hydrogen-line under certain conditions and pressures.

under the magnesia. I carefully examined w
were visible in the spectrum of the chromospher

I also searched for the stronger barium-lines
the spectrum; but I did not find them, prob
elevation of the barium-vapour above the ger
sphere, which made the observation in this regio

I detected another chromosphere-line very ne
(on the east side of it).

The sodium-lines were also visible.

Unfortunately clouds prevented my continuing
tions; but the action was evidently toning down.

Here, then, we have an uprush of

Barium,
Magnesium,
? Nickel,

and an unknown substance
from the photosphere into the chromosphere, and
prominence; accompanying the uprush we have
magnitude in the prominence; and as the uprush
melts away.

As stated in the former part of this paper, the l
lines were thinner than the corresponding Fraun
nexion with this subject, I beg to be allowed to
menced a careful comparison of Kirchhoff's map
lished one of Ångström. From what I have alrea
important conclusions, in addition to that before all
from this comparison; but I hesitate to say more a
not been able to

“Note on the Blood-vessel-system of the Retina of the Hedgehog (being a fourth Contribution to the Anatomy of the Retina).”

By J. W. HULKE, F.R.S., Assistant-Surgeon to the Middlesex Hospital and the Royal London Ophthalmic Hospital.
Received May 26, 1868*.

The distribution of the retinal blood-vessels in this common British Insectivore is so remarkable that I deem it worthy of a separate notice—*only capillaries enter the retina.*

The vasa centralia pierce the optic nerve in the sclerotic canal, and, passing forwards through the lamina cribrosa, divide, at the bottom of a relatively large and deep pit in the centre of the intraocular disk of the nerve, into a variable number of primary branches, from three to six. These primary divisions quickly subdivide, furnishing many large arteries and veins, which, radiating on all sides from the nerve-entrance towards the ora retinæ, appear to the observer's unaided eye as strongly projecting ridges upon the inner surface of the retina. When vertical sections parallel to and across the direction of these ridges are examined with a quarter-inch objective, we immediately perceive that the arteries and veins lie, throughout their entire course, upon the inner surface of the membrana limitans interna retinæ, between this and the membrana hyaloidea of the vitreous humour, and that only capillaries penetrate the retina itself.

In sections of the retina across the larger vessels the membrana limitans may be seen as a clean distinctly unbroken line passing over the divided vessels, with which it does not appear to have any direct structural connexion. The relation of the hyaloidea to the large vessels seems to be more intimate, but its exact nature can be less certainly demonstrated, owing to the extreme tenuity of this membrane. In my best sections I saw the hyaloidea also crossing the large vessels, as does the limitans, but excessively delicate extensions of the hyaloidea appeared to me to lose themselves upon the vessels.

The capillaries, shortly after their origin, bend outwards away from the large vessels, and, piercing the retina vertically to its stratification in a direction more or less radial from the centre of the globe, and branching dichotomously in the granular and inner granule-layers, they form loops, the outermost of which reach the intergranule-layer. As they enter the retina the membrana limitans interna is prolonged upon the capillaries in the form of a sheath, which is wide and funnel-like at first, but soon embraces the vessels so closely as to become indistinguishable from their proper wall; so that, notwithstanding the existence of a sheath, there is no perivascular space about the retinal capillaries, such as His has described

* Read June 18, 1868: see Abstract, vol. xvi. p. 439.

channels the hyaloidea, and by the hi
in other reptiles and in birds. Thus it is poss
two classes, according as their retina is vascula
classes would be connected by the hedgehog, t
vasa centralia lying upon the membrana limita
the hyaloidea, represent the equivalent vessels c
forms so exquisite a microscopic object in the
vessels channelling the retinal tissues occupy th
do in most mammalia.

[The drawings in illustration of this paper are
the Archives of the Royal Society.]

**“ On the Measurement of the Luminous In
WILLIAM CROOKES, F.R.S. &c. Receiv**

The measurement of the intensity of a ray of li
tion of which has been repeatedly attempted, l
results than the endeavours to measure the ot
problem is susceptible of two divisions—the a
measurement of light.

I. Given a luminous beam, we may require
by some absolute term having reference to a st
previous time, and capable of being reproduced
and at any part of the globe. Possibly two s
necessary, differing greatly in value, so that the s
be subdivided into a definite number of equal p
might perhaps be obtained by the well-known de
rent intensities.

liminary researches and discoveries are yet to be made, before a photometer analogous to a thermometer in fixity of standard and facility of observation could be devised, the realization of an absolute light-measuring method appears somewhat distant. The path to be pursued towards the attainment of this desirable object appears to be indicated in the observations which from time to time have been made by M. Becquerel, Sir John Herschel, R. Hunt, and others, on the chemical action of the solar rays, and the production thereby of a galvanic current, capable of measurement on a delicate galvanometer, by appropriate arrangements of chemical baths and metallic plates connected with the ends of the galvanometer wires.

Many so-called photometers have been devised, by which the chemical action of the rays at the most refrangible end of the spectrum have been measured, and the chemical intensity of light tabulated by appropriate methods; and within the last few years Professors Bunsen and Roscoe have contrived a perfect chemical photometer, based upon the action of the chemical rays of light on a gaseous mixture of chlorine and hydrogen, causing them to combine with formation of hydrochloric acid.

But the measurement of the chemical action of a beam of light is as distinct from photometry proper as is the thermometric registration of the heat-rays constituting the other end of the spectrum. What we want is a method of measuring the intensity of those rays which are situated at the intermediate parts of the spectrum, and produce in the eye the sensation of light and colour; and, as previously suggested, there is a reasonable presumption that further researches may place us in possession of a photometric method based upon the chemical action of the *luminous* rays of light.

The rays which affect an ordinary photographic sensitive surface are so constantly spoken of and thought about as the ultra-violet invisible rays, that it is apt to be forgotten that some of the highly luminous rays of light are capable of exerting chemical action. Fifteen years ago* the writer was engaged in some investigations on the chemical action of light, and he succeeded in producing all the ordinary phenomena of photography, even to the production of good photographs in the camera, by purely luminous rays of light free from any admixture with the violet and invisible rays. When the solar spectrum (of sufficient purity to show the principal fixed lines) is projected for a few seconds on to a sensitive film of iodide of silver, and the latent image then developed, the action is seen to extend from about the fixed line G to a considerable distance into the ultra-violet invisible rays. When the same experiment was repeated with a sensitive surface of bromide of silver instead of iodide of silver, the result of the development of the latent image showed that, in this case, the action commenced at about the fixed line *b*, and extended, as in the case of the iodide of silver, far beyond the violet. A transparent cell, with parallel glass sides one inch across, was filled with a solution of twenty-five parts of sulphate of quinine

* The Journal of the Photographic Society, vol. i. p. 98.

mide and iodide of silver to the purely luminous latent images, it was now found that the action confined to a very narrow line of rays, close to the case of bromide of silver, to the space between the spaces of action by colours instead of fixed lines, behind a screen of sulphate of quinine, iodide of silver the luminous rays about the centre of the indigo whilst bromide of silver was affected by the green indigo rays.

It is very likely that a continuance of these experiments the construction of a photometer capable of measuring for although bromide of silver behind quinine is or yellow rays, still it is by the green and blue ; the red, yellow, green, and blue rays is always invariant the light would not be white, but coloured), a mixture set of the components of white light would give what is want—just as in an analysis of a definite chemical is satisfied with an estimation of one or two constituents relates the others.

Methods based upon the foregoing considerations what may be termed an *absolute* photometer, the intensity be always the same for the same amount of illumination standard light for comparison ; and pending the experiments which the writer is prosecuting in this direction to devise a new and, as he believes, a valuable photometer.

A relative photometer is one in which the observations are made upon the same substance under the same conditions.

obtaining uniform results with the Act-of-Parliament candle. A true sperm-candle is made from a mixture of refined sperm with a small proportion of wax, to give it a certain toughness, the pure sperm itself being extremely brittle. The wick is of the best cotton, made up into three cords and plaited. The number of strands in each of the three cords composing the wick of a six-to-the-pound candle is seventeen, although Mr. Sugg says there does not appear to be any fixed rule, some candles having more and others less, according to the quality of the sperm. Sperm-candles are made to burn at the rate of one inch per hour, and the cup should be clean, smooth, and dry. The wick should be curved slightly at the top, the red tip just showing through the flame, and consuming away without requiring snuffing. To obtain these results, the tightness of the plaiting and size of the wick require careful attention; and as the quality of the sperm differs in richness or hardness, so must the plaiting and number of strands. A variety of modifying circumstances thus tend to affect the illuminating power of a standard sperm-candle. These difficulties, however, are small compared with those which have resulted from the substitution of paraffin &c. for part of the sperm; and Mr. Sugg points out that candles can be made with such combinations of stearin, wax, or sperm, and paraffin, as to possess all the characteristics of sperm-candles and yet be superior to them in illuminating-power; while, on the other hand, candles made from the same materials otherwise combined are inferior. When, in addition to this, it is found that candles containing paraffin require wicks more tightly plaited and with fewer strands than those suitable for the true sperm-candle, our readers will be enabled to judge of the almost insurmountable difficulties which beset the present system of photometry.

But assuming that the true parliamentary sperm-candle is obtained, made from the proper materials, and burning at the specified rate, its illuminating-power will be found to vary with the temperature of the place where it has been kept, the time which has elapsed since it was made, and the temperature of the room wherein the experiment is tried.

The Rev. W. R. Bowditch, in his work on 'The Analysis, Purification &c. of Coal-gas,' enters at some length into the question of test-candles, and emphatically condemns them as light-measurers. One experiment quoted by this author showed that the same gas was reported to be 14.63 or 17.36 candle-gas, according to the way the experiment was conducted.

The present writer has taken some pains to devise a source of light which should be at the same time fairly uniform in its results, would not vary by keeping, and would be capable of accurate imitation at any time and in any part of the world by mere description. The absence of these conditions seems to be one of the greatest objections to the sperm-candle. It would be impossible for an observer on the continent, ten or twenty years hence, from a description of the sperm-candle now employed, to make a standard which would bring his photometric results into relation

with those obtained here. Without presuming to say that he has satisfactorily solved all difficulties, the writer believes that he has advanced some distance in the right direction, and pointed out the road for further improvement.

Before deciding upon a standard light, experiments were made to ascertain whether the electric current could be made available. Through a coil of platinum wire, so as to render it brightly incandescent, a powerful galvanic current was passed, and its strength was kept as constant as possible by a thick wire galvanometer and rheostat. To prevent the cooling action of air-currents, the incandescent coil was surrounded with glass; and it was hoped that by employing the same kind of battery, and by varying the resistance so as to keep the galvanometer-needle at the same deflection, uniform results could be obtained. In practice, however, it was found that many things interfered with the uniformity of the results, and the light being much feebler than it was advisable to work with, this plan was deemed not sufficiently promising, and it was abandoned.

The method ultimately decided upon is the following:—Alcohol of sp. gr. 0.805, and pure benzol boiling at 81° C., are mixed together in the proportion of 5 volumes of alcohol and 1 of benzol. This burning fluid can be accurately imitated from description at any future time and in any country; and if a lamp could be devised equally simple and invariable, the light which it would yield would, it is presumed, be invariable. This difficulty the writer has attempted to overcome in the following manner.

A glass lamp is taken of about two ounces capacity, the aperture in the neck being 0.25 inch diameter; another aperture at the side allows the liquid fuel to be introduced, and, by a well-known laboratory device, the level of the fluid in the lamp can be kept uniform. The wick-holder consists of a platinum tube 1.81 inch long and 0.125 inch internal diameter. The bottom of this is closed with a flat plug of platinum, apertures being left in the sides to allow free access of spirit. A small platinum cup 0.5 inch diameter and 0.1 inch deep is soldered round the outside of the tube 0.5 inch from the top, answering the threefold purpose of keeping the wick-holder at a proper height in the lamp, preventing evaporation of the liquid, and keeping out dust. The wick consists of fifty-two pieces of hard-drawn platinum wire, each 0.01 inch in diameter and 2 inches long, perfectly straight, and tightly pushed down into the platinum holder, until only 0.1 inch projects above the tube. The height of the burning fluid in the lamp must be sufficient to cover the bottom of the wick-holder: it answers best to keep it always at the uniform distance of 1.75 inch from the top of the platinum wick; a slight variation of level, however, has not been found to influence the light to an extent appreciable by our present means of photometry. The lamp with reservoir of spirit thus arranged, with the platinum wires parallel, and their projecting ends level, a light is applied, and the flame instantly appears, forming a perfectly shaped cone 1.25 inch in height, the point of maximum bril-

liancy being 0·56 inch from the top of the wick. The extremity of the flame is perfectly sharp without any tendency to smoke; without flicker or movement of any kind, it burns when protected from currents of air at a uniform rate of 136 grains of liquid per hour. The temperature should be about 60° F., although moderate variations on either side exert no perceptible influence. Bearing in mind Dr. Frankland's observations on the direct increase in the light of a candle with the atmospheric pressure, accurate observations ought to be taken only at one height of the barometer. To avoid the inconvenience and delay which this would occasion, a table of corrections should be constructed for each 0·1 inch variation of barometric pressure.

There is no doubt that this flame is very much more uniform than that of the sperm-candle sold for photometric purposes. Tested against a candle, considerable variations in relative illuminating-power have been observed; but on placing two of these lamps in opposition, no such variations have been detected. The same candles have been used, and the experiments have been repeated at wide intervals, using all customary precautions to ensure uniformity. The results are thus shown to be due to variations in the candle, and not in the lamp.

It is expected that whoever may be inclined to adopt the kind of lamp here suggested will find not only that its uniformity may be relied upon, but that, by following accurately the description and dimensions here laid down, each observer will possess a lamp of equivalent and convertible photometric value; so that results may not only be strictly comparable between themselves, but, within slight limits of accuracy, comparable with those obtained by other experimentalists. The dimensions of wick &c. here laid down are not intended to fix the standard. Persons engaged in photometry as an important branch of their regular occupation will be better able to fix these data than the writer, by whom photometry is only occasionally pursued as a means of scientific research. Already many improvements suggest themselves, and several causes of variation in the light have been noticed. Future experiments may point out how these sources of error are to be overcome; but at present there is no necessity to refine our source of standard light to a greater degree of accuracy than the photometric instrument admits of.

The instrument for measuring the relative intensities of the standard and other lights next demands attention. The contrivances in ordinary use are well known. Most of them depend on the law in optics, that the amount of light which falls upon a given surface varies inversely with the square of the distance between the source of light and the object illuminated. The simplest observation which can be taken is made by placing two sources of light (say, a candle and gas-lamp) opposite a white screen a few feet off, and placing a stick in front of them, so that two shadows of the stick may fall on the screen. The strongest light will cast the strongest shadow; and by moving this light away from the stick, keeping the sha-

dows side by side, a position will at last be found at which the two shadows appear of equal strength. By measuring the distance of each light from the screen and squaring it, the product will give the relative intensities of the two sources of light.

In practice this plan is not sufficiently accurate to be used except for the roughest approximations; and from time to time several ingenious contrivances, all founded upon the same law, have been introduced by scientific men by which a much greater accuracy is obtained; thus, in Ritchie's photometer, the lights are reflected on to a piece of oiled paper in a box, and their distances are varied until the two halves of the paper are equally illuminated. In Bunsen's photometer, which is the one now generally used, the lights shine on opposite sides of a disk of white paper, part of which has been smeared with melted spermaceti to make it more transparent. When illuminated by a front light, the greased portion of the paper will look dark; but if the observer goes to the other side of the paper, the greased part looks the lighter. If, therefore, lights of unequal intensity are placed on opposite sides of a piece of paper so prepared, a difference will be observed; but by moving one backwards or forwards, so as to equalize the intensity, the whole surface of the paper will appear uniformly illuminated on both sides. This photometer has been modified by many observers. By some the disk of paper is moved, the lights remaining stationary; by others the whole is enclosed in a box, and various contrivances are adopted to increase the sensitiveness of the eye, and to facilitate calculation: but in all these the sensitiveness is not greatly augmented, as the eye cannot judge of very minute differences of illumination approximating to equality.

In 1833 Arago described a photometer in which the phenomena of polarized light were employed. This instrument is fully described, with drawings, in the tenth volume of the '*Œuvres complètes de François Arago*;' but the description, although voluminous, is far from clear. The principle of its construction is founded on the *law of the square of the cosines*, according to which polarized rays pass from the ordinary to the extraordinary image. The knowledge of this law, he says, will not only prove theoretically important, but will further lead to the solution of a great number of very important astronomical questions. Suppose, for example, that it is wished to compare the luminous intensity of that portion of the moon directly illuminated by the solar rays, with that of the part which receives only light reflected from the earth, called the *partie cendrée*. Were the law in question known, the way to proceed would be as follows:—After having polarized the moon's light, pass it through a doubly refracting crystal, so disposed that the rays, not being able to bifurcate, may entirely undergo ordinary refraction. A lens placed behind this crystal will therefore show but one image of our satellite; but as the crystal, in rotating on its axis, passes from its original position, the second image will appear, and its intensity will go on augmenting. The movement of the crystal

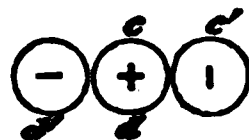
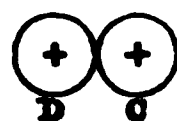
must be arrested at the moment when, in this growing extraordinary image, the segment corresponding to the part of the moon illuminated by the sun exhibits the intensity of the ashy part shown by the ordinary image. From these data it is easy to perceive, he says, that the problem is capable of solution.

In another part of the same volume, after speaking of the polariscope which goes by his name, Arago writes:—"I have now arrived at the general principle upon which my photometric method is entirely founded. The quantity (I do not say the proportion)—the quantity of completely polarized light which forms part of a beam partially polarized by reflection, and the quantity of light polarized rectangularly which is contained in the beam transmitted under the same angle, are exactly equal to each other. The reflected beam, and the beam transmitted under the same angle by a sheet of parallel glass, have in general very dissimilar intensities; if, however, we examine with a doubly refracting crystal first the reflected and then the transmitted beam, the greatest difference of intensity between the ordinary and the extraordinary images will be the same in the two cases, because this difference is precisely equal to the quantity of polarized light which is mixed with the common light."

In Arago's 'Astronomy,' the author again describes his photometer in the following words:—"I have constructed an apparatus by means of which, upon operating with the polarized image of a star, we can succeed in attenuating its intensity by degrees exactly calculable after a law which I have demonstrated." It is difficult to obtain an exact idea of this instrument from the description given; but from the drawings it would appear to be exceedingly complicated and to be different in principle and construction from the one now about to be described. The present photometer has this in common with that of Arago, as well as with those described in 1853 by Bernard *, and in 1854 by Babinet †, that the phenomena of polarized light are used for effecting the desired end; but it is believed that the present arrangement is quite new, and it certainly appears to answer the purpose in a way which leaves little to be desired. The instrument will be better understood if the principles on which it is based are first described.

Fig. 1 shows a plan of the arrangement of parts, not drawn to scale, and only to be regarded as an outline sketch to assist in the comprehension of general principles. Let D represent a source of light. This may be a white disk of porcelain or paper illuminated by any artificial or natural light. C represents a similar white disk, likewise illuminated. It is required to com-

Fig. 1.



* Comptes Rendus, April 25, 1853.

† Proceedings of the British Association, Liverpool Meeting, 1854.

pare the photometric intensities of D and C. (It is necessary that neither D nor C should contain any polarized light, but that the light coming from them, represented on each disk by the two lines at right angles to each other, forming a cross, should be entirely unpolarized.) Let H represent a double refracting achromatic prism of Iceland spar; this will resolve the disk D into two disks d and d' , polarized in opposite directions; the plane of d being, we will assume, vertical, and that of d' horizontal. The prism H will likewise give two images of the disk C; the image c being polarized horizontally, and c' vertically. The size of the disks D, C, and the separating power of the prism H, are to be so arranged that the vertically polarized image d , and the horizontally polarized image c , exactly overlap each other, forming, as shown in the figure, one compound disk $c d$, built up of half the light from D and half that from C.

The measure of the amount of free polarization present in the disk $c d$ will give the relative photometric intensities of D and C.

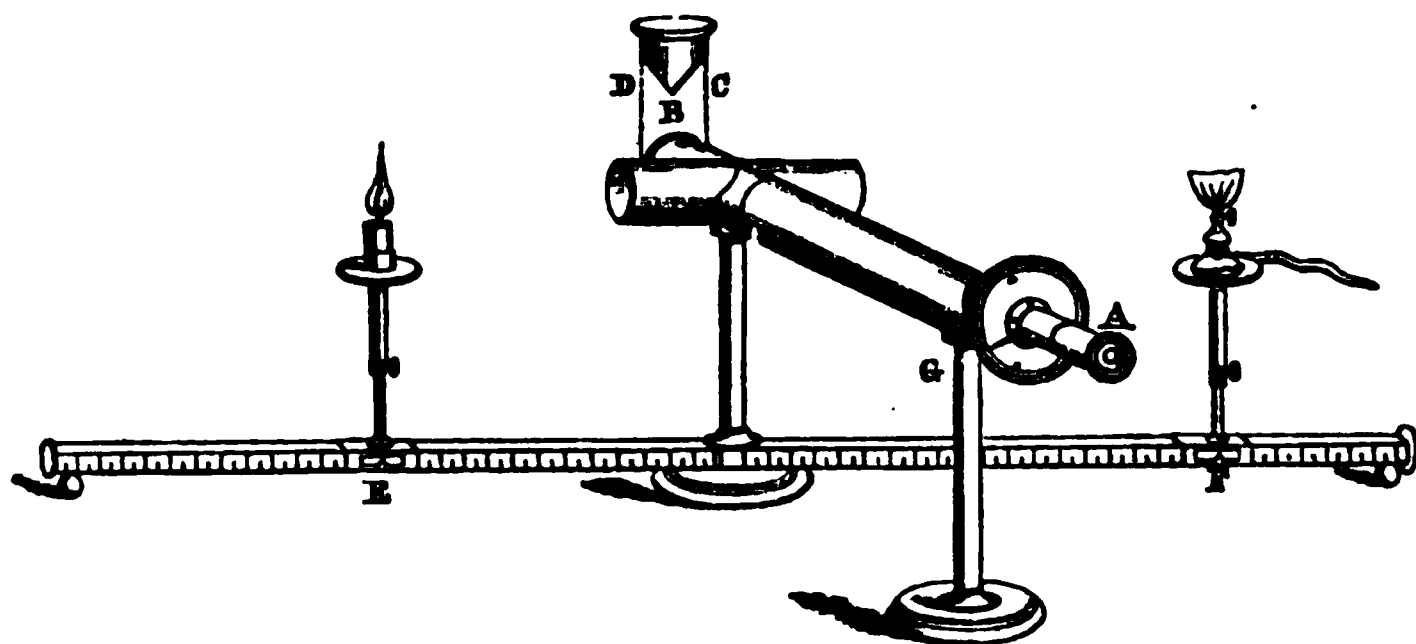
The letter I represents a diaphragm with a circular hole in the centre, just large enough to allow the compound disk $c d$ to be seen, but cutting off from view the side disks c' , d' . In front of the aperture in I is placed a piece of selenite, of appropriate thickness for it to give a strongly contrasting red and green image under the influence of polarized light. K is a doubly refracting prism, similar in all respects to H, placed at such a distance from the aperture in I that the two disks into which I appears to be split up are separated from each other, as at g , r . If the disk $c d$ contains no polarized light, the images g , r will be white, consisting of oppositely polarized rays of white light; but if there is a trace of polarized light in $c d$, the two disks g , r will be coloured complementarily, the contrast between the green and the red being stronger in proportion to the quantity of polarized light in $c d$.

The action of this arrangement will be readily evident. Let it be supposed, in the first place, that the two sources of light, D and C, are exactly equal. They will each be divided by H into two disks d' d and c c' , and the two polarized rays of which $c d$ is compounded will also be absolutely equal in intensity, and will neutralize each other, and form common light, no trace of free polarization being present. In this case the two disks of light, g , r , will be colourless. Let it now be supposed that one source of light (D, for instance) is stronger than the other (C). It follows that the two images d' , d will be more luminous than the two images c , c' , and that the vertically polarized ray d will be stronger than the horizontally polarized ray c . The compound disk $c d$ will therefore shine with partially polarized light, the amount of free polarization being in exact ratio with the photometric intensity of D over C. In this case the image of the selenite plate in front of the aperture I will be divided by K into a red and a green disk.

Fig. 2 shows the instrument fitted up. A is the eyepiece (shown in enlarged section at fig. 3); G B is a brass tube, blacked inside, having a

piece (shown separate at D C) slipping into the end B. The sloping sides, D B, B C, are covered with a white reflecting surface (white paper or finely

Fig. 2.

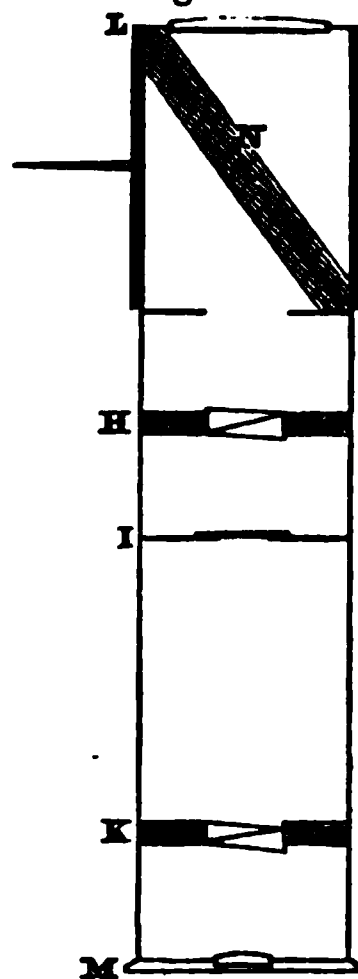


ground porcelain), so that when D C is pushed into the end B, one white surface D B may be illuminated (as in fig. 2) by the candle, and the other surface B C by the lamp. If the eyepiece A is removed, the observer, looking down the tube G B, will see at the end a luminous white disk divided vertically into two parts, one half being illuminated by the candle E, and the other half by the lamp F. By moving the candle E, for instance, along the scale, the illumination of the half D B can be varied at will, the illumination of the other half remaining stationary.

The eyepiece A (shown enlarged at fig. 3) will be understood by reference to fig. 1, the same letters representing similar parts. At L is a lens to collect the rays from D B C (fig. 2), and throw the image into the proper part of the tube. At M is another lens so adjusted as to give a sharp image of the two disks into which I is divided by the prism K. The part N is an adaptation of Arago's polarimeter; it consists of a series of thin plates of glass capable of moving round the axis of the tube, and furnished with a pointer and graduated arc (shown at A G, fig. 2). By means of this pile it is possible to partially polarize the rays coming from the illuminated disks in one or the other direction, and thus bring to the neutral state the partially polarized beam *c d* (fig. 1), so as to get the images *g, r* free from colour. It is so adjusted that when at the zero-point it produces an equal effect on both disks.

The action of the instrument is as follows. The standard lamp being placed on one of the supporting pillars which slide along the graduated stem (fig. 2), it is adjusted to the proper height, and moved

Fig. 3.



along the bar to a convenient distance, depending on the intensity of the light to be measured: the whole length being a little over 4 feet, each light can be placed at a distance of 24 inches from the disk. The flame is then sheltered from currents of air by black screens placed round, and the light to be compared is fixed in a similar way on the other side of the instrument. The whole should be placed in a dark room, or surrounded with non-reflecting screens; and the eye must also be protected from the direct rays of the two lights. On looking through the eyepiece two bright disks will be seen, probably of different colours. Supposing F represents the standard flame, and E the light to be compared with it, the latter must now be slid along the scale until the two disks of light, seen through the eyepiece, are about equal in tint. Equality of illumination is easily obtained; for as the eye is observing two adjacent disks of light, which pass rapidly from *red-green* to *green-red* through a neutral point of no colour, there is no difficulty in hitting this point with great precision. It has been found most convenient not to attempt to get absolute equality in this manner, but to move the flame to the nearest inch on one side or the other of equality. The final adjustment is now effected at the eye-end by turning the polarimeter one way or the other up to 45° , until the images are seen without any trace of colour. This will be found more accurate than the plan of relying entirely on the alteration of the distance of the flame along the scale; and by a series of experimental adjustments the value of every angle through which the bundle of plates is rotated can be ascertained once for all, when the future calculations will present no difficulty. Squaring the number of inches between the flames and the centre will give their approximate ratios; and the number of degrees the eyepiece rotates will give the number to be added or subtracted in order to obtain the necessary accuracy.

The delicacy of the instrument is very great. With two lamps, each about 24 inches from the centre, it is easy to distinguish a movement of one of them to the extent of 0.1 inch to or fro; and by using the polarimeter an accuracy considerably exceeding this can be attained.

The employment of a photometer of this kind enables us to compare lights of different colours with one another, and leads to the solution of a problem which, from the nature of their construction, would be beyond the powers of the instruments in general use. So long as the observer, by the eye alone, has to compare the relative intensities of tint-surfaces, respectively illuminated by the lights under trial, it is evident that, unless they are of the same tint, it is impossible to obtain that equality of illumination in the instrument which is requisite for a comparison. By the unaided eye one cannot tell which is the brighter half of a paper disk, illuminated on one side with a reddish, and on the other with a yellowish light; but by using the above-described photometer the problem becomes practicable. For instance, on reference to fig. 1, suppose the disk D were illuminated with light of a reddish colour, and the disk C with greenish

light, the polarized disks d' , d would be reddish and the disks c , c' greenish, the central disk $c d$ being of the tint formed by the union of the two shades. The analyzing prism K, and the selenite disk I, will detect free polarization in the disk $c d$, if it be coloured, as readily as if it were white; the only difference being that the two disks of light, g , r , cannot be brought to a uniform *white* colour when the lights from D and C are equal in intensity, but will assume a tint similar to that of $c d$. When the contrasts of colour between D and C are very strong—when, for instance, one is bright green and the other scarlet—there is some difficulty in estimating the exact point of neutrality; but this only diminishes the accuracy of the comparison, and does not render it impossible, as it would be according to other systems.

No attempt has been made in these experiments to ascertain the exact value of the standard spirit-flame in terms of the Parliamentary sperm-candle. Difficulty was experienced in getting two lots of candles yielding light of equal intensities; and when their flames were compared between themselves and with the spirit-flame, variations of as much as 10 per cent. were sometimes observed in the light they gave. Two standard spirit-flames, on the other hand, seldom showed a variation of 1 per cent., and had they been more carefully made they would not have varied 0.1 per cent.

This plan of photometry is capable of far more accuracy than the present instrument will give. It can scarcely be expected that the first instrument of the kind, made by an amateur workman, should possess equal sensitiveness with one in which all the parts have been skilfully made with special adaptation to the end in view.

ADDENDUM to *description of Photometer.* By W. CROOKES, F.R.S.
Received December 17, 1868.

When I wrote that other experimentalists had already made use of the phenomena of polarized light for measuring the intensity of light, I was not aware that a photometer already existed in which the principle of the one above described was adopted.

By the kindness of Sir Charles Wheatstone I have, within the last few days, been enabled to experiment with a photometer devised by M. Jamin, founded on the same principle. I have not yet succeeded in finding a printed account of this instrument, but a written one was supplied with it, and having been allowed to take it to pieces its construction is evident.

It consists, first, of a Nicol's prism, then of an achromatized doubly refracting prism; next, of two plates of quartz, cut oblique to the axis, reversed, and superposed; and finally, at the eye-end, of a second Nicol's prism. As in my instrument, each of the two lights to be compared split

into two images; the ordinary ray from one is superposed on the extraordinary ray from the other, and the compound beam so produced is examined further. The means adopted to effect the desired object are, however, very different, being much simpler in my method, whilst the results are superior.

In Jamin's photometer the light which eventually reaches the eye is comparatively feeble, and the field of view is very restricted; the objects themselves under comparison are seen direct through the instrument without the interposition of a telescopic arrangement, and no means are taken to prevent extraneous light from entering. The deficiency of light makes observations by artificial light difficult, whilst when examining objects illuminated by diffused or direct sunlight the eye is fatigued and bewildered by the variations of shape, size, and colour assumed by the overlapping objects seen through the instrument. In the photometer described in the former part of this paper, there is abundance of light, and the observation is made upon two luminous disks, which are magnified by means of a lens, so as to appear close to the eye. It will be found much easier to detect differences of colour between these two adjacent disks than to observe the presence or absence of the coloured fringes in the central portion of the field of Jamin's photometer. In the former case the eye has nothing to observe but two uniform and purely coloured disks, changing from red-green to green-red through an intermediate stage of neutrality; in the latter case the eye has to detect the stage of neutrality in the central portion of the field, where the two images under comparison overlap, the attention being distracted, and the sensitiveness of the eye weakened, by the brilliantly coloured fringes which cross the adjacent objects.

A direct comparison of the two instruments for sensitiveness shows that the present photometer will detect much more minute differences of intensity than Jamin's will, whilst it will work with tolerable accuracy in a light too feeble to give any results with the latter instrument.

April 8, 1869.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

- I. "Preliminary Notice on the Mineral Constituents of the Breitenbach Meteorite." By Professor N. STORY MASKELYNE, M.A. Communicated by Professor WARINGTON W. SMYTH, F.R.S. Received March 2, 1869.

This meteorite, which belongs to the rare class intermediate between meteoric irons or siderites and meteoric stones or aërolites (a class to

which I applied some years since the term siderolites), was found in Breitenbach in Bohemia.

It is a spongy metallic mass, very similar to the siderolite of Rittersgrün in Saxony, the hollows in the iron being filled by a mixture of crystalline minerals. These minerals are two in number; and the present notice deals with these two minerals.

1. One of them is of a pale-green colour, crystallizing in the prismatic system, and presenting at once the formula of an augitic mineral and a crystalline form nearly approximating to that of olivine. Dr. Viktor von Lang, when my colleague at the British Museum, measured some merohedral crystals of this mineral, and obtained for its elements

$$a : b : c = 0.8757 : 0.8496 : 1,$$

$$\begin{aligned} 110.010 &= 44 \frac{0}{8} \\ 101.100 &= 41 \frac{11}{16} \\ 104.100 &= 74 \frac{3}{16} \\ 011.010 &= 40 \frac{16}{16} \end{aligned}$$

The analysis of this green mineral gave, from 0.4127 grm.,—

		per cent.	Oxygen-ratios.	Equivalent ratios.
Silica	0.2315	56.101	29.920	1.87
Magnesia	0.1247	30.215	12.087	1.51
Ferrous oxide	0.0560	13.583	3.018	0.37
	<u>0.4122</u>	<u>99.899</u>		<u>1.88</u>

results which correspond very nearly with an Enstatite of the formula $(\text{Mg}_4 \text{Fe}_4) \text{SiO}_8$.

The specific gravity is 3.23.

It is remarkable that of the minerals presenting the general formula



where M stands for one or more metals of the calcium and magnesium groups, we are acquainted with two anorthic types (Rhodonite and Babingtonite); three oblique types, those, namely, of Wollastonite, of Hornblende, and of Augite; two prismatic types, those, namely, of Enstatite and Anthophyllite, homœomorphous with the oblique Augites and Hornblendes; and to these we shall have now to add (if the measurements of Dr. Lang shall prove to be distinct from those of Enstatite) a third, in the green mineral under description.

Of these, the prismatic types are essentially those of the magnesian group. The rest, with the exception of the calcium silicate (Wollastonite), are types belonging to the mixed groups.

2. The other mineral is one of very great interest. It is, in short, silica crystallized in forms and in a system distinct from quartz, and pos-

sibly is tridymite. In bulk it forms about a third part of the mixed crystalline mass.

The crystals are very imperfect, and are twinned: but there are two cleavages parallel to the planes of a prism of about 119° ; and, on looking through a plane that is perpendicular to this zone, it is seen that the crystal is biaxial. The normal to this plane is parallel to the second mean line, the optical character being negative.

A section made for examination in the microscope showed two small crystals in which light traverses the section with equal brilliancy during its rotation between crossed Nicol prisms. This, and possibly a similar case recorded by Vom Rath, seems to result from the section being cut parallel to a composite portion of the crystal.

The analysis of the mineral gave, by distillation of the silica as silicic difluoride, and subsequent determination as potassic fluosilicate, 97.43 per cent. of silica, the remainder being oxide of iron and lime. Thus 0.3114 grm. gave:

		per cent.
Silica	0.3034	97.43
Ferric oxide	0.0035	1.124
Lime	0.0018	0.578
	<u>0.3087</u>	<u>99.132</u>

A second analysis gave 99.21 per cent. silica, 0.79 of residue.

Its specific gravity, as determined from a very small amount of the mineral picked under the microscope, was 2.18; a second determination made on a larger amount gave the value 2.245. That of tridymite is 2.295 to 2.3. This may be taken as evidence that the mineral is not quartz, the specific gravity of which is 2.65. Vom Rath's experiments were made on a rather less pure form of tridymite.

There can be no doubt from these results, further details of which shall be shortly laid before the Society, that this mineral is silica in the form of its allotropic condition and lower density. It may possibly be the mineral to which Vom Rath has given the name of Tridymite; the crystalline system, however, of Tridymite, as given by Vom Rath, does not accord with the above facts.

II. "On the Derivatives of Propane (Hydride of Propyl)." By C. SCHORLEMMER. Communicated by Prof. STOKES, Sec. R.S Received March 5, 1869.

At the time when I commenced this investigation, the existence of normal propyl alcohol was very doubtful. According to Chancel*, this body is found in the fusel-oil from the marc of grapes; but Mendelegeff† tried in vain to isolate it from a sample of this oil which he had obtained

* Compt Rend. vol. xxxvii. p. 410.

† Zeitschrift für Chemie, 1868, p. 25.

from Chancel himself. Several attempts to prepare the normal alcohol by synthesis failed. Thus Linnemann and Siersch* tried to obtain it by converting acetonitril into propylamine, by means of hydrogen in the nascent state, and decomposing the hydrochlorate of this base with silver nitrite; but the alcohol thus formed was found to be the secondary one. The same compound was obtained by Butlerow and Ossokin†, by acting upon ethylene iodohydrine, $C_2H_4 \begin{Bmatrix} O & H \\ I & \end{Bmatrix}$, with zinc methyl, in order to replace iodine by methyl. Now as in both cases, according to theory, the normal or primary alcohol ought to have been formed, and as we have no explanation why instead of this compound the secondary alcohol was obtained, Butlerow and Ossokin believe that the normal propyl-alcohol cannot exist. Not agreeing with this view, I was led to an investigation of this subject, the results of which I have the honour to lay before the Society.

My reasoning was as follows:—It appears, as the most probable theory, and which is now accepted by most chemists, that the four combining powers of the carbon atom have the same value. If so, only one hydrocarbon having the composition C_3H_8 can exist. This *propane* must be formed by replacing the iodine in the secondary propyl iodide, by hydrogen, and subjecting the hydrocarbon thus obtained to the action of chlorine, by which primary propyl chloride must be formed in accordance with the behaviour of other hydrocarbons of the same series.

I soon found that my theory was correct; and in a short note, which I published in 'Zeitschrift für Chemie' (1868, p. 49), I stated that I had obtained the normal propyl alcohol by this method. At the same time, Fittig proved that it was contained in fusel-oils‡, and lately Linnemann prepared it synthetically from ethyl-compounds by converting acetonitrile (ethyl cyanide) into propionic anhydride, and acting upon this body with nascent hydrogen§.

The propane which I used in my researches was obtained by acting upon isopropyl iodide with zinc turnings and diluted hydrochloric acid. A continuous evolution of gas takes place if the flask containing the mixture is kept cold. If it is not cooled down a violent reaction soon sets in. The gas always contains vapour of the iodide, even if it has been evolved very slowly. In order to purify it as much as possible, it was washed with Nordhausen sulphuric acid, with a mixture of nitric and sulphuric acids and with caustic soda solution.

As a gas-holder I used a tubulated bell-jar, which was suspended in a larger inverted one, filled with a concentrated solution of common salt. When a sufficient quantity of gas had collected, chlorine was passed into

* Annalen Chem. Pharm. vol. cxliv. p. 137.

† Ibid. vol. cxlv. p. 257.

‡ Zeitschrift für Chemie, 1868, p. 44.

§ Annalen Chem. Pharm. vol. cxlviii. p. 251.

it, care being taken not to have it in excess. In diffused daylight substitution-products were formed, which collected as an oily layer on the salt solution. Alternately more propane and chlorine were passed into the apparatus, until it was nearly filled with the excess of propane and vapours of the most volatile substitution-products. The latter were condensed by passing the gas into a receiver surrounded by a freezing-mixture. To collect the liquid chlorides which were contained in the gas-holder, the tubulus of the bell-jar was closed with cork, which was provided with a wide short glass tube, open at both ends, and so much salt solution put into the gas-holder that the chlorides entered this tube, from which they could easily be removed with a pipette. By repeating this process several times, a quantity of chlorine compounds, sufficient for further investigation, was obtained. This was washed with water, dried over caustic potash, and distilled. The liquid commenced to boil at $42^{\circ}\text{C}.$, the boiling-point rising towards the end above $200^{\circ}\text{C}.$ By fractional distillation, a comparatively small quantity of a liquid was obtained, which boiled at 42° – 46° , and consisted of the primary propyl chloride, $\text{C}_3\text{H}_7\text{Cl}$.

0.0975 of this chloride gave 0.1730 silver chloride, and 0.005 silver, corresponding to 0.044 chlorine.

Calculated for $\text{C}_3\text{H}_7\text{Cl}$.

45.2 per cent. Cl.

Found.

45.5 per cent. Cl.

In order to prove that this body was really the normal chloride, it had to be converted into the alcohol. For this purpose I used that portion of the chlorides which, after repeated distillation, boiled below $80^{\circ}\text{C}.$ It was heated in sealed tubes with potassium acetate and glacial acetic acid for several hours to $200^{\circ}\text{C}.$, and thus converted into the acetate, a light colourless liquid, possessing the characteristic odour of the acetic ethers. I did not endeavour to obtain this ether in the pure state, as this could have been effected only with great loss of material, but converted it at once into the alcohol, by heating it with a diluted solution of potash, in sealed tubes, up to $120^{\circ}\text{C}.$ After cooling, the contents of the tubes were distilled and rectified. A portion of it was oxidized with a cold diluted solution of chromic acid. No gas was evolved, but a strong smell of aldehyde was perceived, which disappeared on adding more chromic acid. On distilling to dryness, an acid liquid was obtained, which was neutralized with sodium carbonate. The solution was evaporated to dryness, and the residue distilled with a quantity of sulphuric acid, sufficient to liberate about one-fourth of the acid. The residue in the retort was again distilled with the same quantity of sulphuric acid, and, by repeating this process, the acid was obtained in four fractions. Each of these was converted into the silver-salt by boiling with silver carbonate. The silver-salts crystallized from the hot saturated solution in small shining needles, which were grouped in stars and feathers. These were dried, first, over sulphuric acid, afterwards in the steam-bath, and the silver determined by ignition.

				per cent.
Fraction (1)	0·2350	gave	0·1404 silver	=59·74
„ (2)	0·2420	„	0·1450 „	=59·91
„ (3)	0·1676	„	0·1002 „	=59·78
„ (4)	0·2124	„	0·1264 „	=59·51
			Mean	59·73
Silver propionate contains				59·67

I also prepared the lead-salt, which exhibited the properties of lead-propionate; it did not crystallize, but dried up to an amorphous gum-like mass. As by oxidation no other acid besides propionic was found, it follows that the alcoholic liquid could only contain normal propyl alcohol. I tried to isolate this body from the remaining liquid, by adding potassium carbonate until it separated into two layers. The upper one was taken off and dried, first over fused potassium carbonate, and afterwards over anhydrous baryta. This liquid, however, proved to be a mixture; it began to boil at 80° C., and the boiling-point rose slowly to 96° C. By fractionating it could be separated into two portions—a smaller one boiling between 80°–85°, and a larger one boiling above 90°. The portion boiling between 92°–96° gave, by combustion, numbers agreeing with the composition of propyl alcohol.

0·2238 substance gave 0·4098 carbon dioxide and 0·2675 water.

	Calculated.		Found.
C ₃	36	60	59·81
H ₈	8	13·33	13·28
O	16	26·67	—
	60	100·00	

I have not yet studied the properties of this alcohol, as I hope to obtain it soon in larger quantities.

The liquid boiling between 80°–85° appears to be an acetal; it is not acted upon by sodium, and therefore can easily be obtained free from alcohol, by distilling it over this metal. The small quantity was just sufficient for two analyses, the results of which give C₆ H₁₂ O₂ as the probable formula.

(1) 0·2500 gave 0·2725 water and 0·5280 carbon dioxide.

(2) 0·2755 gave 0·2950 water; the determination of carbon was lost.

	Calculated.		Found.	
			I.	II.
C ₆	60	57·96	57·60	—
H ₁₂	12	11·53	12·11	11·93
O ₂	32	30·78	—	—
	104	100·00		

How this body has been formed I cannot explain.

As I have already mentioned, chloride of propyl forms only a small fraction of the products obtained by subjecting propane to the action of

chlorine, the chief product of the reaction being a liquid which boils at 94° – 98° C., and has the formula $C_3H_6Cl_2$.

0.1600 gave 0.3970 silver chloride and 0.005 silver.

Calculated for $C_3H_6Cl_2$.
62.8 per cent. Cl.

Found.
62.4 per cent.

This body is propylene dichloride; for its boiling-point not only coincides with that of this compound, but also all its reactions are the same. Heated with potassium acetate and acetic acid in closed tubes, it is readily decomposed, a high boiling acetate being formed, which, on heating with concentrated potash solution and distilling, yields a liquid the last portion of which boils between 180° – 190° C., and possesses the sweet taste of propyl glycol. I did not isolate the glycol in the pure state, but proposed to establish its structure by oxidation.

A diluted cold solution of chromic acid acts violently on it, carbon dioxide being evolved in abundance, and a strong odour of aldehyde being recognized, which, on further addition of the oxidizing liquid, was changed into that of acetic acid. By distillation an acid liquid was obtained, which, on boiling with silver carbonate, yielded a silver-salt, which crystallized in the well-known needles of silver acetate.

0.3013 of this salt left on ignition 0.1935 silver.

Silver acetate contains
64.67 per cent Aq.

Found.
64.22 per cent.

The oxidation-products (carbon dioxide and acetic acid) prove sufficiently that the structure of the glycol is expressed by the formula $CH_2-CH(OH)-CH_2(OH)$, which is that of the known propyl glycol.

The foregoing researches establish a general reaction for converting secondary compounds of the alcohols into those of primary radicals. This is effected by replacing the iodine in secondary iodides by hydrogen, and subjecting the hydrocarbons thus obtained to the action of chlorine, by which the primary chlorides are formed.

Of greater interest, perhaps, as possessing an important bearing on the theory of substitution, is the fact that the second substitution-product of propane consists of propylene dichloride, having the structure $CH_2-CHCl-CH_2Cl$. This was the less to be expected, as ethane, C_2H_6 , the hydrocarbon next lower in the series, yields, by acting on it with chlorine as second product, ethylidene dichloride, CH_2-CHCl_2 . Whilst, therefore, in propane first one hydrogen atom in the methyl group is replaced by chlorine, and afterwards one which is combined with the adjoining carbon atom, in ethane the substitution takes place at one and the same carbon atom. The action of chlorine upon propane is certainly in contradiction to all theories of substitution which have been expounded.

In a second communication I propose to describe the higher chlorinated substitution-products of propane.

III. "Researches in Animal Electricity." By CHARLES BLAND RADCLIFFE, M.D. Communicated by C. BROOKE, F.R.S. Received, Part I., Feb. 18; Part II.-V., March 11, 1869.

(Abstract.)

After a description of certain instruments, now employed for the first time in researches of this kind, the topics inquired into successively are:—the electrical phenomena which belong to nerve and muscle in a state of rest; the electrical phenomena which mark the passing of nerve and muscle from the state of rest into that of action; the motor phenomena ascribed to the action of the "inverse" and "direct" voltaic currents; and electrotonus.

I. *On certain Instruments now employed for the first time in Researches in Animal Electricity.*

The instruments here referred to and described are Sir Wm. Thomson's Reflecting Galvanometer, Mr. Latimer Clarke's Potentiometer, and some new electrodes devised by the author.

Sir Wm. Thomson's Reflecting Galvanometer, which is the principal galvanometer made use of, is stated to be more manageable than the old galvanometer of Prof. Du Bois Reymond, and not less sensitive.

Mr. Latimer Clarke's "Potentiometer," which is really a very ingenious adaptation of the idea upon which the Wheatstone's Bridge is based, is the instrument employed for the measurement of tension. It is so delicate as to measure with certainty the $\frac{1}{10,000}$ part of the tension of a Daniell's cell.

The new electrodes are simply pieces of platinum wire, flattened and pointed at the free ends, and having these free ends freshly tipped with sculptor's clay at the time of an experiment. The necessary homogeneity of the two is secured by pushing the clay a little further on one of the wires, or by pulling it a little further off; for by a simple manipulation of this kind it is found that the clay tips of the two electrodes may be so adjusted as to allow them to be brought together without the development of the slightest current. After a very little practice it is found, indeed, that in a very few moments the two electrodes may be made perfectly homogeneous by thus covering or uncovering one of them. And, further, it is found that the secondary polarization arising from the passage of a current may be got rid of at once by simply bringing the clay tips of the two electrodes together so as to exclude the polarizing current from the circuit of the galvanometer, and by leaving them in this position for a moment or two—by *short-circuiting* the galvanometer, that is to say, for a very brief period. It is found, in short, that these electrodes are infinitely more manageable, and not less effectual, than the electrodes commonly in use, in which enter zinc troughs filled with saturated solution of zinc, and pads of blotting-paper, the pads being kept sodden with this

solution by having one of their ends dipping into it—electrodes which, to say the least, are not easily put in order or kept in order.

II. *On the Electrical Phenomena which belong to Living Nerve and Muscle during the state of rest.*

Living nerve and muscle supply currents to the galvanometer (the *nerve-current*, and the *muscular current*, so called) which are not supplied by dead nerve and muscle. These currents, when the tissues supplying them are fresh and at rest, show that the surface composed of the sides of the fibres, and the surface composed of the ends of the fibres, are in opposite electrical conditions, the former surface being positive, the latter negative. These currents, when the tissues supplying them are about to die, and, in some cases, when they are put in action, are wholly or partially reversed—are so changed in direction, that is to say, as to show that there is at this time a total or partial reversal in the electrical relations of the ends and sides of the fibres. The fact of a *partial* reversal, in which the fibres may be positive in some part of their sides and negative in others, or positive at one of their ends and negative at the other, is now pointed out for the first time.

Nerve and muscle, and the animal tissues generally, oppose a very high resistance to the passage of a common voltaic current—so high, indeed, as to justify the inference that muscles and nerves may be looked upon as non-conductors rather than as conductors. The resistance in an inch of the sciatic nerve of a frog, for example, is about 40,000 B.A. units, or nearly seven times that of the whole Atlantic Cable.

The mean tension of the nerve-current and the muscular current proves to be about half that of a Daniell's cell. Moreover, negative and positive electricity, in equal amounts, are both found to be present. The case is not one in which only one kind of electricity is present,—in which what appears to be negative is only a lower degree of positive, or *vice versa*; it is one in which two electricities are present, one above the zero of the earth, the other below it—one as much above the zero of the earth as the other is below it. These facts are made out by means of the potentiometer.

Looking at these facts, and especially at the comparative non-conductibility of nerve and muscle, wholly or in part, and at the presence in these tissues of positive and negative electricity in equal quantities, it is thought probable—

That the comparative non-conductibility of nerve and muscle may allow certain parts of these tissues to act as *dielectrics* rather than as conductors, and that these parts may be the sheaths of the fibres.

That the development of one kind of electricity (by oxygenation, or in some other way) on the exterior of the sheaths of the nerve and muscular fibres may lead, *by induction*, to the development of the other kind of electricity on the interior of these sheaths.

That the exterior and interior of the sheaths of the fibres in nerve and muscle may be in opposite electrical conditions, because the sheath plays the part of a dielectric.

That the surface composed of the ends of the fibres in nerve and muscle may be in an electrical condition opposed to that of the surface composed of the sides of these fibres, because there may be a communication at the ends of the fibres with the interior of the sheaths of the fibres.

That the nerve-current and muscular current may be no more than accidental phenomena, depending upon the mere fact of the positive exterior and the negative interior of the nerve and muscular fibre being connected by a conductor.

That the fundamental electrical condition of nerve and muscle *during rest* may be, not one of currents ever circulating in closed circuits around *peripolar* molecules, of which currents the nerve-current and the muscular current are only derived portions, but one of tension—a condition, not current in any sense, but static—a state which, as long as it lasts, must tend to keep the molecules acted upon in a state of mutual repulsion.

III. *On the Electrical Phenomena which mark the passing of Nerve and Muscle from the state of Rest into that of Action.*

The fact of “induced contraction” so called, together with the analogies existing between the muscles and the electric organ of the torpedo as to the relation to the nervous system and the manner of acting in more cases than one, are cited as reasons for believing, with Matteucci, that a discharge, analogous to that of the torpedo, marks the passage of both muscle and nerve from the state of rest into that of action.

And, further, the fact, well established by Prof. Du Bois Reymond, that the nerve-current and the muscular current are both alike greatly *weakened* when the nerve or muscle passes from the state of rest into that of action, is cited as corroborative evidence in support of Matteucci's conclusion—as demonstrating, in short, the actual disappearance of electricity in the very cases in which Matteucci, from analogy solely, infers the existence of *discharge*.

Again, the conclusion arrived at as to the electrical condition of muscle and nerve during the state of rest, is looked upon as another argument to the same effect; for if it be true that this condition is one, not of current, but of *charge*, then there is a substantial ground for supposing that the passing of nerve and muscle from the state of rest into that of action may be marked by *discharge*.

In a word, the more the evidence is considered the more it seems to justify this conclusion,—that the passing of nerve and muscle from the state of rest into that of action is marked by a discharge of electricity analogous to that of the torpedo.

IV. *On the Motor Phenomena ascribed to the action of the "Inverse and Direct" Voltaic Currents.*

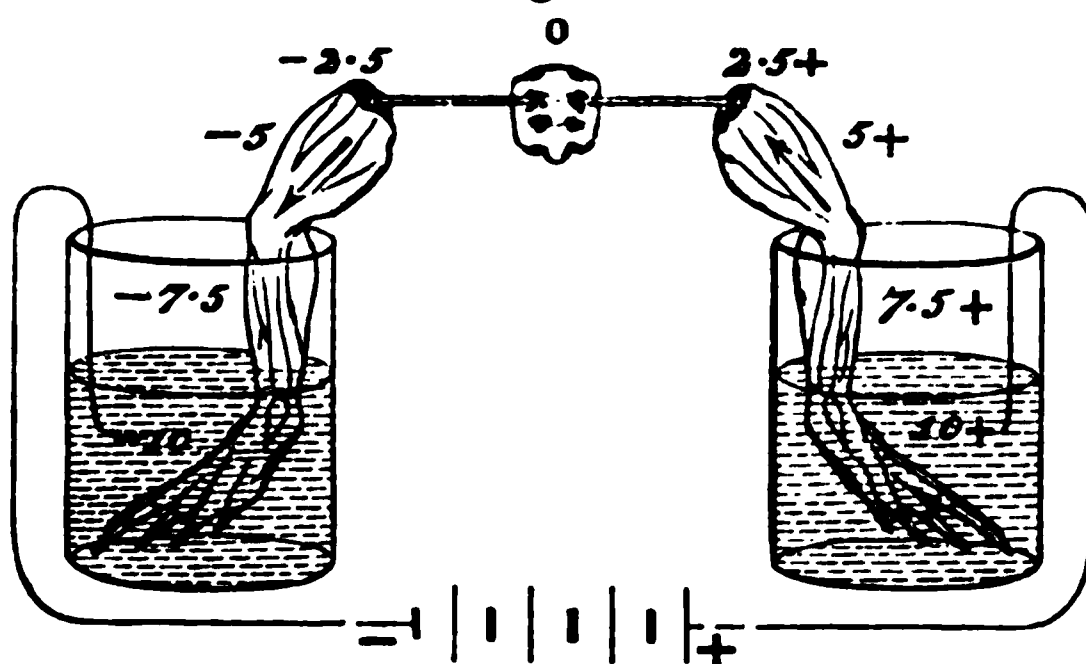
When a voltaic current is so made to pass through the two hind limbs of a prepared frog that the current is "inverse" in one limb and "direct" in the other, it is found that the closing and opening of the circuit may or may not be attended by contraction, and that the presence or absence of contraction may or may not obey a similar rule in the two limbs. The facts admit of being arranged in three stages, thus:—

		The limb in which the current is "Direct."		The limb in which the current is "Inverse."	
		On closing circuit.	On opening circuit.	On closing circuit.	On opening circuit.
<i>Stage I.</i> When the electricity acts <i>similarly</i> upon the two limbs.	(a) With a <i>weak</i> battery.	Contraction.	0.	Contraction.	0.
	(b) With a <i>strong</i> battery.	Contraction.	Contraction.	Contraction.	Contraction.
<i>Stage II.</i> When the electricity acts <i>differently</i> upon the two limbs.	1st period.	Contraction.	0.	Contraction.	Contraction.
	2nd period.	Contraction.	0.	0.	Contraction.
	3rd period.	0.	0.	0.	Contraction.
<i>Stage III.</i> When the electricity is again made to act <i>similarly</i> upon the two limbs by reversing the position of the poles.	(a) With a <i>weak</i> battery.	Contraction.	0.	Contraction.	0.
	(b) With a <i>strong</i> battery.	Contraction.	Contraction.	Contraction.	Contraction.

In seeking to account for these facts, the "direct" and "inverse" currents are not the only agencies which have to be taken into consideration. If the limbs were perfectly sufficient conductors, the sole agencies at work might be these currents; but instead of being very good conductors, the limbs are, in fact, non-conductors rather than conductors, opposing a resistance to the current of about 40,000 B.A. units (a resistance nearly seven times that of the whole Atlantic Cable); and the result of closing the circuit with them is this—that each limb is found to be charged with the free electricity which is present at the poles when the circuit is open, and which would be entirely discharged if the place of the limbs were supplied by a perfectly sufficient conductor. The case is one in which, in accordance with the investigations of Mr. Latimer Clarke on the tension of the voltaic circuit, each limb is found to participate in the charge of the pole nearest to it, the charge being positive in the limb in which the current is inverse, and negative in the limb in which the current is direct, the tension of the charge in each limb diminishing regularly from the pole where it is highest, to some point midway between the poles, where it is at zero; the

case is one in which, supposing the value of the tension at each of the poles to be 10, the state as to tension at different points between the poles is found to be that which is indicated by the figures in the accompanying sketch:—

Fig. 1.



This, then, being the state of the limbs as to tension under these circumstances, it is plain that there must be definite changes in tension at the closing and opening of the circuit. It is plain that the limbs must be traversed by a discharge at the moment of closing the circuit; for the charge of the poles must diminish in direct proportion to the freedom with which the current passes. It is plain also that the opposite electricities which are accumulated in the limbs while the circuit is closed must be discharged when the circuit is opened. It is possible also that the discharge at the opening of the circuit may be *less* in amount than that which occurs at the closing of the circuit; for immediately after the opening both the limbs may be supposed to receive a charge from the pole with which they happen to remain in connexion, which charge will to some degree counteract the discharge.

How then? Is it possible that these changes of tension may have to do with the motor phenomena which are ascribed to the action of the direct and inverse currents?

That the changes of tension in question are of themselves sufficient to tell upon the muscles in the requisite manner is proved by a new and very curious experiment. The two hind legs of a frog, prepared and arranged as in the experiment for exhibiting the action of the inverse and direct currents, are connected, time after time, first with one pole of the battery and then with the other, but never with the two poles at once. The result, for a time at least, is contraction in one or both of the limbs when they are thus carried from one pole to the other. There is a succession of charges and discharges; for before a charge can be received from either pole this charge must neutralize the charge carried away from the other pole. The contraction must have to do with changes of tension, and with changes of tension only; for the circuit remains open from the beginning to the end of

the experiment. The case, indeed, appears to be not remotely analogous to that in which the prepared limbs of a frog are made to hang from the prime conductor of an electrical machine, and then charged and discharged alternately; for here the rule as to contraction is the same, namely this,—that the limbs contract, not when they receive or while they retain the charge, but at the moment of discharge.

That the changes of tension in question *do* actually affect the limbs as they are found to be affected under the action of the inverse and direct current appears to gain in probability as the matter is more fully inquired into.

There is no difficulty in referring to changes of tension the phenomena belonging to the first stage (*vide* Table). If the closing and opening of the circuit be attended by discharge, and if contraction be coincident, not with charge but with discharge, the presence of contraction in both limbs at the moments of opening and closing the circuit is in accordance with rule; and if the discharge at the opening of the circuit be weaker than that which happens at the closing, it is easy to see that with a weak battery the stronger discharge at the moment of closing the circuit may be strong enough to tell upon the muscles when the weaker discharge at the opening of the circuit is not strong enough to do so. Indeed it is plain that the absence of contraction at the opening of the circuit in the case where a weak battery is used is merely a matter of wanting battery power, for the missing contraction is made to appear by simply supplying this want.

Nor is there any difficulty in applying the same key to the explanation of the phenomena belonging to the second stage (*vide* Table).

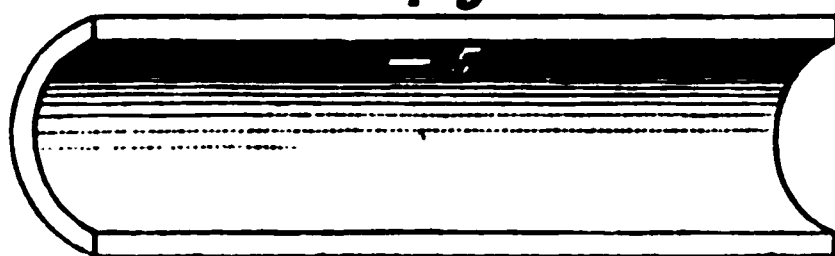
It is a fact that the power of contracting is affected very differently in the two limbs by the action of the electricity. The limb in which the current is direct loses this power much more speedily than it does when left to itself; the limb in which the current is inverse retains this power much longer than it does when left to itself; the limb in which a direct current has been passed until the power of contracting is at an end recovers this power, and this, too, more than once, if the direction of the current be changed for a time. Of these facts—the impairment of the power of contracting in the limb in which the current is direct, the preservation and restoration of this power in the limb in which the current is inverse—there can be no question.

There is also reason to believe that there are electrical differences in the two limbs which will, in some degree at least, account for the differences in the power of contracting, and for other differences which have yet to be considered.

The conclusion already arrived at respecting the natural electricity of nerve and muscle is that the state during rest is one of charge—that, ordinarily at least, the sheaths of the fibres are charged positively at their exterior and negatively at their interior. The resistance of the animal *tissues* to electrical conduction, it is assumed, is sufficient to keep the two

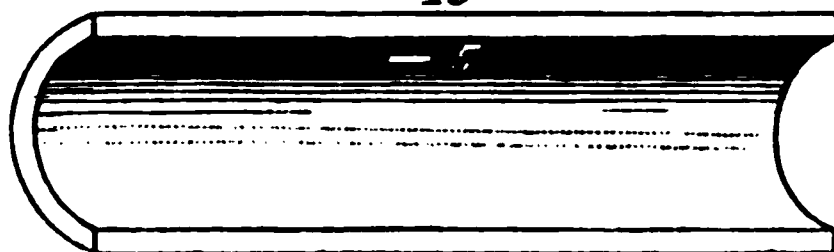
opposite electricities apart—an assumption, be it remarked, which is not a little borne out by the fact that the resistance which the voltaic current encounters in the hind limbs of a frog when its course is up one limb and down the other (*vide* fig. 1) is sufficient to keep the two limbs in opposite electrical conditions as regards charge. In short, the natural electrical condition of nerve and muscle during rest may be assumed to be one in which the exterior of the sheath of the fibre is positive and the interior negative—a state of charge which, taking 5 as the value of the tension, and viewing the sheath in longitudinal section from within, may be figured thus :—

Fig. 2.
+ 5



The electrical condition of the fibres of the nerves and muscles of the *limb in which the current is direct* may be assumed to be one in which the exteriors of the sheaths are charged negatively from the negative pole, and the interiors positively by induction—a state in which the disposition of the two electricities forming the charge is the reverse of that which belongs to the natural charge—in which, before this reversal can take place, there must be a meeting of opposite electricities without and within the sheaths which must result in the discharge of the weaker natural charge and of an equivalent quantity of the artificial charge—which, assuming 10 as the value of the tension, and taking the figure already used to illustrate the state of things in the natural charge, may be represented thus :—

Fig. 3.
- 10

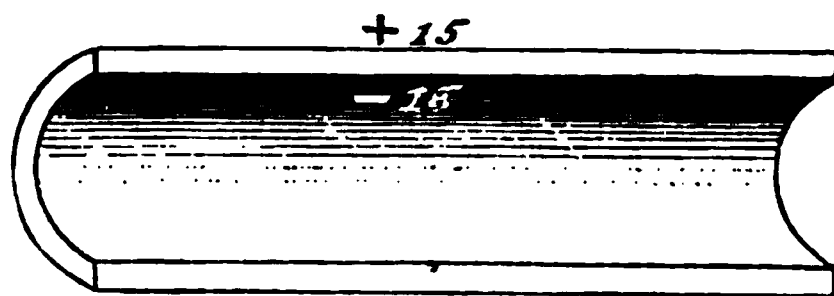


The case, indeed, is one in which the artificial charge of the fibres involves a *reversal* similar to that which happens naturally when these fibres, in some instances at least, have lost a great portion of their activity,—in which there may be supposed to be a similar reason, whatever that may be, for failure in this activity: and hence it need not be altogether a matter of wonder that the limb in which the current is direct should lose its power of contracting more rapidly than the same limb when left to itself.

The electrical condition of the fibres of the nerves and muscles of the *limb in which the current is inverse*, on the other hand, may be taken as one in which the exteriors of the sheaths are charged positively from the positive pole, and the interiors negatively by induction—a state in which

the sheaths are affected without and within similarly by the natural and artificial charges—in which the artificial may be added to the natural charge, causing, not discharge, as in the case of the limb in which the current is direct, but *surcharge*—in which, assuming the value of tension to be 10 for the artificial and 5 for the natural charge, and taking the figure used before in illustration, the result of this combination of charges may be set down thus :—

Fig. 4.



The case is one in which the artificial charge, by supplementing the natural charge, may be supposed to retard the disappearance of the natural charge, and with it the power of contracting ; for between this charge and this power there is, without question, a connexion which may not be severed. And if this be so, then it is not difficult to advance a step further and perceive how it is that this artificial charge may restore the natural power of contracting after it is lost, and how, in this way, after this power has disappeared from the limb in which the current is direct, it may be brought back again by reversing the position of the poles. In a word, it is not altogether unintelligible that there should be along with the inverse current an action which preserves and restores the power of contracting.

And if the condition of the two limbs be thus different when the circuit is closed, a clue is found, by tracing which it is possible to arrive at an explanation of the different behaviour of the two limbs at the moment of closing and opening the circuit.

In the second period of the second stage (*vide* Table), the limb in which the current is direct contracts at the moment of closing and not at the moment of opening the circuit, and, contrariwise, the limb in which the current is inverse contracts at the moment of opening the circuit and not at the moment of closing it ; and most assuredly there is nothing anomalous in these differences.

In the *limb in which the current is direct*, as will appear on comparing the two figures 2 & 3, there must be at the moment of closing the circuit a conflict between the natural and artificial charges of the fibres before the stronger artificial charge can have the victory which it gains in the end,—a conflict in which the neutralization of the natural charge by an equivalent quantity of the artificial charge, must issue in discharge ; and hence the presence of contraction at this moment, if contraction be coincident, not with charge, but with discharge. Indeed *there is a double reason for contraction at this moment* ; for in addition to

this discharge is the discharge of the opposite electricities of the poles which attends upon the closing of the circuit in any case. Nor is the absence of contraction at the opening of the circuit unintelligible; for it is easy to see that the loss in the power of contracting which the limb in which the current is direct has experienced by this time, may have rendered the limb incapable of responding to the weaker discharge which attends upon the opening of the circuit.

In the *limb in which the current is inverse*, as will appear on comparing the figures 2 & 4, there must be the addition of the artificial charge to the natural charge—a surcharge—a state which may nullify the discharge attending upon the closing of the circuit; and hence the absence of contraction at the closing of the circuit; for, according to the premises, there will be no contraction if there be no discharge, or, rather, there will be no contraction if there be no *sufficient* discharge. Nor is a reason wanting for the presence of contraction at the opening of the circuit in this case; for if the action of the electricity be to preserve and restore the power of contracting in the limb in which the current is inverse, it is easy to suppose that in the case in question this power is so far preserved as to allow the limb to respond to the discharge which attends upon the opening of the circuit.

Nor need there be any difficulty in dealing with the phenomena belonging to the other periods of the second stage. The presence of contraction at the closing as well as at the opening of the circuit, in the case of the limb in which the current is inverse (second stage, first period, in Table), would seem to imply no more than this, that the conditions present in the first stage have not yet come to an end. The absence of contraction at the closing as well as at the opening of the circuit in the limb in which the current is direct (second stage, third period, in Table), may merely be due to the electricity having now so far destroyed the power of contracting as to make the limb incapable of responding to the stronger no less than to the weaker of the discharges acting upon it. The differences in question are merely transitional, nothing more.

A few words will suffice for all that need be said respecting the phenomena which remain to be considered (third stage in Table). For if the charges of the poles play the part which has been ascribed to them, it is to be expected that, by reversing the position of the poles, what was done in either limb by either pole may be undone by the other pole, and that at a certain moment after this reversal the two limbs may be restored to that state of similarity in which they will, as at first, contract similarly on closing and opening the circuit, one or both—at the opening as well as at the closing if the battery power be strong, at the closing only if the battery power be feeble.

It would seem, then, as if the changes of tension, to which attention has been directed, supplied an explanation of the motor phenomena ascribed to the action of the “inverse” and “direct” currents, which, to say the least,

is more intelligible than any which can be found in the action of the currents themselves, and that in fact it is a gain rather than a loss to discard altogether the "inverse" and "direct" currents from the field of operation in which they have hitherto been supposed to play so all-important a part.

It would seem, in fact, that the evidence in this section agrees with that supplied in the two previous sections in leading to the conclusion that muscular relaxation is associated with a state of charge, and muscular contraction with a state of discharge. It would even seem as if all the evidence so far gave countenance to the conclusion that the state of charge may cause muscular relaxation by keeping the molecules of the muscle in a condition of mutual repulsion, and that the state of discharge may lead to muscular contraction by doing away with that state of electrical tension which prevents the molecules of the muscle from yielding to the attractive force, inherent in their physical constitution, which is ever striving to bring them together.

V. *On Electrotonus.*

It is not enough to be content with repeating, after Professor Du Bois Reymond, that the nerve-current and voltaic current are in the same direction in anelectrotonus and in opposite directions in cathelectrotonus, or, after Professor Eckhard, that the activity of the nerve is paralyzed in the former of these states and exalted in the latter. In fact, the subject of electrotonus requires complete revision.

The direction of the nerve-current and voltaic current is found to agree in anelectrotonus and to disagree in cathelectrotonus, if, as is commonly the case, the direction of the former current is from the end to the side of the fibres; but not so if, as may happen, the course of the nerve-current is the reverse of this. In this case the direction of the two currents will agree in cathelectrotonus and disagree in anelectrotonus. Nay, more, there are movements of the needle, corresponding perfectly to those which happen in the two electrotonic states, when the experiment is made upon dead nerve and upon other bodies too, provided these bodies are sufficiently bad conductors of electricity.

If a piece of wire be placed as the piece of nerve is placed in an experiment on electrotonus and dealt with in the same manner, the needle of the galvanometer remains motionless; and so likewise if a piece of cotton or hempen thread moistened with water be substituted for the wire; but not so if the nerve be represented by silk or gutta percha moistened with water. In the latter case, indeed, the needle is found to move as it moves in anelectrotonus and cathelectrotonus when the voltaic poles are placed in the way necessary to produce these two electrotonic conditions. The needle may be at zero before these movements are manifested, or it may not. It is at zero if the electrodes of the galvanometer are homogeneous; it is on this or that side of the zero-point if, as commonly happens,

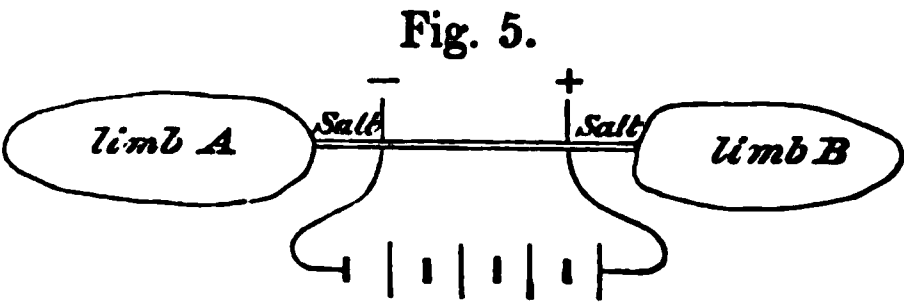
there is some accidental heterogeneity in these electrodes. Still no serious complication in the problem is introduced by the presence of this purely accidental current; for all that it does is to shift in one direction or the other the point from which the electrotonic movements of the needle have to be reckoned. Be this accidental current present or absent, indeed, the degree and direction of the electrotonic movements of the needle remain the same, and it is only the starting-point of the movement which is shifted.

These, then, being the facts, it is difficult to regard the electrotonic phenomena in nerve which are exhibited in the galvanometer as modifications of the nerve-current. The nerve-current, if present, is undoubtedly modified, just as is the accidental current depending upon the heterogeneity of the electrodes of the galvanometer to which attention has just been directed; but the essential workings in electrotonus must be deeper than the nerve-current, deeper even than the nerve. It would seem, indeed, that the nerve-current must in reality play as accidental a part in the phenomena of electrotonus as does the current depending upon heterogeneity in the electrodes of the galvanometer in the experiment in which gutta serena or silk moistened with water is substituted for the nerve. It would seem, indeed, that a given degree of resistance between the voltaic poles is in reality all that is essential to the manifestation of the galvanometric phenomena of electrotonus—a resistance sufficient to pen up free positive electricity at the positive pole and free negative electricity at the negative pole; and that the movements of the needle may be owing to the outflowing or inflowing of this free electricity through the coil of the galvanometer from or to the pole which happens to be nearest to the coil; *for it is found that similar movements to those which happen in electrotonus are witnessed when the part of the nerve acted upon ordinarily by the voltaic current is charged alternately with positive and negative electricity from a friction-machine.* In an experiment on electrotonus, as commonly conducted, the insulation of the circuits of the galvanometer and the battery is sufficient to prevent any passage of the voltaic current proper into the coil, but it is not sufficient to hem in electricity of a higher tension; it is not sufficient to prevent the flowing of a stream of free electricity *from* the positive pole, and *to* the negative pole, of which stream a *part* may pass through the coil of the galvanometer, and so act upon the needle. And hence the movements of the needle of the galvanometer in anelectrotonus and cath-electrotonus; for the movement in anelectrotonus is only that which happens when free positive electricity is passed through the coil, and the movement in cath-electrotonus is only that which happens when free negative electricity is so passed.

Instead of the activity of nerve being paralyzed in anelectrotonus and exalted in cath-electrotonus, a very different conclusion appears to be necessary. Taking the prepared limbs of a frog, and placing the middle portion of the connecting band of nerve belonging to them across the poles of a voltaic battery of which the circuit is open, a drop of salt water is ap-

plied on each side to the portion of nerve beyond the pole. Then, having waited until the salt has set up a state of tetanus in both limbs, the voltaic circuit is closed and opened in turn, with the poles first in one position and then in the other. On closing the circuit, anelectrotonus is set up on the side of the positive pole, cathelectrotonus on the side of the negative pole; and what has to be done is to notice the behaviour of the limb before, during, and after the setting up of these states. In this experiment are four steps, of which the particulars may be tabulated thus:—

Step 1. Poles arranged so as to cause cathelectrotonus in limb A, anelectrotonus in limb B.



Cathelectrotonus on the side of limb A.	Action of salt, causing in limb A	Anelectrotonus on the side of limb B.	Action of salt, causing in limb B
<i>Before.</i>	Tetanus.	<i>Before.</i>	Tetanus.
<i>During.</i>	0.	<i>During.</i>	0.
<i>After.</i>	Momentary contraction.	<i>After.</i>	Tetanus.

Step 2. Poles transposed so as to cause anelectrotonus in limb A, cathelectrotonus in limb B.

Limb A +
 −Limb B

Anelectrotonus after Cathelectrotonus on the side of limb A.	Action of salt, causing in limb A	Cathelectrotonus after Anelectrotonus on the side of limb B.	Action of salt, causing in limb B
<i>During.</i>	Rest at first, then twitchings, progressively increasing in frequency and force	<i>During.</i>	Semi-tetanus at first, then rest.
<i>After.</i>	Tetanus.	<i>After.</i>	Momentary contraction.

Step 3. Poles arranged as at first, so as to cause cathelectrotonus in limb A, anelectrotonus in limb B.

Limb A—		+ Limb B	
Cathelectrotonus after Anelectrotonus on the side of limb A.	Action of salt, causing in limb A	Anelectrotonus after Cathelectrotonus on the side of limb B.	Action of salt, causing in limb B
<i>During.</i>	Semi-tetanus at first, then rest.	<i>During.</i>	Rest at first, then twitchings, progressively increasing in frequency and force.
<i>After.</i>	0.	<i>After.</i>	Tetanus.

Step 4. Poles transposed again, so as to cause anelectrotonus in limb A, cathelectrotonus in limb B.

Limb A +		— Limb B	
Anelectrotonus after Cathelectrotonus on the side of limb A.	Action of salt, causing in limb A	Cathelectrotonus after Anelectrotonus on the side of limb B.	Action of salt, causing in limb B
<i>During.</i>	Rest at first, then twitchings, progressively increasing in frequency and force.	<i>During.</i>	Semi-tetanus at first, then rest.
<i>After.</i>	Semi-tetanus.	<i>After.</i>	0.

In order to explain this experiment, all that is necessary is to realize the fact (for fact it is) that anelectrotonus has to do with a charge from the positive pole, and cathelectrotonus with a charge from the negative pole, and to suppose that these charges react with the natural charge of the animal tissues precisely as they do in the case of the limbs in which inverse and direct currents are passing—in similar cases, that is to say; for, *as regards the phenomena of tension*, the state in anelectrotonus is identical with that which is present in the limb in which the current is inverse (see fig. 4), and in cathelectrotonus with that which is present in the limb in which the current is direct (see fig. 3).

In the first stage of the experiment the facts are—suspension of the tetanus caused by the salt during cathelectrotonus and anelectrotonus alike, return of tetanus after anelectrotonus, momentary contraction only after cathelectrotonus; and these facts are not inexplicable. The tetanus after anelectrotonus, and the momentary contraction only after cathelectrotonus, show, as it would seem, that the power of responding to the action of the salt has been preserved in anelectrotonus, as in the case of the limb in which the current is inverse, and lost in cathelectrotonus, as in the case of the limb in which the current is direct. It is quite intelligible also that the tetanus caused by the salt should be suspended during the continuance of the electrotonic state, if this state be based upon charge, and if this charge have that power of counteracting contraction which would seem to belong to it.

In the other steps of the experiment the two topics which have to be considered are (1) what happens when anelectrotonus follows cathelectrotonus, and (2) what happens when cathelectrotonus follows anelectrotonus.

In the case in which anelectrotonus follows cathelectrotonus the facts are these:—during anelectrotonus, rest at first, then twichings progressively increasing in frequency and force; and after anelectrotonus, tetanus; and so it should be. If, indeed, the power of contracting is impaired in cathelectrotonus and preserved in anelectrotonus, it may be supposed, when anelectrotonus is made to follow cathelectrotonus, that the power of contracting has been so far impaired by the previous state of cathelectrotonus as to oblige the muscles to remain in a state of rest until this power is to a certain degree restored by the state of anelectrotonus; and that the rest at first, and the twichings progressively increasing in force and frequency afterwards, when anelectrotonus is made to follow cathelectrotonus, may be accounted for in this way. Moreover the tetanus upon the cessation of anelectrotonus may be supposed to receive its explanation also, if the action of the anelectrotonus has been to preserve and restore the power of contraction, and if the state of charge upon which that of anelectrotonus is based, has, in some degree at least, the effect of counteracting contraction.

In the case of cathelectrotonus after anelectrotonus, also, what happens is intelligible enough when the same principles of interpretation are applied to the facts. The facts themselves are these:—during cathelectrotonus, first tetanus, then rest; after cathelectrotonus, momentary contraction. Now when cathelectrotonus follows upon anelectrotonus, as a comparison of figs. 3 & 4 will show, there must be discharge. And, further, when either electrotonic state is established, there must be that discharge which attends upon the closing of the circuit in any case; and hence the tetanus which happens when cathelectrotonus is made to follow anelectrotonus; for in addition to being acted upon by the salt, the muscles (the power of contracting is preserved in anelectrotonus) are at this time acted upon by the

two discharges which have been mentioned. Nor is it difficult to find a reason for the rest which follows the tetanus when cathelectrotonus is established, and the momentary contraction which happens when cathelectrotonus passes off. The rest which follows the tetanus under these circumstances is intelligible; for the cathelectrotonus may be supposed to do away with the power of responding to the action of the salt; and the momentary contraction which happens when the cathelectrotonus passes off is intelligible also; for, according to the premises, the cessation of the state of electrotonus implies the cessation of a state which counteracts that action of the salt which causes contraction. Moreover, it is intelligible enough that there should be tetanus after anelectrotonus, and momentary contraction only after cathelectrotonus, if the power of contracting be impaired in the one case and preserved in the other.

Nor is it otherwise with other experiments on electrotonus when care is taken to eliminate what is fallacious.

One and the same explanation, indeed, would seem to apply to the motor phenomena connected with anelectrotonus and cathelectrotonus, and to the motor phenomena connected with the inverse and direct currents; and this explanation is to be found, as it would seem, in the workings, not of the constant current, but of statical electricity.

The electrotonic variations in the conductivity of nerve detected by Professor von Bezold are reserved for future investigation.

April 15, 1869.

Lieut.-General SABINE, President, in the Chair.

The following communications were read:—

I. "On the Source of Free Hydrochloric Acid in the Gastric Juice."

By Professor E. N. HORSFORD, Cambridge, U. S. A. Communicated by T. GRAHAM, F.R.S. Received January 18, 1869.

The long-disputed position of Prout that the gastric juice contains free hydrochloric acid, was at length established by C. Schmidt, who, in an absolute quantitative analysis of the juice, found about twice as much hydrochloric acid as was required to neutralize all the bases present. The prolonged discussion of this subject (now since 1823) has brought to light, through the researches of Lassaigne, Tiedemann and Gmelin, Berzelius, Blondlot, Claude Bernard, Schwann, and numerous others, the unmistakable evidence of the presence of lactic acid and of acid phosphates in the gastric juice, which latter might or might not be due to the presence of lactic or hydrochloric acid. A point of special interest to the chemist and physiologist still remained, and was this:

How could free hydrochloric acid be secreted from the blood, which is an alkaline fluid?

The blood freshly drawn consists of a fluid (the plasma) in which there are swimming myriads of exceedingly minute irregularly spheroidal bodies (the corpuscles). The plasma consists of two bodies, one of which, the fibrine, spontaneously separates from the other, the serum. The corpuscles are little sacs of delicate animal membrane enclosing a fluid. This fluid has an acid reaction, and its ash contains a monobasic alkaline phosphate. The fibrine of the plasma contains, according to Virchow, a glycerophosphate of lime, though the plasma, as a whole, has an alkaline reaction, and contains in its ash a great measure (11 per cent.) of chloride of sodium.

The moist corpuscles constitute about one-half of the blood.

I assume that in healthy digestion, as a consequence of increased flow of blood to the gastric mucous membrane and of the normal elasticity of the walls of the capillaries, there exists in the membrane a condition which is the equivalent of engorgement. Under the pressure which attends this condition, the corpuscles in contact with the walls of the capillaries would discharge a portion of their acid contents, which, with the adjacent plasma, would pass through the walls of the capillaries. This mixture would contain acid phosphates and chlorides.

The mucous membrane of the stomach presents on its inner surface the mouths of numerous microscopic tubes, which, like stockings, are sometimes single blind sacs, or, like gloves, terminate in several blind sacs like the glove fingers. In the bottoms of these tubes, and along their sides, are several closed spherical sacs or cells, containing other lesser sacs and fluid within. The tubes, as a whole, dip down into the spongy tissue that underlies the mucous coat, where they are surrounded by the fluid poured from the network of nutritive capillaries, which fluid, as remarked above, contains acid phosphates and chlorides.

Now by pressure and osmosis a portion of this fluid will pass through the walls of the gastric tubes, and the question is:

Whether the fluid that goes through will contain free hydrochloric acid?

The experiments I have made are conclusive on the principal point.

By employing acid phosphate of lime and common salt I had this advantage, that as increased acidity on the one hand is a just inference from increased alkalinity on the other, and as increased alkalinity would be shown by the precipitation of phosphate of lime (a visible white powder) I could determine the qualitative fact without the difficulties and delay attending on accurate quantitative analysis of the solutions, before and after the experiments on both sides of the membrane.

I employed an acid phosphate of lime of specific gravity 1.117, of a constitution of $3(\text{Ca O P O}_3) + 2\text{P O}_5$, with an amount of phosphate of peroxide of iron present as one to twenty-eight of the acid phosphate of lime. The various other solutions employed were the ordinary laboratory reagents.

On adding ammonia in small quantities to the solution of acid phosphate,

with alternate agitation, it required, as might be inferred, several repetitions before the peroxide with its phosphoric acid became a permanent precipitate, and still several more before the precipitate of phosphate of lime became permanent.

In my earlier experiments, in which I employed parchment-paper, I was embarrassed with the presence of sulphate of lime in the precipitated powder; so that what was at first supposed to be phosphates of lime and iron was found to be in part sulphate of lime. This sulphate was due to imperfectly washed parchment-paper, which still contained sulphuric acid. This difficulty overcome, the experiments were made with parchment-paper prepared from German and Swedish filter-paper, as well as with goldbeater's skin (animal membrane).

I employed acid phosphate of the formula above, with (each by itself) chloride of sodium, chloride of ammonium, chloride of potassium, chloride of calcium, and chloride of magnesium.

I also experimented with acetate of potassa and acid phosphate of lime.

With all of these there was obtained the same kind of evidence of increased acidity on one side and of increased alkalinity on the other, to wit, the powder thrown down from the mixture of acid phosphate and chloride. What successive additions of ammonia had been required to effect, had been accomplished by dialysis.

The same effect took place from a mixture of acid phosphate of soda and chloride of calcium.

It follows from the above, if these experiments fairly represent the case, and from the known composition of the blood, its condition in the walls of the stomach, and the structure of the gastric tubules, that free or uncombined hydrochloric acid must find its way into the bottoms of the gastric tubules, and thence into the cavity of the stomach.

It may be urged that I should show that the acid phosphate pressed from the corpuscles more than neutralizes the alkalinity of the plasma present. In reply it may be said that I present a condition of things in which there is the *kind* of physical change required *going on*, namely, relative augmentation of the corpuscles, under pressure, the concomitant of increased supply of blood to the gastric mucous membrane. Its degree must be inferred from the effects on the secretions, which I have endeavoured to point out, by conducting an experiment under what I conceive to be essentially like conditions, and obtaining the result due to *identical* conditions.

The secretion of hydrochloric acid is of course mixed with acid phosphates and alkaline chlorides.

That such a result as I have arrived at would follow experiment might have been predicted from Graham's researches on dialysis. Phosphates of lime and soda are colloidal relatively to more crystalloidal hydrochloric acid. Graham found that bisulphate of potassa, by dialysis, was resolved into two salts or mixtures of greater and lesser acidity than the original bi-

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sulphate. So he found that acetate of peroxide of iron was resolved by dialysis into hydrated peroxide of iron and free acetic acid. It is possible and probable that the albuminoid bodies present take part in determining the contrast between colloid and crystalloid bodies. Graham found that by dialysis he could separate free hydrochloric acid from the gastric juice thrown up in vomiting.

It may be further objected that anatomists are not agreed as to the structure of the corpuscles. But it will be seen that there is no more required than may be regarded as established. The corpuscles act in many particulars, if not in all, as if they were membranous sacs more or less distended with fluid. They may be swollen by immersion in a thinner (less colloid) fluid, and reduced by immersion in a more colloid fluid—that is, they are susceptible of endosmosis and exosmosis as membranous sacs would be. In their ordinary condition as seen under a microscope, they present the appearance of collapsed spherical or oval sacs or cells. They appear as double concave disks. In swelling (by endosmosis) the lowest part of each concavity is the last to take on the spherical contour, just as it would do if the corpuscles were membranous sacs. The corpuscles sometimes so collapse (by exosmosis) that one-half of the hollow sphere is reversed, while the other half retains its form unchanged, the former sitting like a cup in the latter—a conformation inconceivable on the theory of homogeneity of the corpuscles as a whole. Crystallizable substances may be extracted from the corpuscles by pressure and by endosmosis. They must have been in solution in order to crystallization, and solution involves a fluid. The liquid expressed from the corpuscles has an acid reaction, and contains an organic acid and acid phosphates. It contains, among other bodies, the hæmatoidin of Virchow. The ash of these crystals consists almost wholly of metaphosphates*, which point directly to tribasic phosphoric acid in solution, combined with one atom of fixed base, which is inconceivable unless separated by membrane from the plasma, which is always alkaline.

In fine, whatever other peculiarities the blood-corpuscles may possess, they have the requisites for furnishing acid phosphates in solution under pressure, such as must attend engorgement of the capillaries in the walls of the stomach.

Let us glance at what takes place in all probability as the acid fluid enters the gastric tubules. They are surrounded by a mixture of hydrochloric acid, acid salts, neutral salts, and albuminoid bodies. Dialysis must be repeated, and a stronger acid solution pass into the sacs or cells contained in them. The sacs swelling by endosmosis, and corroded by the acid, must at length burst, and the liquid contents, together with the disintegrated and partially digested membrane of the sacs, pass out into the

* The ether-extract of the blood-corpuscles yields, according to Schwann, an ash containing acid phosphate of soda. Owen, Rees, and Berzelius maintained the existence of oleo-phosphoric acid in the corpuscles.

stomach, to constitute the gastric juice, the free hydrochloric acid, acid phosphates and chlorides, and the albuminoid bodies and disintegrated tissue (*the pepsine* ?) to act in the liquefaction of food.

II. "Contributions to the History of Explosive Agents." By
F. A. ABEL, F.R.S., For. Sec. C. S. Received March 9, 1868.

(Abstract.)

The degree of rapidity with which an explosive substance undergoes metamorphosis, as also the nature and results of such change, are, in the greater number of instances, susceptible of several modifications by variation of the circumstances under which the conditions essential to chemical change are fulfilled.

Excellent illustrations of the modes by which such modifications may be brought about are furnished by gun-cotton, which may be made to burn very slowly, almost without flame, to inflame with great rapidity, but without development of great explosive force, or to exercise a violent destructive action, according as the mode of applying heat, the circumstances attending such application of heat, and the mechanical condition of the explosive agent, are modified*. The character of explosion and the mechanical force developed, within given periods, by the metamorphosis of explosive mixtures such as gunpowder, is similarly subject to modifications; and even the most violent explosive compounds known (the mercuric and silver fulminates, and the chloride and iodide of nitrogen) behave in very different ways, under the operation of heat or other disturbing influences, according to the circumstances which attend the metamorphosis of the explosive agent (*e.g.* the position of the source of heat with reference to the mass of the substance to be exploded, or the extent of initial resistance opposed to the escape of the products of explosion).

Some new and striking illustrations have been obtained of the susceptibility to modification in explosive action possessed by these substances.

The product of the action of nitric acid upon glycerine, known as nitro-glycerine or glonoine, which bears some resemblance to chloride of nitrogen in its power of sudden explosion, requires the fulfilment of special conditions for the development of its explosive force. Its explosion by the simple application of heat can only be accomplished if the source of heat be applied, for a protracted period, in such a way that chemical decomposition is established in some portion of the mass, and is favoured by the continued application of heat to that part. Under these circumstances, the chemical change proceeds with very rapidly accelerating violence, and the sudden transformation, into gaseous products, of the heated portion eventually results, a transformation which is instantly communicated

* Proceedings of the Royal Society, vol. xiii. pp. 205 *et seq.*

throughout the mass of nitroglycerine, so that confinement of the substance is not necessary to develop its full explosive force. This result can be obtained more expeditiously and with greater certainty by exposing the substance to the concussive action of a detonation produced by the ignition of a small quantity of fulminating powder, closely confined and placed in contact with, or proximity to, the nitroglycerine.

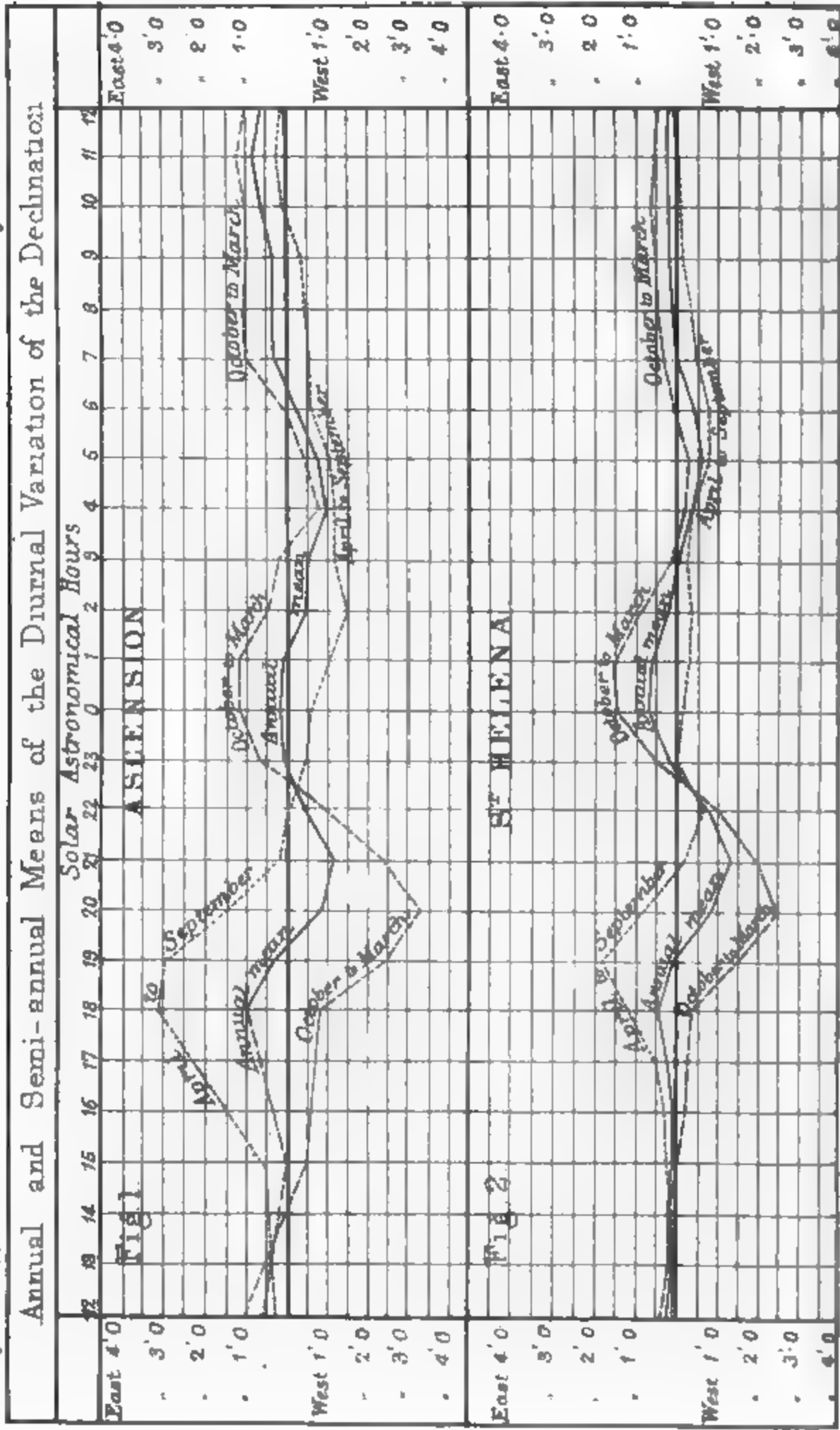
The development of the violent explosive action of nitroglycerine, freely exposed to air, through the agency of a detonation, was regarded until recently as a peculiarity of that substance; it is now demonstrated that gun-cotton and other explosive compounds and mixtures do not necessarily require confinement for the full development of their explosive force, but that this result is attainable (and very readily in some instances, especially in the case of gun-cotton) by means similar to those applied in the case of nitroglycerine.

The manner in which a detonation operates in determining the violent explosion of gun-cotton, nitroglycerine, &c., has been made the subject of careful investigation. It is demonstrated experimentally that the result cannot be ascribed to the direct operation of the heat developed by the chemical changes of the charge of detonating material used as the exploding agent. An experimental comparison of the mechanical force exerted by different explosive compounds, and by the same compound employed in different ways, has shown that the remarkable power possessed by the explosion of small quantities of certain bodies (the mercuric and silver-fulminates) to accomplish the detonation of gun-cotton, while comparatively very large quantities of other highly explosive agents are incapable of producing that result, is generally accounted for satisfactorily by the difference in the amount of force suddenly brought to bear in the different instances upon some portion of the mass operated upon. Most generally, therefore, the degree of facility with which the detonation of a substance will develop similar change in a neighbouring explosive substance may be regarded as proportionate to the amount of force developed within the shortest period of time by that detonation, the latter being, in fact, analogous in its operation to that of a blow from a hammer, or of the impact of a projectile.

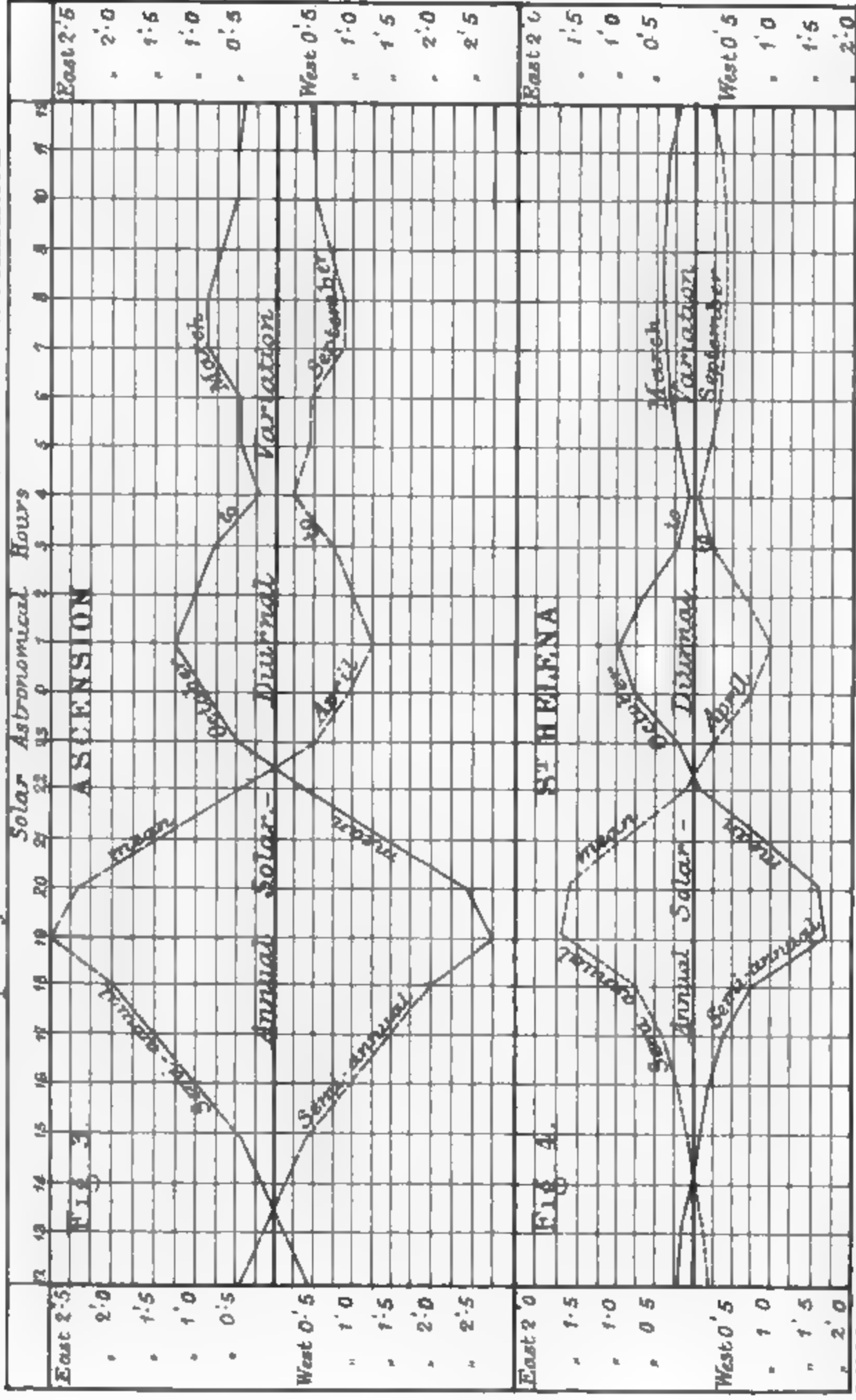
Several remarkable results of an exceptional character have been obtained, which indicate that the development of explosive force under the circumstances referred to is not always simply ascribable to the sudden operation of mechanical force. These were especially observed in the course of a comparison of the conditions essential to the detonation of gun-cotton and of nitroglycerine by means of particular explosive agents (chloride of nitrogen, &c.), as well as in an examination into the effects produced upon each other by the detonation of those two substances.

The explanation offered of these exceptional results is to the effect that the vibrations attendant upon a particular explosion, if synchronous with those which would result from the explosion of a neighbouring substance





Semi-annual Inequality of the Solar-diurnal Variation of the Declination





in a state of high chemical tension, will, by their tendency to develop those vibrations, either determine the explosion of that substance, or at any rate greatly aid the disturbing effect of mechanical force suddenly applied, while, in the instance of another explosion, which develops vibratory impulses of different character, the mechanical force applied through its agency has to operate with little or no aid, greater force, or a more powerful detonation, being therefore required in the latter instance to accomplish the same result.

Instances of the apparently simultaneous explosion of numerous distinct and even somewhat widely separated masses of explosive substances (such as simultaneous explosions in several distinct buildings at powder-mills) do not unfrequently occur, in which the generation of a disruptive impulse by the first or initiative explosion, which is communicated with extreme rapidity to contiguous masses of the same nature, appears much more likely to be the operating cause, than that such simultaneous explosions should be brought about by the direct operation of heat and mechanical force.

A practical examination has been instituted into the influence which the explosion of gun-cotton through the agency of a detonation, exercises upon the nature of its metamorphosis, upon the character and effects of its explosion, and upon the uses to which gun-cotton is susceptible of application.

III. "Results of Magnetical Observations made at Ascension Island, Latitude $7^{\circ} 55' 20''$ South, Longitude $14^{\circ} 25' 30''$ West, from July 1863 to March 1866." By Lieut. ROKEBY, R.M. Reduced by G. M. WHIPPLE, Magnetical Assistant at the Kew Observatory. Communicated by B. STEWART, LL.D. Received March 11, 1869.

On leaving England for Ascension Island in May 1862, Lieut. Rokeby was supplied by General Sabine with the following instruments for the purpose of making observations of magnetical variation and intensity, viz. :—

A portable declinometer and unifilar for absolute observations of declination and horizontal intensity, a Barrow's dip-circle (No. 24), a differential declinometer, and a differential bifilar.

The differential declinometer and the bifilar were erected at George Town, Ascension, in August 1862, and bihorary observations commenced; but in consequence of instability in the supports of the instruments, caused probably by the shifting of the volcanic cinders which formed the ground at the observing-station, the observations made exhibit frequent discrepancies. The whole of the bifilar observations, and all the differential declinometer observations prior to June 1864, have therefore been omitted from the present discussion.

These observations were discontinued in June 1866, when Lieut. Rokeby left the Island.

Observations of absolute horizontal force and dip were made on the Green Mountain, Ascension, once every month from July 1863 to March 1866, two months in 1865 excepted.

Observations were not made with the portable declinometer.

Observations of Horizontal Force and Dip.

The horizontal, vertical, and total forces (Table No. 1) are calculated to English measure; one foot, one second of mean solar time, and one grain being assumed as the limits of space, of time, and of mass.

The vertical and total forces are obtained from the absolute measures of the horizontal force and the dip.

The observations of dip (Table No. 1) were made in every instance save one with the needle marked A 2.

For the observations of deflection and vibration taken each month for absolute measure of horizontal force, the same magnet (collimator 5) has always been employed.

The moment of inertia of the magnet, with its stirrup for different degrees of temperature, and the coefficients in the corrections required for the effects of temperature and of terrestrial magnetic induction on the magnetic moment of the magnet, were determined in 1858 at the Kew Observatory by the late Mr. Welsh.

That these corrections held good in 1862 was proved by the agreement of the horizontal force obtained at Kew with this instrument previous to its departure, with the value of the force determined by the observatory unifilar.

The moment of inertia of the magnet with its stirrup is 5.3828 at 60° Fahr.

The induction-coefficient $\mu = 0.000252$.

The correction for error of graduation of the deflection-bar at 1.0 foot is 0.00000 foot, and at 1.3 foot 0.00003 foot.

The formula used for determining the temperature correction was

$$q(t_0 - 35^\circ) + q'(t_0 - 35^\circ)^2,$$

where t_0 is the observed temperature, 35° F. being the adopted standard temperature.

The values of q and q' for the magnet used are respectively 0.00011035 and 0.000000581.

The observed times of vibration have been corrected for rate of chronometer when it has exceeded five seconds per day. A correction has also been applied for the effect of torsion on the suspending thread.

The initial and terminal semiarcs of vibration have always been less than 30', consequently no correction was requisite on this account.

The time of one vibration is derived from the mean of twelve observations of the time of 100 vibrations.

The angles of deflection given are each the mean of two determinations.

In deducing from these observations the ratio and product of the magnetic moment m of the magnet, and the earth's horizontal magnetic intensity X , the induction- and temperature-corrections have always been applied.

In calculation of the ratio $\frac{m}{X}$, the third and subsequent terms of the series $1 + \frac{P}{\gamma^2} + \frac{Q}{\gamma^4} + \&c.$ have always been omitted. The value of the constant P was found to be -0.00291 , by the mean of ten determinations obtained each from six pairs of deflection-observations at distances 1.0 and 1.3 foot.

The mean of the values of $\frac{m}{X}$ derived from deflection-observations at the distances 1.0 foot and 1.3 foot has been used in calculating the measure of horizontal force.

Observations made with the Differential Declinometer.

Observations were made of the scale-reading of the differential declinometer at 3 A.M., and hourly from 6 A.M. to midnight, daily, from December 1863 to June 1865, from which date to the conclusion of the series the 3 A.M. observations were discontinued.

The observations from the commencement until May 1864 were found on inspection to be valueless for the purposes of reduction, for the reason assigned in the introduction.

The two years' records, June 1864 to 1866, were treated according to the method employed by General Sabine in the reduction of declination-observations.

In the first place, hourly means were taken for each month. All observations which vary from these means to a greater extent than 4'.0 were then rejected, and new means taken of the unrejected observations.

The mean reading for each month was then computed; finally, the hourly means were subtracted from the monthly means.

Table 2 shows these differences as derived from the mean of two years' observations, excepting in the case of 15 hours, which is the result of one year only.

In the Table, the sign $+$ represents the north pole of the magnet to the east of the mean position, and the sign $-$ that it was to the west of the mean.

At the bottom of the Table semiannual and annual means are given, and these means are exhibited in the form of curves in the diagram accompanying the paper.

Figure 1 shows the differences of the semiannual and annual means from the normal position, which is represented by the straight horizontal line.

In figure 3 the annual mean is represented as a straight line, and the

curves show the deviations of the semiannual means from it. In both figures the curves between twelve and fifteen hours and fifteen and eighteen hours are interpolated.

Figures 2 and 4 are copied from General Sabine's *St. Helena Observations*, vol. ii., and show the similarity between the movements of the magnet at Ascension and St. Helena.

The Tables annexed to this Paper are preserved for reference in the Archives.

April 22, 1869.

JOSEPH PRESTWICH, Esq., Vice-President, in the Chair.

Alphonse DeCandolle, of Geneva, Charles Eugène Delaunay, of Paris, and Louis Pasteur, of Paris, were proposed for election as Foreign Members, and notice was given from the Chair that these gentlemen would be ballotted for at the next Meeting.

The following communications were read :—

- I. "Description of *Parkeria* and *Loftusia*, two gigantic Types of Arenaceous Foraminifera." By Dr. CARPENTER, V.P.R.S., and H. B. BRADY, F.L.S. Received March 18, 1869.

(Abstract.)

The Authors of this Memoir commence by referring to the separation of the series of *Arenaceous* Foraminifera from the *Imperforate* or *Porcellanous*, and from the *Tubular* or *Vitreous*, first distinctly propounded in Dr. Carpenter's 'Introduction to the Study of the Foraminifera' (1862), on the basis of the special researches of Messrs. Parker and Rupert Jones; who had pointed out that whilst there are several genera in some forms of which a cementation of sand-grains into the substance of the calcareous shell is a common occurrence, there are certain genera in which a "test" formed entirely of an aggregation of sand-grains takes the place of a calcareous shell; and that these genera constitute a distinct Family, to which important additions might probably be made by further research.

The propriety of this separation of the *Arenacea* from the calcareous-shelled Foraminifera has been fully recognized by Prof. Reuss, the highest Continental authority upon the group; who had come to accept the principle laid down in Dr. Carpenter's successive Memoirs (*Phil. Trans.* 1856–1860), that the *texture of the shell* is a character of fundamental importance in the classification of this group, the *plan of growth* (taken by M. d'Orbigny as his primary character) being of very subordinate value; and who had, on this basis, independently worked out a Systematic Arrangement of the entire group, which presents a most remarkable correspondence with that propounded by Dr. Carpenter and his coadjutors. And their anticipation of important additions to the Arenaceous series has

been fully borne out, on the one hand by the discovery of several most remarkable new forms at present existing at great depths in the Ocean, which has been made by the dredgings of M. Sars, Jun., and those of the 'Lightning' Expedition, and on the other by the determination of the real characters of two fossils, one of the Cretaceous, and the other probably of the earlier Tertiary period, which prove to be gigantic examples of the same type.

The first of these, discovered by Prof. Morris more than twenty years ago in the Upper Greensand near Cambridge, was long supposed to be a Sponge; but his more recent discovery of two specimens which had been but little changed by fossilization, led him to suspect their Foraminiferal character; and this suspicion has been fully confirmed by the careful examination made of their structure by Dr. Carpenter, to whom he committed the inquiry, and by whom, with his concurrence, the name *Parkeria* was assigned to the genus. The second, which was obtained by the late Mr. W. K. Loftus from "a hard rock of blue marly limestone" between the N.E. corner of the Persian Gulf and Ispahan, bears so strong a resemblance in its general form and mode of increase to the genus *Alveolina*, that its Foraminiferal character was from the first recognized by the discoverer; but as all the specimens brought by Mr. Loftus had undergone considerable alteration by fossilization, their minute structure, though carefully studied by means of transparent sections, could not in the first instance be satisfactorily made out. When, however, Dr. Carpenter's investigation of *Parkeria*, with the full advantage of specimens but little changed by fossilization, revealed the very remarkable plan of its structure, the investigation of this type was resumed by Mr. Brady (who assigned to it the name *Loftusia*), with the new light thence derived: for as transparent sections of infiltrated *Parkeriæ* furnish a middle term of comparison between specimens of the same type which retain their original character, and transparent sections of infiltrated *Loftusiæ*, the last-mentioned can now be interpreted by reference to the preceding; so that the obscurities which previously hung over their minute structure have been almost entirely dissipated.—The description of the structure of *Parkeria* in this Memoir is by Dr. Carpenter, and that of the structure of *Loftusia* by Mr. H. B. Brady; but each has gone over the work of the other, and can testify to its correctness.

The specimens of *Parkeria* which have been collected by Prof. Morris* are spheres varying in diameter from about 3-4ths of an inch to about 1½

* Since this Memoir was completed, the Author has learned that Mr. Harry Seeley, of Cambridge, has collected several specimens of this type, and has been studying it independently with a view to publication. And Mr. Henry Woodward has placed in his hands a specimen from the Upper Greensand in the Isle of Wight, which is not less than 2½ inches in diameter. It is interesting to remark that the "nucleus" of a smaller specimen from the same locality consists of a considerable number of chambers arranged in a *spire*, the structure of its concentric spherical layers being exactly the same as in the specimens described in the text.

inch. The character of their external surface differs considerably in different individuals; but the Author gives reason for believing that it was originally tuberculated, like a mulberry, and that the departures from this have been the result of subsequent abrasion. The entire sphere is composed of a great number of concentric layers, all of which, except the innermost, are arranged with very considerable regularity around a central "nucleus," which consists of five chambers, disposed in *rectilineal* sequence, thus unmistakeably indicating the Foraminiferal character of the organism, which might otherwise have remained in doubt, on account of the entire divergence from any known type presented in the structure of the concentric layers. The first of these layers is moulded (as it were) on the exterior of the nucleus, and partakes of its elongated form; but the parts of every additional exogenous layer are so arranged as to bring about a gradual approximation to the spherical form, which is afterwards maintained with great constancy. Each layer may be described as consisting of a lamella of "labyrinthic structure" (that is, of an assemblage of minute chamberlets, whose cavities communicate freely with one another), separated from the contiguous lamellæ by an "interspace," which is traversed by "radial tubes," that pass from each lamella to the one external to it. All these structures, in common with the chamber-walls and septa of the "nucleus," are built up by the *aggregation of sand-grains of very uniform size*. These sand-grains are found to consist of *Phosphate of lime*; and they seem to be united by a cement composed of *Carbonate of lime*, which was probably exuded by the animal itself. Although there is a very general uniformity in the thickness of the successive layers, the proportion of their several components varies considerably in different parts of the sphere. In those which immediately surround the nucleus, the solid lamellæ, which are composed of labyrinthic structure, are comparatively thin; whilst the interspaces which separate them from one another are very broad, so that the radial tubes which traverse these interspaces are very conspicuous. As we pass outwards, we find the labyrinthic lamellæ increasing in thickness, whilst the breadth of the interspaces diminishes in the same degree, until we meet with layers in which the interspaces are almost entirely replaced by labyrinthic structure. With this increased development of the labyrinthic structure in the concentric lamellæ themselves, we find it extending between one lamella and another, as an investment to the radial tubes; thus forming "radial processes" of a sub-conical form, which occupy a considerable part of what would otherwise be the interspaces between the successive lamellæ. Still every lamella is separated from that which invests it (except where brought into connexion with it by its radial processes) by a system of cavities, which are in free communication with each other, and which may be collectively designated the "interspace-system;" and from this system the labyrinthic structure of the investing lamella is entirely cut off by an impervious wall, which bounds it upon its *inner* side; whilst its chamberlets open freely upon

the *outer* side of the lamella, into what, when it is newly formed, is the surrounding medium, but, when it has itself been invested by another layer, into its "interspace-system."—In the larger of the two non-infiltrated specimens which have furnished the materials for the present description, the number of concentric layers is 40, and their average breadth about 1-65th of an inch.

The Author discusses the mode in which this composite structure was formed; and comes to the conclusion that the production of each new layer was probably accomplished by the instrumentality of the sarcodic substance, which not only filled the chamberlets of the preceding layer, but projected beyond it; that the radial processes were first built up like the columns of a Gothic cathedral, and that their impervious investing wall spread itself from their summits, so as to form a continuous lamella over the sarcodic layer, in the manner that the summits of such columns extend themselves to form the arched roof of the edifice; and that on the floor of the new layer thus laid the partitions of the chamberlets were progressively built up by the agency of the sarcodic substance conveyed to the outer surface of that floor through the radial tubes. The author further argues, from the analogy of living *Foraminifera*, that notwithstanding the indirectness of the communication between the cavitory system of the inner layers and the external surface, the whole of that system (consisting of the labyrinthic structure of the successive lamellæ, and of the interspaces which separate them) was occupied during the life of the animal by its sarcode-body.

The *plan of growth* in *Loftusia* is stated by Mr. Brady to differ extremely from that of *Parkeria*, whilst its *intimate structure*, on which its physiological condition must have depended, is essentially the same; thus affording a conspicuous example of the validity of the principle of Classification already referred to. This difference is indicated by its shape, which closely resembles that of many *Alveolina* and *Fusulina*; being a long oval, frequently tapering almost to a point at either end, though sometimes obtusely rounded at its extremities. Of two large and perfect examples in the collection of the late Mr. Loftus, one measures $3\frac{1}{4}$ inches by 1 inch, the other $2\frac{1}{4}$ inches by $1\frac{1}{4}$ inch. A transverse section at once indicates that the plan of growth is a spiral, formed by the winding of a continuous lamina around an elongated axis; the general disposition of the chambered structure being very similar to that which would be produced if one of the simple *Rotalians* were thickened and drawn out at the umbilici. The space inclosed by the *primary lamina* is divided into chambers by longitudinal septa, which may be regarded as ingrowths from it, extending, not perpendicularly (as in *Alveolina*), but very obliquely. The chambers, separated by these principal or *secondary* septa, are long and very narrow, and extend from one end of the body to the other. Their cavities are further divided into chamberlets by *tertiary* ingrowths, which are gene-

rally at right angles to the septa or nearly so, but are otherwise irregular in their arrangement. No large primordial chamber, such as is common among Foraminifera, has been yet discovered in *Loftusia*; but its absence cannot be certainly affirmed. In fully grown specimens the turns of the spire, which succeed each other with tolerable regularity at intervals of from 1-50th to 1-30th of an inch, are usually from twelve to twenty in number; but as many as twenty-five have been counted in one instance, and a yet larger number might not improbably be met with. The *spiral lamina* and its prolongations, forming the accessory skeleton, are all constructed of almost impalpable grains of sand, which is proved by analysis to have consisted of *Carbonate of Lime*, united by a cement of the same material.

The Author then describes in detail the several components of the fabric of *Loftusia*, and compares them with the corresponding parts of *Parkeria*. The continuity of increase of the spiral lamina always leaves an open fissure between its last-formed margin and the surface of the previous whorl; and through this aperture the whole system of chambers included within its successive laminæ communicates with the exterior, through the passages between their cavities, which are left in the building up of the septa. As already explained, the labyrinthic structure takes its origin from the *inner* surface of the impervious spiral lamina, the septa being directed towards the central axis. These ingrowths have in many instances the form of tubular columns, which traverse the chambers in a radial direction (*i. e.* perpendicular to the spiral lamina), terminating either on the septum of the previous chamber, or on the exterior wall of the preceding whorl of chambers. But these tubes do not seem to be homologous with the "radial tubes" of *Parkeria*, whose relations differ in important particulars. The range of variation in a number of specimens, as to the amount of the "secondary" and "tertiary" ingrowths which divide and subdivide the chambers in *Loftusia* is very great. The principal office fulfilled by this accessory skeleton seems to be that of a support to the primary spiral lamina, imparting the necessary solidity to the organism. The degree of subdivision of the chambers into chamberlets seems to have little bearing on the general economy of the animal.

The Author attempts to determine from the other Foraminifera, of which the remains are found associated in the same Limestone with those of *Loftusia*, what was its probable Geological age, and under what conditions it was deposited; and he thence draws the conclusion that the rock belongs to the lowest portion of the Tertiary period, presenting a microzoic Fauna very similar to that of some of our Miliolite Limestones, but richer in the small arenaceous *Rhizopods*; and that the sea-bottom was a soft Calcareous mud lying at a depth of from 90 to 100 fathoms.

II. "On Remains of a large extinct Lama (*Palauchenia magna*, Owen) from Quaternary deposits in the Valley of Mexico." By Professor OWEN, F.R.S. &c. Received March 22, 1869.

(Abstract.)

The author premises to his descriptions of these remains a summary of the evidence of Fossil Cameloid Quadrupeds in the memoirs and works of Lund, Pictet, De Blainville, Gervais, Burmeister, and Leidy, deferring the further analysis and comparison of the descriptions by the latter palæontologist to the conclusion of the present paper. The subject of it consists of casts and photographs of fossils discovered by Don Antonio del Castillo, mining engineer, in a posttertiary deposit beneath volcanic tufa in the Valley of Mexico.

The fossils include the dentition of the left ramus of the lower jaw, wanting the incisors; also the series of cervical vertebræ, wanting the first or atlas.

Assuming the incisors to be in number as in Ruminants, the dentition of this mandibular ramus is formularized as :— $i\ 3, c\ 1, p\ 3, m\ 3 = 10$.

Of the grinding-teeth, the three molars, with the last two premolars, form a close-set or continuous series of five teeth, the first of which ($p\ 3$) is small, simple, conical, and obtusely pointed. A still smaller or rudimental premolar ($p\ 2$ or $p\ 1$) is situated in the long diastema between the series of five teeth and the canine; the latter tooth is relatively smaller than in the Camel.

Detailed descriptions are given, illustrated by drawings, of each of the teeth, from which the author shows that they have belonged to a Cameloid species, as large as the larger variety of existing Dromedary, but with modifications of the teeth, testifying to a closer affinity with the Lama and Vicugna.

He then proceeds to give detailed descriptions, with figures, of the cervical vertebræ; they present the intraneural position of the vertebro-arterial canals characteristic of the *Camelidæ*, and of the extinct Perissodactyle genus *Macrauchenia*; and the comparisons of the fossil vertebræ are made with the corresponding one of that extinct genus and of the existing species of *Camelus* and *Auchenia*.

The result of the comparison concurs with that of the dental characters in demonstrating the former existence in America of a Cameline Ruminant as large as the largest variety of living Camel or Dromedary, with closer affinities to the Lamas and Vicuñas, yet with such departures from the dental and osteological characters of *Auchenia*, Illig., as justify the author in indicating them by the generic or subgeneric term *Palauchenia*, which he proposes for such extinct form of American Cameline quadruped.

The author, in conclusion, refers more at large to Prof. Leidy's descriptions of *Procamelus occidentalis*, Leidy, and *Camelops Kansanus*, Leidy, pointing out the more important particulars wherein they differ from *Palauchenia*.

chenia magna, Owen, and dwelling on the evidences of a progress from a more generalized to a more specialized type of Ruminant dentition in the extinct Cameloid forms succeeding each other, from the old Pliocene of Nebraska to the new or Postpliocene of Mexico.

Tables of dimensions of teeth and vertebræ of *Palauchenia*, *Auchenia*, and *Camelus*, and drawings arranged for one folding and three 4to plates, accompany the memoir.

III. "On the Proof of the Law of Errors of Observations."

By M. W. CROFTON, F.R.S. Received March 24, 1869.

(Abstract.)

The object of this Paper is to give the mathematical proof, in its most general form, of the law of single errors of observations, on the hypothesis that each error in practice arises from the joint operation of a large number of independent sources of error, each of which, did it exist alone, would occasion errors of extremely small amount as compared generally with those actually produced by all the sources combined. This proof is contained in a process given for a different object, namely, Poisson's generalization of Laplace's investigation of the law of the mean results of a large number of observations, to be found in the 'Connaissance des Temps' for 1827, and also in his 'Recherches sur la Probabilité des Jugements;' it is also reproduced in Mr. Todhunter's able 'History of the Theory of Probability.' It is not therefore pretended that any new results are arrived at in the present Paper. Considering, however, the importance and celebrity of the question, and the refined and difficult character of Poisson's analysis, it will not probably be deemed superfluous to show how the same law may be demonstrated with equal generality, in a much more simple and elementary manner. The difficulty of the general proof seems indeed to have been so extensively felt, that several attempts have been made to simplify it. However, so far as the present writer is aware, no proof has been given, except Poisson's, which is not open to grave objection, as based upon unjustifiable assumptions, or as unduly limiting the generality of the investigation.

The mathematical reasoning in this Paper is based entirely on the above-mentioned hypothesis as to the causation of error, namely, that errors in *rerum naturâ* result from the superposition of a large number of minuter errors arising from a number of independent sources. The laws of these elementary errors are supposed entirely unknown, no further restriction whatever being imposed on the generality of the investigation; as would be the case, for instance, were we to assume (as has sometimes been done) that each independent source gives positive and negative errors with equal facility. To decide fully how far the above hypothesis (which seems now to be generally accepted) really agrees with facts, is an extremely subtle question in

philosophy,—one which probably never can be more than partially resolved. Still, even a cursory and superficial examination of a few particular cases seems to show that, far from being a mere arbitrary assumption, it is at least a reasonable and probable account of what really does take place in nature, in many large classes of errors of observations. The history of practical astronomy, in particular, seems to prove that, whatever doubt may be entertained of its exactness as applied to the errors of rude and primitive observers, we may safely accept it in the case of the refined and delicate observations of modern astronomers.

It would be scarcely possible in this Abstract to convey any clear idea of the mathematical analysis employed in reducing the above hypothesis to calculation. It will suffice to remark that, whereas in the processes given by Laplace and Poisson, when applied to the problem before us, the elementary component errors are at first supposed of finite magnitude, and finite in number, and the results are afterwards modified for the supposition that the magnitude of the errors becomes infinitesimal and their number infinite; much simplicity is gained in this Paper by making these suppositions at the commencement. Also, instead of taking a simultaneous view of all the elementary errors, as affecting the actual or resultant error, the latter is considered as produced by the superposition of some one of the elementary errors upon the error produced by the combination of all the others. We are thus led to examine the infinitesimal change produced in a given finite error, as expressed by a given function, by the superposition of a new infinitesimal error; and from the analytical expression arrived at, it is shown how to find the form of the function of error resulting from the combination of an infinite number of given infinitesimal errors. This form is found to be altogether independent of the nature or laws of the component errors. If we assume the following data as known, viz.

m = sum of the mean values of the component errors,

h = sum of the mean values of the squares of component errors,

i = sum of squares of the mean values of component errors,

it is proved that the probability of the actual resulting error being found to lie between x and $x + dx$ is

$$\frac{1}{\sqrt{2\pi(h-i)}} e^{-\frac{(x-m)^2}{2(h-i)}} dx.$$

This result will be found to agree with Poisson's.

April 29, 1869.

Lieut.-General SABINE, President, in the Chair.

Pursuant to notice given at the last Meeting, Alphonse DeCandolle, of Geneva; Charles Eugène Delaunay, of Paris; and Louis Pasteur, of Paris, were ballotted for and elected Foreign Members of the Society.

The following communications were read:—

I. "On a certain Excretion of Carbonic Acid by Living Plants."

By J. BROUGHTON, B.Sc., F.C.S., Chemist to the Cinchona Plantations of the Madras Government. Communicated by J. D. HOOKER, M.D., F.R.S. Received March 31, 1869.

[Abstract.]

While the author was engaged in some experimental determinations of the changes that take place in the composition of the Cinchona barks after being taken from the tree, he noticed a somewhat singular circumstance, which induced him to institute a series of experiments, by which he discovered that the various parts of living plants excrete carbonic acid, not only in their normal condition, but after they have been deprived for days together of all access of oxygen. The experiments were mostly made on cut portions of the plants; but experiments were also made, for control, on plants as they actually grow. The deprivation of oxygen was effected sometimes by Sprengel's air-pump, sometimes by substituting for air an atmosphere of hydrogen or nitrogen; while comparative experiments were made on plants supplied with air that had been freed from carbonic acid. The main conclusions to which he was led are those enunciated by the author:—

1st. That nearly all parts of growing plants evolve carbonic acid in considerable quantities, quite independently of direct oxidation.

2nd. That this evolution is connected with the life of the plant.

3rd. That it is due to two causes, namely, to previous oxidation, resulting after a lapse of time in the production of carbonic acid, and to the separation of carbonic acid from the proximate principles of the plant while undergoing the chemical changes incident to plant-growth.

II. "On the Causes of the Loss of the Iron-built Sailing-ship 'Glenorchy.'" By ARCHIBALD SMITH, Esq., M.A., LL.D., F.R.S. Received April 15, 1869.

When the loss of an iron-built vessel has been caused by an error in the direction of her course by dead reckoning, as derived from her course by compass, it is a question of scientific interest whether the error has or has not arisen from an error in the assumed deviation of the compass. By careful consideration of all the circumstances of the case, and by piecing together the generally scanty fragments of information which can be obtained as to the magnetic state of the ship, a probable or certain answer to this question may be given more frequently than might be supposed possible by those who do not know how perfectly definite and well ascertained the laws of the deviation of the compass are, how small is the number of quantities involved which are peculiar to each particular ship, and from what apparently slight indications an approximate estimate of the numerical values of these quantities can be made.

The case the circumstances of which I now propose to lay before the Royal Society, is one in which it appears to me that a positive answer to the question can be given. It will, I hope, be found to have some interest as an example of the manner in which such an answer can be elicited from the data. It may have some scientific interest as the first case in which any information as to the magnetic character of an English merchant-ship has been published since the publication of the Third Report of the Liverpool Compass Committee in 1861; and I think it will be found to have much practical interest, as bringing into prominence a particular error of great importance, not as yet, I believe, ascertained or corrected in the usual course of adjustment of compasses in merchant-ships, even by the most experienced and skilful compass-adjusters, but which, ever since the mode of ascertaining and correcting it without heeling the ship was given in the 'Admiralty Manual for the Deviation of the Compass' in 1862, has been ascertained, and when necessary corrected, in the ships of the Royal Navy, viz. the Heeling Error. The case to which I refer is the loss of the ship 'Glenorchy' of Glasgow, on the Kish Bank, in Dublin Bay, on the 1st of January 1869, on which a court of inquiry was held under the direction of the Board of Trade in pursuance of the Merchant Shipping Act. In examining this case I have had the advantage, by the permission of the Board of Trade, of perusing the evidence taken before the Court of inquiry, and the report of the Court. I have also had the advantage of discussing the nautical as well as the magnetical circumstances of the case with Captain Evans, F.R.S., the highest authority in all that relates to such an inquiry, and who permits me to state his concurrence in the conclusions at which I have arrived; and above all, I have to express my obligations to Mr. William Fleming, compass-adjuster, James Watt Street, Glasgow, for the full particulars with which he has kindly furnished me of the deviations and correction of the compasses of the 'Glenorchy'—information without which the results of this inquiry would have been in a great measure conjectural.

The 'Glenorchy' was an iron-built sailing-ship of 1200 tons, having an iron poop, with a wooden deck laid upon iron beams, with iron bulwarks, except on the poop-deck, above which there was a light rail. She was built at Dumbarton in 1868. Her head in building was about N.N.E. After being launched she was taken to Glasgow, where she lay for some time head N.W. taking in a cargo of about 1100 tons of iron railway-chairs and sleepers.

She had two compasses on deck—a steering-compass and a standard compass. The card of each had two edge-bar needles $8\frac{3}{4}$ inches long, the ends separated 50° .

The steering-compass was near the stern, about 32 or 33 inches above the poop-deck, and 2 feet in front of the steering-wheel, which had an iron spindle. The standard compass was on a wooden pillar about 5 feet high,

standing on a wooden platform laid from the poop to the mainmast, and about 15 feet abaft the mainmast, which was of iron.

On the 18th of December the 'Glenorchy' had her compasses adjusted in the Gareloch by Mr. Fleming in the usual way.

The deviation of the steering-compass, as might have been expected from the combined effect of the position of the compass in the ship and of the ship in building, was *enormous*. Mr. Fleming says it was "as bad if not worse than any he ever saw." Mr. Fleming informs me that before magnets were applied to the steering-compass, when the ship's head bore N. (magnetic) it bore S. by the steering-compass; when the ship's head bore W. (magnetic) it bore about S.W. by S. by the steering-compass. In other words, at N. (magnetic) there was a deviation 180° , at W. (magnetic) a deviation of about $56^{\circ} 15' E$. The quadrantal deviation was about 10° .

These data give, using the notation of the 'Admiralty Manual for the Deviation of the Compass,'

$$\mathfrak{B} = -1.250,$$

$$\mathfrak{C} = 0,$$

or a force of the ship to the stern exceeding by one-fourth the whole directive force of the earth's magnetism acting on the compass, a disturbing force about twice as great as that found at the steering-compass in any of the iron-built armour-plated ships in Her Majesty's Navy.

This enormous disturbing force was corrected by three large magnets one of 36 inches and two of 26 and 28 inches placed together, fore and aft, on the starboard side of the binnacle, and by two or three smaller magnets placed so as to correct as far as possible the residual error on the other cardinal points.

The ship was then placed head N.W. (magnetic), when a westerly deviation of three-fourths of a point $8^{\circ} 26'$ was observed. This was of course approximately the amount of the quadrantal deviation, and it was corrected by a No. 12 iron jack-chain placed in the chain-boxes on each side of the compass.

The ship was then swung on sixteen points and the following deviations of the steering-compass obtained (+ signifying that the N. point of the needle was drawn to the E., — to the W.).

'Glenorchy' Steering-Compass, December 18, 1868.

Magnetic Course.	Deviation.	Magnetic Course.	Deviation.
N.	0	S.	-2°
N.N.E.	$+3^{\circ}$	S.S.W.	$+3$
N.E.	$+7$	S.W.	0
E.N.E.	$+2$	W.S.W.	0
E.	$+3$	W.	$+5$
E.S.E.	-3	W.N.W.	-3
S.E.	-2	N.W.	0
S.S.E.	-1	N.N.W.	-2

From these I derive the following expression for the deviation (δ) in terms of the azimuth of the ship's head (ζ) measured eastward from the magnetic N.

$$\delta = 30' + 30' \sin \zeta + 1^\circ 2' \cos \zeta + 2^\circ 37' \sin 2\zeta - 38' \cos 2\zeta.$$

These values show that the semicircular deviation had been entirely corrected. Of the quadrantal deviation a small part appears to have been uncorrected. There are practical difficulties in the way of correcting very large amounts of this deviation by soft iron, and I have no doubt Mr. Fleming acted with judgment in not attempting to carry this correction further. We may probably assume the maximum quadrantal deviation to have been about 10° .

The standard compass was not corrected by magnets, but its deviations were observed, and a Table of the deviations furnished. They were:—

'Glenorchy' Standard Compass, December 18, 1868.

Magnetic Course.	Deviation.	Magnetic Course.	Deviation.
N.	+12°	S.	— 5°
N.N.E.	— 7 30'	S.S.W.	+ 7 30'
N.E.	—24	S.W.	+16
E.N.E.	—37 30	W.S.W.	+22 30
E.	—38	W.	+33
E.S.E.	—31 30	W.N.W.	+35 30
S.E.	—24	N.W.	+35
S.S.E.	—11 30	N.N.W.	+20 30

These values give

$$\mathcal{A}=0, \quad \mathcal{B}=-\cdot610, \quad \mathcal{C}=+\cdot105, \quad \mathcal{D}=+\cdot100, \quad \mathcal{E}=0.$$

This Table and these values do not bear directly on the loss of the ship, because owing, as I collect, to the unsteadiness of the pillar the standard compass was found to be useless, and the ship was navigated by the steering-compass alone; but it is interesting from the light it throws on the general magnetic character of the ship, and its confirmation of the results obtained from the steering-compass.

The proportion of \mathcal{C} to $-\mathcal{B}$ exactly agrees with what we know of the direction in which the ship was built.

The large value of $-\mathcal{B}$ was no doubt owing to the original magnetism of the hull and not to the iron cargo, which in fact probably rather diminished than increased the $-\mathcal{B}$.

Cards containing the deviations of both compasses were furnished to the captain.

The question of the correction of the standard compass by magnets is one which has become of so much importance that I may be pardoned for interposing a digression on this subject and for inserting a passage from the third edition of the 'Admiralty Manual' now in the press.

“The question of the mechanical deviation of the compass has materially changed its aspect of late years. Before that time the deviation of a properly placed standard compass was of moderate amount, its maximum seldom exceeding 20° , and the directive force which acted upon it being generally comprised within the limits of two-thirds and four-thirds of the mean force. There was then no difficulty and some advantage in dispensing altogether with mechanical correction; or, if mechanical correction was employed, it was possible, at least in vessels which did not change their magnetic latitude, to make the correction so complete that tabular correction might be dispensed with. But in the present day it is frequently impossible to find a position for the standard compass at which the deviation and the variation of directive force do not greatly exceed these limits. In such cases the application of magnets for the purpose of equalizing the directive force on different azimuths becomes a matter of necessity; while at the same time the danger of trusting to mechanical correction alone without ascertaining and applying the residual errors is increased.

“This change of the condition of the question has produced a corresponding change in the practice in the Royal Navy.

“The same care as before is still used in the selection of a place for the standard compass; but a magnet is frequently or generally introduced for the purpose of equalizing the directive force on different azimuths, and at the same time diminishing the semicircular deviation. The quadrantal deviation is not often corrected mechanically, but is generally left for tabular correction.

“The heeling deviation is always ascertained, and is sometimes corrected mechanically.”

After the ‘Glenorchy’ was swung she took in an additional quantity (about 120 tons) of iron. I do not, however, think it possible that this quantity could have altered the deviations sensibly.

The ‘Glenorchy’ sailed from Greenock on the 25th of December. She had on board a pilot accustomed to the navigation of the Irish Channel. She was towed to Lamlash Harbour, in the Island of Arran, where she lay till 3 A.M. on the 31st of December. She then got under way, the wind blowing moderately from the N.W., and steered a course down mid-channel, sighting the Copeland, the Mull of Galloway, the North and South Rock, St. John’s Point, and the Calf of Man lights.

The wind gradually heading her, she tacked about 6.15 A.M. on the 1st of January. At 7.10 A.M. her position was determined by a bearing and distance of the South Stack Light, which then bore S. by W., distant five miles.

Till the ship tacked she had been on the starboard tack, on courses from S.W. to S., on which the deviation-card gave small deviations for the steering-compass. The bearing of the lights successively passed had,

however, been carefully taken by the captain, and from these he found that the compass had a westerly deviation of one point not shown by the deviation-card. From 7.10 A.M. till 3 P.M. the ship was on the port tack, sailing by the wind but kept good full, her course by the steering-compass being about S.W. by W. During the whole of this time a gale of wind was blowing from S.S.E., gradually increasing in intensity, with thick weather and rain, which cleared only for a little about 1.30, when land was seen in the distance bearing W.N.W. The lead was cast and 35 fathoms found. The captain and pilot consulted the chart, and making what they considered a proper allowance for tide and leeway, came to the conclusion that the land was Wicklow Head, bearing W.N.W., distant twenty-two miles.

The ship then stood on the same course till 3 P.M., when soundings were again taken and 25 fathoms found. Orders were then given to wear, but in wearing, and when nearly before the wind, the ship struck and remained fixed on the Kish Bank, about four miles S. of the Kish Lightship.

The point at which the ship so unexpectedly found herself was about twenty geographical miles to leeward of that at which the captain and pilot supposed themselves to be. In other words, the ship's actual course was about 28° or $2\frac{1}{2}$ points to the right of her supposed course. To what, then, was the error due?

In the first place, it seems impossible to attribute any large part of the error to an insufficient allowance for the effects of tide and leeway. It is true that from 7 to 1 o'clock a spring flood-tide, assisted by a southerly gale, had been running, but this was known to the captain and pilot. They had watched with great care throughout the day the courses, the leeway, and the rate, and, if we may judge from their estimate of the distance run, had estimated them with great exactness.

The next cause that suggests itself is a deviation of the compass not allowed for.

The steering-compass by which the ship was navigated was, we have seen, carefully adjusted in the Clyde, and was then nearly correct on a S.W. by W. course. Is it possible that any change in the magnetism of the ship had taken place, as has sometimes been found or supposed in new ships, which would account for the error? The answer to this must be in the negative. It is certain that any such change in the 'Glenorchy' would have had the effect of producing an error of the opposite kind, and, had it operated, she would have been found to the south, not to the north, of her supposed course.

Is there, then, any other cause adequate to produce an easterly deviation on a S.W. by W. course which might lurk concealed and undetected in the process of adjustment and only emerge during the voyage? To this the answer is emphatically Yes! *The Heeling Error.*

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From the combined effects of the position of the steering-compass in the ship and of the ship in building, it is certain that there must have been a very large heeling error drawing the north point of the compass to the weather side of the ship. This error was probably not less than 3° or 4° for each degree of heel on a N. or S. course, before the chain-correctors were applied. The chain-correctors would reduce it about 50', leaving 2° or 3° for each degree of heel. On a S.W. by W. course this error would be reduced to five-ninths of its maximum amount, or would be from 1° to $1\frac{1}{2}^{\circ}$ for each degree of heel. Hence if the 'Glenorchy' was heeling 10° , she would *certainly* have an easterly deviation of a point to a point and a half, or possibly more, introduced.

But it may be asked, if the ship had this large amount of heeling deviation, how did it escape detection in the earlier part of the voyage, when the ship was on a southerly course and the bearings of the lights were taken? and if detected, how was it not allowed for on the 1st of January?

The answer to these questions is remarkable; it is shortly this. The error *was* detected and *was* allowed for correctly when the ship was on the starboard tack. Afterwards, and when the ship was put on the port tack, it was still allowed for, *but in the same direction as before*, and therefore in the wrong direction. It was allowed for as a *westerly* deviation, although it had become an *easterly* deviation; and consequently the heeling error instead of being corrected, was doubled. And of this the cause was as follows.

Between Greenock and Lamlash, the ship being towed and on even keel, there were no means of detecting the error. Between Lamlash and the Calf of Man, when the ship was on the starboard tack and on a southerly course, an error of a *point of westerly deviation* was, as we have seen, detected and allowed for by the captain. This error I think there cannot be a doubt was heeling error.

But when on the morning of the 1st of January the ship tacked and was put on the port tack, the heeling deviation changed from being a westerly deviation to being an easterly deviation. The captain not being aware that there would be this change, and having no opportunity of verifying his course, continued to make the same allowance as before, and consequently made it, as I have said, in the wrong direction. As to the fact I think I cannot be mistaken.

The captain's words are:—"Our observations of the different lights all the way down Channel showed the compasses were inaccurate, and during the whole course on the starboard tack we had to steer one point more to the west than the proper course."

Then, speaking of the ship's supposed position at 1.30, he says:—"The courses I had-observed, and the rate we were going, allowing for the tide and the leeway, *and the point the compass was in error while on the*

starboard tack, should have brought us to a point with Wicklow Head, lying W. by N., twenty-one miles distant."

It is clear from this that the captain made an allowance for the point of error he had discovered. Had he applied it in the opposite direction, he would undoubtedly have mentioned that he did so and why he did so.

The particular conclusions, then, which I draw from the facts of the case are these :—

1. There must have been a large heeling error affecting the steering-compass of the 'Glenorchy,' which, on the courses steered, would be a westerly deviation on the starboard tack, an easterly deviation on the port tack.

2. The westerly deviation detected on the starboard tack was this heeling error.

3. The true construction to be put on the captain's statement is, that when on the port tack he allowed for the point of deviation which he had detected on the starboard tack as a point of westerly deviation, not as a point of easterly deviation, as he would have done had he known the cause and the law of the deviation which he had detected.

4. That, in consequence, his supposed course was in error one point *plus* the heeling deviation, which, on a S.W. by W. course, was probably about one point more.

The general conclusions to be drawn from the history of the shipwreck seem to me to be :—

1. The great importance of selecting a position for the navigating-compass where the force of the ship's magnetism is moderate and uniform.

2. The importance of extending the usual process of "adjustment" of a compass to the ascertaining and (if necessary) the correcting of the heeling error. This is a matter of no difficulty if the compass-adjuster is duly instructed and supplied with the requisite instruments.

III. "Spectroscopic Observations of the Sun.—No. IV." By J. NORMAN LOCKYER, F.R.A.S. Communicated by Dr. SHARPEY, Sec.R.S. Received April 14, 1869.

I beg to lay before the Royal Society very briefly the results of observations made on the 11th instant in the neighbourhood of a fine spot, situated not very far from the sun's limb.

I. Under certain conditions the C and F lines may be observed *bright on the sun*, and in the spot-spectrum also, as in prominences or in the chromosphere.

II. Under certain conditions, although they are not observed as bright lines, the corresponding Fraunhofer lines are blotted out.

III. The accompanying changes of refrangibility of the lines in question

show that the absorbing material moves upwards and downwards as regards the radiating material, and that these motions may be determined with considerable accuracy.

IV. The bright lines observable in the ordinary spectrum are sometimes interrupted by the spot-spectrum, *i. e.* they are only visible in those parts of the solar spectrum near, and away from, spots.

V. The C and F lines vary excessively in thickness over and near a spot, and on the 11th in the deeper portion of the spot they were much thicker than usual.

IV. Stars, in the spectrum of which the absorption-lines of hydrogen are absent, may either have their chromospheric light radiated from beyond the limb just balanced by the light absorbed by the chromosphere on the disk, or they may come under the condition referred to in (II.), either absolutely or on the average.

ADDENDUM.—Received April 29, 1869.

Since the date on which the foregoing paper was written, I have obtained additional evidence on the points referred to. I beg therefore to be permitted to make the following additions to it.

The possibility of our being able to determine the velocity of movements of uprush and downrush taking place in the chromosphere depends upon the alterations of wave-length observed.

It is clear therefore that a mere uprush or downrush at the sun's limb will not affect the wave-length, but that if we have at the limb cyclones, or backward or forward movements, the wave-length will be altered; so that we may have:—

1. An alteration of wave-length near the centre of the disk caused by upward or downward movements.
2. An alteration of wave-length close to the limb, caused by backward or forward movements.

If the hydrogen-lines were invariably observed to broaden out on both sides, the idea of movement would require to be received with great caution; we might be in presence of phenomena due to greater pressure, both when the lines observed are bright or black upon the sun; but when they widen out sometimes on one side, sometimes on the other, and sometimes on both, this explanation appears to be untenable, as Dr. Frankland and myself in our researches at the College of Chemistry have never failed to observe a widening out on both sides the F line when the pressure of the gas has been increased.

On the 21st I was enabled to extend my former observations.

On that day the spot, observations of which form the subject of the paper, was very near the limb; as this was the first opportunity of

observing a fine spot under such circumstances I had been able to utilize, I at once commenced work upon it. The spot was so near the limb that its spectrum and that of the chromosphere were both visible in the field of view.

The spot-spectrum was very narrow, as the spot itself was so greatly foreshortened; but the spectrum of the chromosphere showed me that the whole adjacent limb was covered with prominences of various heights all blended together.

Further, the prominences seemed fed, so to speak, from, apparently, the preceding edge of the spot; for both C, F, and the line near D, *were magnificently bright on the sun itself*, the latter especially striking me with its thickness and brilliancy.

In the prominences C and F were observed to be strangely gnarled, knotty, and irregular, and I thought at once that some "injection" must be taking place. I was not mistaken. On turning to the magnesium lines I saw them far above the spectrum of the limb and unconnected with it.

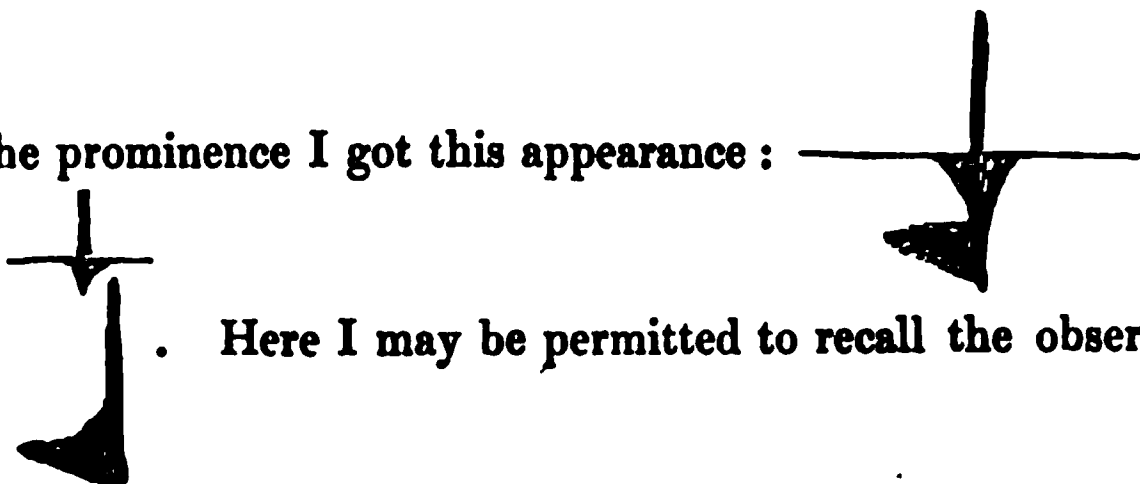
A portion of the upper layer of the photosphere had in fact been lifted up beyond the usual limits of the chromosphere, and was there floating cloud-like.

The vapour of sodium was also present in the chromosphere, though not so high as the magnesium, or unconnected with the spectrum of the limb, and, as I expected, with such a tremendous uplifting force, I saw the iron lines (for the first time) in the spectrum of the chromosphere.

My observations commenced at 7.30 A.M.; by 8.30 there was comparative quiet.

At 9.30 the action had commenced afresh; there was now a single prominence.




At the base of the prominence I got this appearance :



Higher up this :

. Here I may be permitted to recall the obser-

vation made on March 14, in which a slight movement of the slit gave

me first , then , and finally , all these

appearances being due to cyclonic action.

On the following side of the spot, at about 10 A.M., I observed that the F line had disappeared; at the point of disappearance there appeared to be an elongated brilliantly illuminated lozenge lying across it at right angles, as if the spectroscope were analyzing the light proceeding from a cyclone of hydrogen on the sun itself, but so near the limb that the rotatory motion could be detected.

The next observations I have to lay before the Royal Society were made on the 27th inst. Careful observations on the 25th and 26th revealed nothing remarkable except that the chromosphere was unusually uniform.

On the 27th a fine spot with a long train of smaller ones and faculae was well on the disk. The photosphere in advance of the spot, and the large spot itself, showed no alteration from the usual appearance of the hydrogen-lines; but in the tails of the spot the case was widely different.

The F line, at which I worked generally, as the changes of wave-length are better seen, was as irregular as on the former occasions.

I. It often stopped short of one of the small spots, swelling out prior to disappearance.

II. It was invisible in a facula between two small spots.

III. *It was changed into a bright line, and widened out on both sides two or three times* IN THE VERY SMALL SPOTS.

IV. Once I observed it to become bright *near* a spot, and to expand over it on both sides.

V. Very many times near a spot it widened out, sometimes considerably, on the less refrangible side.

VI. Once it extended as a bright line without any thickening over a small spot.

VII. Once it put on this appearance :



bright.

VIII. I observed in it all gradations of darkness.

IX. When the bright and dark lines were alongside, the latter was always the less refrangible.

The Society then adjourned over Ascension Day to Thursday, May 13.

May 13, 1869.

Dr. WILLIAM ALLEN MILLER, Treasurer and Vice-President,
in the Chair.

In conformity with the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows :—

Sir Samuel White Baker, M.A.
John J. Bigsby, M.D.
Charles Chambers, Esq.
William Esson, Esq., M.A.
George Carey Foster, B.A.
William W. Gull, M.D.
J. Norman Lockyer, Esq.
John Robinson McClean, Esq.
St. George Mivart, Esq.

John Russell Reynolds, M.D.
Vice-Admiral Sir Robert Spencer
Robinson, K.C.B.
Major James Francis Tennant, R.E.
Wyville Thomson, LL.D.
Col. Henry Edward Landor Thuillier,
R.A.
Edward Walker, Esq., M.A.

The following communications were read :—

- I. "On some of the minor Fluctuations in the Temperature of the Human Body when at rest, and their Cause." By A. H. GARROD, St. John's College, Cambridge. Communicated by Dr. BEALE. Received April 16, 1869.

The author's object in the following communication is to show that the minor fluctuations in the temperature of the human body, not including those arising from movements of muscles, mainly result from alterations in the amount of blood exposed at its surface to the influence of external absorbing and conducting media.

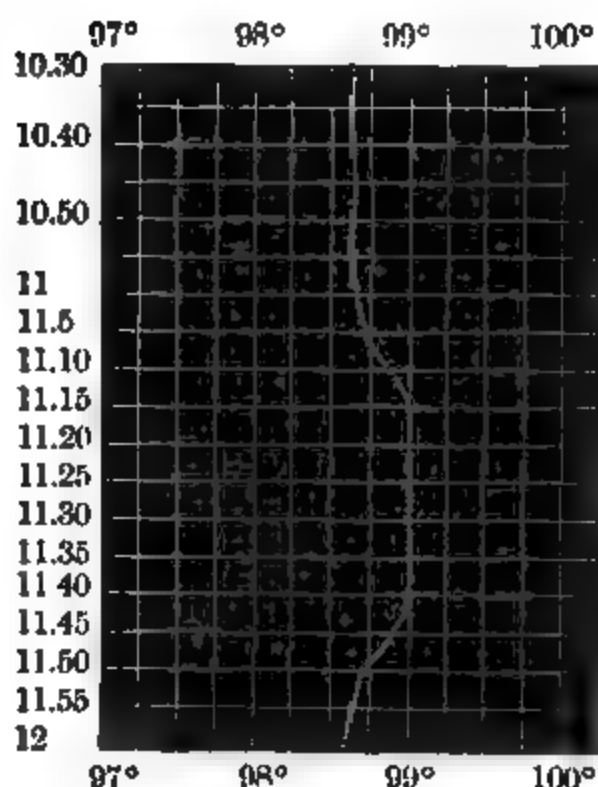
In the following Tables, when not otherwise mentioned, all the temperatures are taken under the tongue, the thermometer remaining in the mouth for five minutes, except when the observations were made each two-and-a-half minutes, on which occasions the temperature of the bulb was not allowed to fall below 85° F.

It may be remarked that in no case mentioned below was the temperature of the air above 65° F., and that on all occasions the skin was dry, whereby any complications from the presence of perceptible moisture were avoided; and the arguments based on the facts necessitate an approximation to those conditions.

The Tables have been selected from a great number of observations; and no results have been obtained which are not easily explained on the theory given.

The temperatures were taken on one subject, aged 22, male, thin.

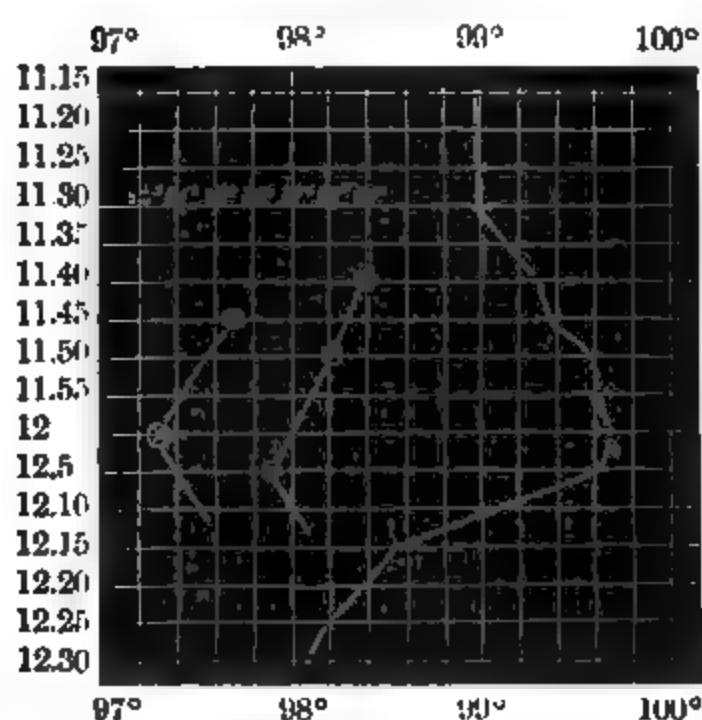
No. I.—From 10.30 p.m. till 12 night.



Sitting in a room (temp. of air 66° F.) all the time. Fully clad till 11, when stripped in a minute, therefore nude at 11.1. Warm when dressed, but got cold when nude. At 11.40 covered body all over with a thick blanket, soon followed by a slight skin-glow. In the blanket until 12 night.

When body covered, pulse much more bounding than when not covered.

No. II.—From 11.15 p.m. till 12.30 night.

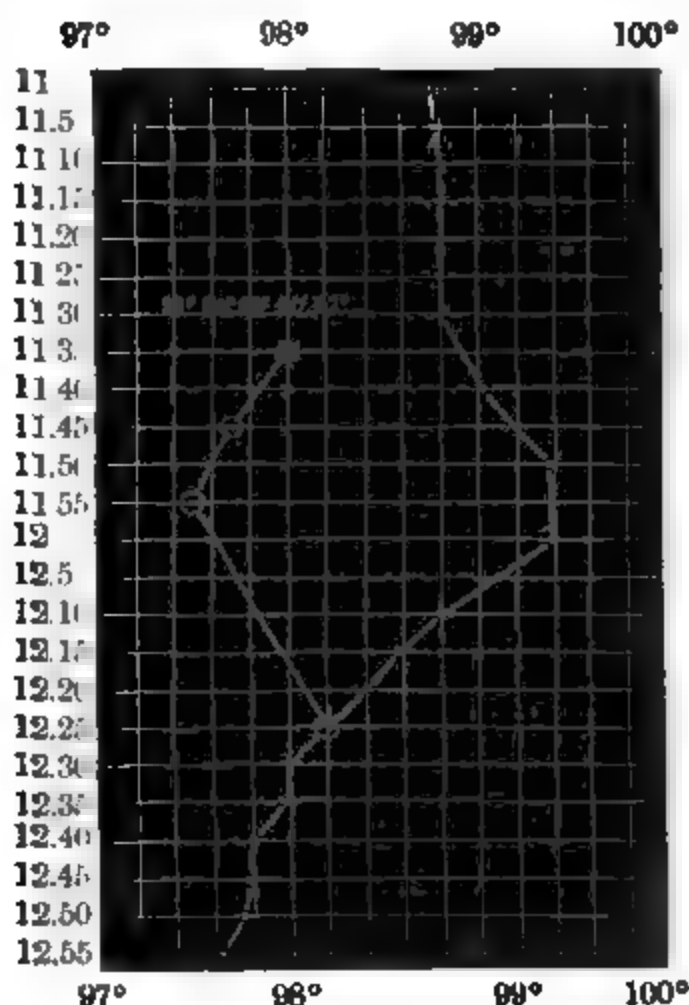


Standing from 11 till 12.5 in a room with the thermometer at 47° F., warmly clad till 11.30, when stripped in two minutes, so nude at 11.32. Fairly warm all the while. Got to bed at 12.6, and lay closely wrapped by bedclothes for the rest of the time. A decided glow came on at 12.11½, lasting a minute, after which feet became a little cold, but skin of body quite warm.

Whilst standing nude pulse small, but bounding when dressed and when in bed.

⊙ Indicates the temperature of the pectoral region, two inches above the nipple, taken by placing, for five minutes, a flat spiral thermometer on the part.

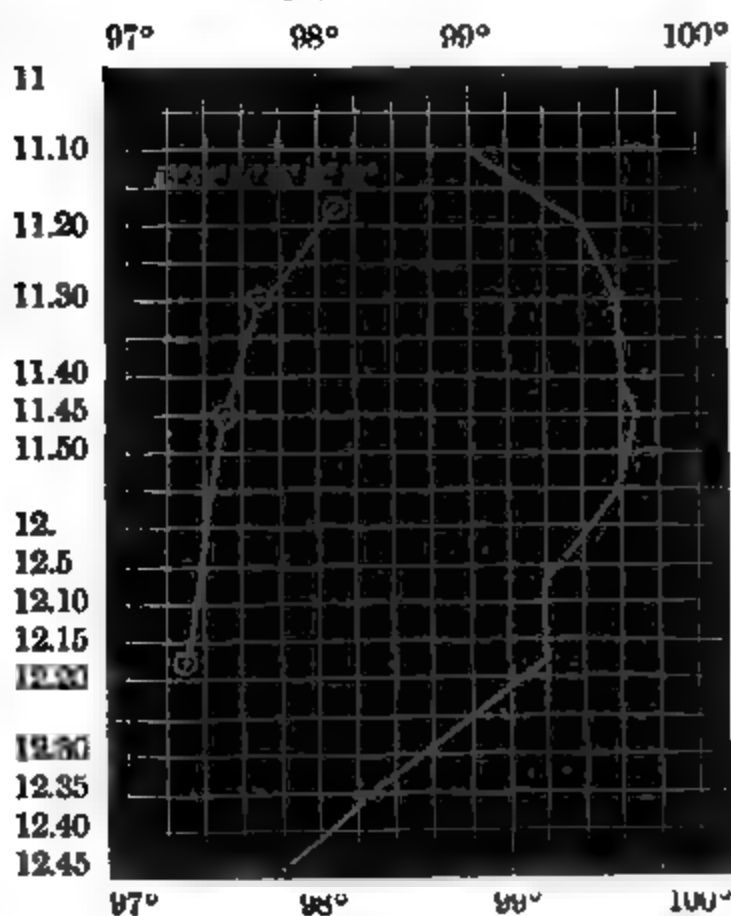
⊗ Indicates the temperature of the front of the thigh, with the same instrument as the last.

No. III.—*From 11 P.M. till 1 A.M.*

Standing in room (temp. of air 52° F.) from 11 until 12. Fully clad until 11.30, and then stripped in two minutes, so nude at 11.32. Warm in body all the while. At 12.2 got to bed, and there the rest of the time, closely wrapped. A glow came on at 12.8½, lasting half a minute, after which feet became coldish.

Pulse not so bounding when nude as when body covered.

⊙ Indicates the temperature of the pectoral region, found by placing a spiral flat thermometer on it, and keeping it there five minutes.

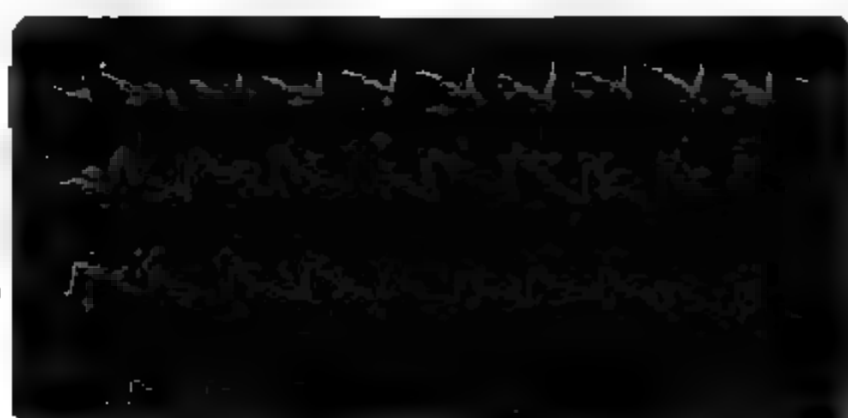
No. IV.—*From 11 P.M. till 12.45 night.*

Nude at 11.11 in a room (temp. of air 56°). Standing from 10.50 until 12.20 nude. At 12.21 got to bed, and remained there rest of time. At 11.45 began moving about and stooping, and whenever stooped felt a chill. Quite shivering from 11.57½ till 12.7½, when, leaving off moving, the shivering ceased.

When in bed had no marked glow, and feet continued to be warm; skin of thighs not warm.

The following is the sphygmographic curve of radial artery at wrist: when in bed at 12.40, pulse same as at 11 (the same pressure was used on the sphygmograph-spring in all the traces):—

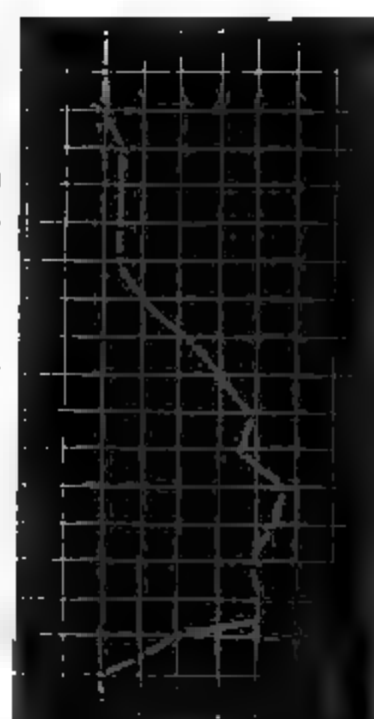
11
11.20
11.35
11.51



No. V.—From 10.30 P.M. till 12 night.

97° 98° 99° 100°

10.30
10.35
10.40
10.45
10.50
10.55
11
11.5
11.10
11.15
11.20
11.25
11.30
11.35
11.40
11.45
11.50
11.55
12



Sitting in a room (temp. of air 58° F.) all the time. Warmly clad till 11, when stripped in two minutes, so nude at 11.2. At 11.20 went for half a minute into a colder room. At 11.45 put on several flannel things, which had been warmed by the fire, and sat in front of a warm fire.

Took sphygmograph-trace from right superficialis volæ at 10.40 and at 11.10. Tried to do so at 11.40, but could not get any indication, from the smallness of its pulsation. At 12 the pulsation was as great as at 10.40.

97° 98° 99° 100°



10.40.

11.10.

No. VI.—From 10.30 P.M. till 11.45 P.M.

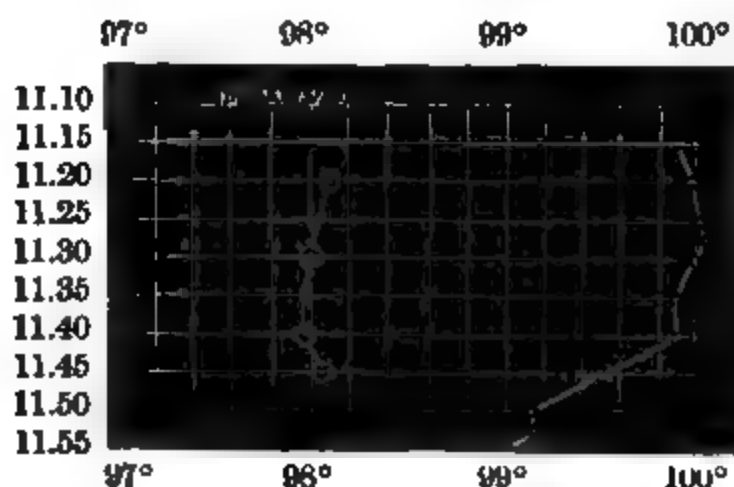
97° 98° 99°

10.30
10.40
11
11.15
11.30
11.45



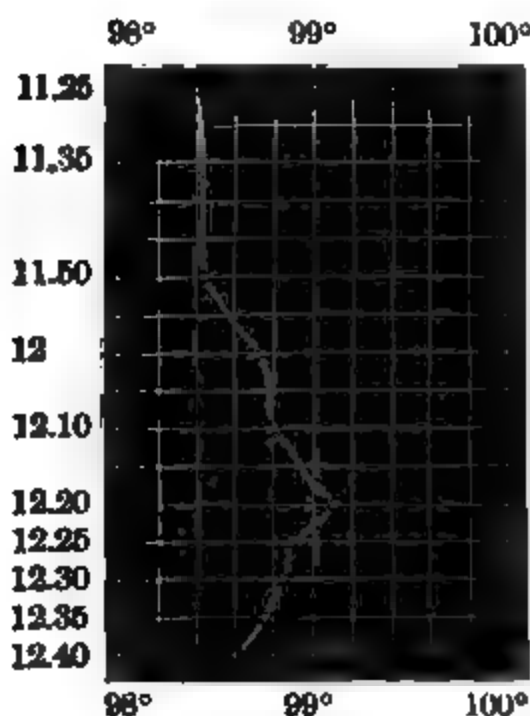
Sitting in a room (temp. of air 59° F.) from 9.30 until 10.40, quiet, cool, and warmly clad. From 10.40 till 10.55 moving about in the same room. Stripped at 10.55, and nude in two minutes. Remained nude until 11.24, when got to bed, and remained there for the rest of the time.

97° 98° 99°

No. VII.—*From 11.10 P.M. till 11.55 P.M.*


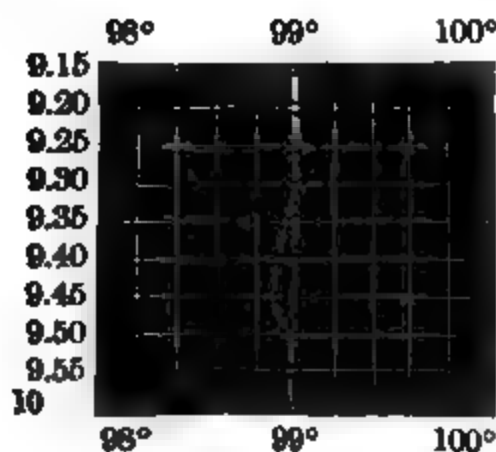
Standing in a room (temp. of air 53° F.) from 11 until 11.25. Fully clad until 11.9, when stripped, and nude at 11.10. Continued nude until 12. At 11.25 seated, and remained so until 12, on a bed. At 11.40 put feet in water from 110°–114°, above ankles, and remained thus rest of time, maintaining the heat of the water. Chilly when feet in bath, not before. At 11.52½ contracted limb muscles tonically, and maintained them so until 11.55.

⊙ Indicates temperature of pectoral region, two inches above nipple, taken with spiral thermometer, for five minutes.

No. VIII.—*From 11.25 P.M. till 12.40 night.*


Standing in a room (temp. of air 58° F.) from 11 until 12, and sitting during the rest of the time on a bed. Fully clad until 11.50. Nude from 11.52, and remained so. Feet a little cold at 12.20, and put them into hot water (108°–114°) at 12.21, gradually increasing the heat of the water. Kept feet in water, above ankles, until 12.40.

On adding more hot water and putting feet in it chills followed.

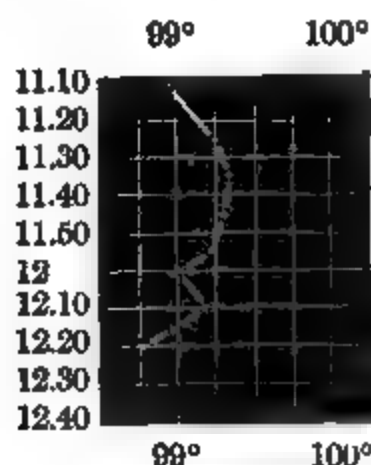
No. IX.—*From 9.15 A.M. till 10 A.M.*


Sitting all the while in a room (temp. of air 52°), not far from an ordinary fire.

Felt cold all over during the time. Reading. At 9.30 turned to the fire and put feet on the fender, having been previously quite at the side of the fireplace. As feet got warm, hands, which were previously warm, became cold.

Clad in winter clothes.

No. X.—From 11.10 A.M. till 12.40 P.M.



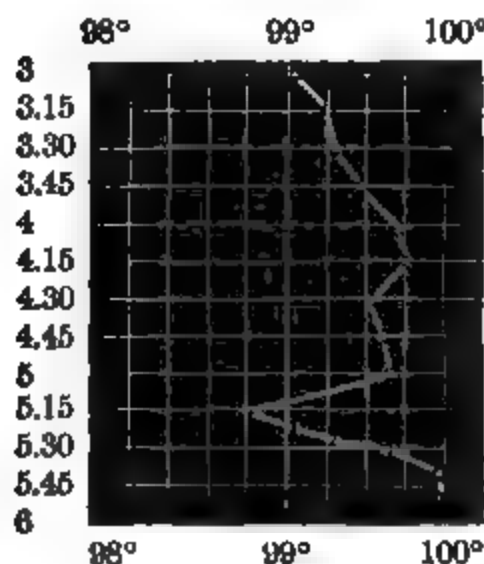
Temperature of air 62° F. A cloudy, breezy day. At 11 walked about 200 yards on to a beach, and sat down on the shingle at 11.5, where there was a slight side breeze. Hands and feet a little cold.

Sun covered by clouds until 11.35, after which it began to shine; immediately after which began to feel warm, and continued to get warmer until 12.7, when at 12.7 a cloud covered sun until 12.11. During time sun covered, several chills came over body.

Walking in sun from 12.16 onward.

Clad in thin merino next skin and summer clothes.

No. XI.—From 3 P.M. till 6 P.M.



Temperature of air 66° F., slowly diminishing to 64° F. Sitting on a beach from 3 until 5, after a dinner at 2.15–2.45. A slight face breeze. In the shade. Warm until 4.15, when feet began to get a little cold, and by 5 so cold that obliged to move about. At 5 began to walk slowly, and had to go up several steps. At 5.20 began to walk briskly. Began to perspire at 5.25. Continued walking, perspiring until 6.

Clad as in last.

To explain these Tables :—

The actual temperature of the body at any given moment must be the resultant of (1) the amount of heat generated in the body, and (2) the amount lost by conduction and radiation.

(1) The *source* of heat in the body is not considered in this paper; and no more will be now said of it, except that there is every reason to believe that it is not in the skin itself, and that, for the short periods through which each observation was made, it is approximately uniform.

(2) The *loss* of heat from the body is modified by changes in the skin and by changes in the surrounding media; and these two are mutually dependent.

It has long been known that cold contracts and heat dilates the small arteries of the skin, respectively raising and lowering the arterial tension, and thus modifying the amount of blood in the cutaneous capillaries.

But modifications in the supply of blood to the skin must alter the amount of heat diffused by the body to surrounding substances; and so we should expect that by increasing the arterial tension, thus lessening the cutaneous circulation, the blood would become hotter from there being less facility for the diffusion of its heat, and that by lowering the ten-

sion, thus increasing the cutaneous circulation, the blood would become colder throughout the body, from increased facility for conduction and radiation.

That such is the case is proved by Tables I., II., III., IV., V., and VI., where, by stripping the warm body of clothing, in a cold air, when the tension was low (as in Tables IV., V., shown by the sphygmograph-trace), the temperature and tension rose, at the same time that the surface became colder.

In Tables I., II., III., IV., V.; and VI., by covering the nude body with badly conducting clothing, when the tension was high, the surface-heat soon accumulated sufficiently to cause a sudden reduction of arterial tension, commonly called a glow, and a rapid fall in the temperatures, from the larger amount of blood exposed at the surface of the body to the influence of colder media.

Changes in the arterial tension are easily recognized by the subject of experiment, from the sensations they produce; a feeling of warmth followed by a shiver, or a shiver itself, generally shows that the tension is lowered, while the opposite effect follows a rise in the tension; and this can be generally confirmed by the sphygmograph-trace. A bounding weak pulse shows a low, and a small thready one a high tension.

We know, from the observations of Davy and others, that by reducing the tension in one part of the body the tension of other parts is lowered; thus by placing one hand in hot water, a thermometer in the other rises. In Tables VII. and VIII. it is shown that by putting the feet in hot water (at 110° to 115°) the lowering of the tension was so great that the amount of heat lost into the air considerably exceeded that gained to the body from the water, so that the temperature of the body began to fall directly, and decreased considerably; and it was noticed that on adding more hot water chills were produced, which was the same as the effect of first putting the feet in the water.

By covering a small part of the body with a bad conductor, the tension of the whole body soon falls, from the accumulation of heat in the covered parts causing a lowering in the tension generally, and a consequent greater carrying away of heat. In this way the fall after sitting down on a bad conductor when nude can be explained (Table VII.).

A glow is felt in the skin directly upon short muscular movement, as stooping, and the temperature falls at the same time, as in Table IV., between 11.45 and 12.20, and in Table XI., between 5.0 and 5.15. In the latter case the muscular movement was carried to such an extent that the loss was made up for by the increase of heat from the muscular movement.

Simply heating the feet lowers the tension and temperature together, as in Table IX. and in Table X. The passage of a cloud before the sun seems to have acted by reducing the loss of heat, as the temperature rose at the time.

Further confirmation of the facts stated as to the modification of tension may be found in Marey's work, '*De la Circulation du*' published in Paris in 1863. In that book the author ascribes the uniformity of the heat in the internal parts to the same cause as the author of the present paper ascribes the variations.

The fact observed by Dr. W. Ogle in the St. George's Hospital for 1866, and by Drs. Ringer and Stewart in a paper read before the Society this year, that the temperature falls at night, and is lowest at 12 to 1 A.M., and begins to rise after that time, is simply explained by the theory given above; for it depends on the custom of Englishmen to bed at about that hour, and thus giving a large amount of heat to the cold bedclothes, which at first is expended in warming the sheet while later on in the night the bedclothes are warm, and therefore the body has only to make up for the heat diffused.

Other natural phenomena can be similarly explained. Thus, on a clear day, the effect of sitting with one side of the body in the direct rays of the sun is to cause the other side to feel much colder than if there were no fire at all, because the fire lowers the tension over the whole body, and supplies heat to the full cutaneous vessels of one side, while the other side, being equally supplied with blood in the skin, does not receive heat, and has to distribute it rapidly to the cold clothes &c.

II. "Observations of the Absolute Direction and Intensity of Terrestrial Magnetism at Bombay." By CHARLES CHAMBERS Esq., Superintendent of the Colaba Observatory. Communicated by Lieut-General SABINE, R.A., President. Received April 5, 1869.

(Abstract.)

The observations made by the author were of the three usual elements—the Dip, Declination, and Intensity of the Horizontal Component of Force. They were taken with instruments supplied to the Colaba Observatory in the year 1867 through the Kew Committee of the British Association, after having been tested at the Kew Observatory. The dip-circle was made by Barrow of London, and is furnished with two needles; the other instrument, the unifilar magnetometer, which serves both for observation of declination and horizontal force, was made by Elliott Brothers of London. The results of the observations for dip only have as yet been received from the author.

A complete observation consists of thirty-two readings, each end of the needle being read twice in each different position of the needle and circle, and the mean of the thirty-two is taken as the result of the observation. The observations were 178 in number, commencing on the 29th of January 1867, and extending to the 29th of December 1868. They were generally taken, with the two needles alternately, on particular days of

week. Up to August 17, 1867, the observations commenced with either end (A or B) of the needle dipping, and without remagnetizing the needle; *i. e.* the magnetization for the latter half of one observation was made to serve for the first half of the next observation with the same needle, the two needles having been kept during the interval with contrary poles adjacent in a zinc box; but after August 17, 1867, the needle was always remagnetized, so as to make the end A dip during the first half of the observation. The effect of this change of practice was to produce a marked increase in the accordance of successive observations. Tables are given containing every complete observation made up to the end of 1868, and showing, as well as the mean dip, the partial results in each position of the circle, and with each end of the needle dipping, and also the mean weekly and mean monthly values. The mean dip obtained for the months April to December 1867 was $19^{\circ} 2' 00$, and for the year 1868 was $19^{\circ} 3' 87$. The period embraced by the observations is too limited to allow of an exact determination of the rate of secular change; nevertheless the observations show distinctly that the dip is increasing. The author takes $+1' 3$ as the rate of annual change.

For the probable error of a single weekly determination, including the effect of actual magnetic disturbance of an irregular character, the author obtains for the period from April 29 to August 16, 1867, $0' 67$; from August 23 to December 31, 1867, $0' 26$; from January 1 to December 31, 1868, $0' 24$. Notwithstanding the extreme smallness of these probable errors, the indications of needle No. 2 exceeded those of needle No. 1 by quantities ranging, in the means of periods of a few months, from about 0 to $+5' 0$. An endeavour is made in another communication to explain a possible cause of these differences.

III. "On the Uneliminated Instrumental Error in the Observations of Magnetic Dip." By CHARLES CHAMBERS, Esq., Superintendent of the Government Observatory, Bombay. Communicated by Lieut.-General SABINE, R.A., President. Received April 15, 1869.

(Abstract.)

A single reading of one end of a dipping-needle placed in a dip-circle provided with microscopes for observing is liable to a variety of instrumental errors, which are eliminated by taking the mean of the sixteen readings of the two ends in the eight different positions included in a complete observation. Nevertheless it is found that with the best modern instruments a mean value results from these sixteen observations different for each different needle, and that the difference between the results obtained with two different needles is not the same at all times.

The irregularities in the values of the dip observed at Bombay with two needles of excellent character made by Barrow of London, led the author

to investigate the effect of a hypothetical irregularity in the shape of the axle of the needle, such that a section of the axle by a plane perpendicular to its axis would be elliptical instead of circular in form. Another source of error, which was brought to the notice of the Royal Society many years ago in a paper published in the *Proceedings*, is the displacement of the centre of gravity of the needle from the centre of the axle, combined with inequality in the magnetization of the needle when the poles are direct and reversed. Experience has led the author to the conclusion that the usual method of magnetization, by a definite number of passes of the same pair of bar-magnets, communicates magnetism to the needle very unequally when the one end of the needle is made north and when the other end is made north. Consequently it is advisable to investigate the effects of ellipticity of the axle and of displacement of the centre of gravity at the same time, which the author proceeds to do.

As each of these errors depends upon two independent unknown quantities, suppose the excentricity and the azimuth of the major axis of the elliptic section of the axle for the first, and the two coordinates of the centre of gravity, referred to axes in the plane of motion of the needle and passing through the centre of the axle, for the second, the equation connecting the true and apparent dip, in any one position of the needle and of the face of the dip-circle, will involve four unknown quantities depending on the above errors. If we suppose the instrumental errors small, so that the apparent dip does not much differ from the true dip, these four unknown quantities will appear as coefficients respectively of the sine and of the cosine of twice the dip for the elliptic error, and of the sine and the cosine of the dip for the error of excentricity of the centre of gravity, and will be divided in each case by the magnetic moment of the needle. On taking the mean of the apparent dips in the four usual positions of the needle and of the dip-circle before the magnetism of the needle is reversed, two of the terms, one for each error, disappear, and there results for the difference between the true dip θ and the mean of the four apparent dips (θ') an equation of the form

$$\pi'(A-B)=(\theta')-\theta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where π' is the reciprocal of the magnetic moment of the needle, and A and B are the constants depending on the errors of the pivot and of the centre of gravity respectively. These two quantities are constant only for the same place, the first involving as a factor the sine of twice the dip divided by the total force, the second the cosine of the dip divided by the total force.

Now let the poles be reversed in the usual way, and let π'' be the reciprocal of the magnetic moment, and (θ'') the mean apparent dip in the four positions after remagnetization; then

$$\pi''(A+B)=(\theta'')-\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The equations (1), (2) contain three unknown quantities A, B, θ ; but if we repeat the observations with the difference that this time the needle is

magnetized as weakly as is consistent with the condition that the apparent shall not greatly differ from the true dip, we shall obtain two more equations of the form

$$n'''(A-B)=(\theta''')-\theta,$$

$$n''''(A+B)=(\theta'''')-\theta;$$

and these four equations, when suitably combined, will determine the values of the three unknown quantities A , B , θ .

The magnetic moments involved in these equations may be determined with little trouble, and with sufficient accuracy, by placing the needle as a deflector on a unifilar magnetometer, and observing the angle of deflection produced thereby upon the suspended magnet.

A series of observations has been commenced by the author with the view of testing whether the true dip can be determined exactly with a single needle by the method above described, the results of which he hopes to communicate to the Royal Society hereafter.

The Society then adjourned over the Whitsuntide Recess to Thursday, May 27.

May 27, 1869.

Lieut.-General SABINE, President, in the Chair.

The following communications were read :—

- I. "On the Laws and Principles concerned in the Aggregation of Blood-corpuscles both within and without the vessels." By RICHARD NORRIS, M.D., Professor of Physiology, Queen's College, Birmingham. Communicated by Dr. SHARPEY. Received April 29, 1869.

(Abstract.)

In 1827, or forty-one years ago, the phenomenon which forms the subject of this paper was first observed by Mr. Joseph Jackson Lister and the late Dr. Hodgkin.

To these observers the microscope revealed the fact that if a minute drop of human blood is placed between two plates of glass, the red corpuscles apply themselves to each other by their concave surfaces in such a manner as to form long cylindrical masses, which resemble piles of coin, and that very frequently these piles are so arranged as to form with each other a complete network of rouleaux with clear intervening spaces occupied by liquor sanguinis.

Simple as this observation may appear, its importance in a pathological point of view can scarcely be overrated; for upon its correct interpretation depends our knowledge of the real nature of one of the most marked characteristics of inflammation, viz. the phenomenon of inflammatory or homogeneous stasis.

During the forty years which have elapsed since the discovery of this

fact, many theories have been advanced to explain its nature ; but all of them, without exception, have laboured under the disadvantage of being purely hypothetical in their character, and quite incapable of demonstration by an appeal to experiment. Thus, while some writers have attributed the effect to an imaginary law of vital attraction, others have more correctly referred it to the operation of some unexplained physical cause.

Professor Lister (son of the original observer already mentioned), who has devoted much attention to this subject, says, in a paper submitted to the Royal Society, June 18, 1857, and published in the Philosophical Transactions for 1858, p. 648 :—“ For my own part, I am satisfied that the rouleaux are simply the result of the biconcave form of the red disks, together with a certain though not very great degree of *adhesiveness*, which retains them pretty firmly attached together when in the position most favourable for its operation, viz. when the margins of their concave surfaces are applied accurately together, but allows them to slip upon one another when in any other position. *There is never to be seen anything indicating the existence of an attractive force drawing the corpuscles towards each other : they merely stick together when brought into contact by accidental causes.* Their adhesiveness does not affect themselves alone, but other substances also, as may be seen when blood is in motion in an extremely thin film between two plates of glass, when they may be observed sticking for a longer or shorter time to one of the surfaces of the glass, each one dragging behind it a short tail-like process.”

Again, at the end of section I., p. 652 of the same paper, Lister says, “ From the facts detailed in this section, it appears that the aggregation of the corpuscles of blood removed from the body *depends on their possessing a certain degree of mutual adhesiveness*, which is much greater in the colourless globules than in the red disks, and that in the latter this property, though apparently not dependent upon vitality, is capable of remarkable variations *in consequence of very slight chemical changes in the liquor sanguinis.*”

From these quotations it is apparent that Mr. Lister ignores altogether the idea of the aggregation of the corpuscles being due to an *attractive force or energy*, and refers it to *adhesiveness or stickiness* of the corpuscles ; in his own words, “ they merely stick together when brought into contact by accidental causes.” At the same time he states that this adhesiveness is liable to great variations, both in the way of increase and diminution, by very slight changes in the chemical qualities of the plasma.

Dipping deeper into the writings of Lister, we find that this idea of adhesiveness or stickiness of the corpuscles is retained in his explanation of the nature of inflammatory stasis. And his views upon the subject generally may be summed up in three propositions :—

1. The blood-corpuscles exhibit no tendency to unite together in healthy blood within the vessels, although such blood may be in a state of rest.

2. The corpuscles become suddenly adhesive (in 10 seconds) when, by being shed, the blood is brought into contact with ordinary matter.

3. Irritation, by reducing the vitality of the surrounding tissues, causes them to bear the same relation to the blood within the vessels in their immediate vicinity as ordinary matter does to that which has been shed, inducing adhesiveness of the corpuscles, and thus bringing about inflammatory stasis.

These effects upon the blood-corpuscles are assumed by Lister to depend upon *chemical* changes induced by ordinary matter or by vitally degraded tissues upon the plasma of the blood ; but inasmuch as chemical changes cannot occur without corresponding physical modifications, it is quite as rational to refer the increased aggregating-tendency displayed by the corpuscles to physical as to chemical changes in the liquor sanguinis ; and this view has the advantage of not requiring us to believe that the functional activity of the tissues is depressed by mild forms of irritation—an idea which is opposed to all we know of the increased nutritive and formative changes which follow in the wake of irritation.

Having now briefly reviewed the existing position of the subject, we will proceed to consider the real causes at work in the production of the phenomenon under consideration.

Many years since, having familiarized myself with the behaviour of, and the appearances presented by blood-corpuscles under almost every conceivable condition, both within and without the vessels, I became profoundly impressed with the conviction that these phenomena had their origin in some physical law of attraction, and at the same time felt not the less certain that, if this view proved to be correct, the behaviour of the blood-corpuscles would be found to be no isolated exhibition of this law, and that, provided conditions similar to those which exist in the case of the blood-corpuscles could be obtained, many illustrative examples of the operation of the law would be immediately forthcoming.

That such attractive force did not exert its influence through distances readily appreciable was obvious, and this fact at once indicated that it must be sought for among those forms of attraction which have been designated molecular.

After much experiment and reflection I came, in 1862, to the conclusion that these phenomena were due to no less universal a law than that of *cohesive attraction* ; and I embodied the views I then held upon the subject in a paper which was read before the Royal Society, and published in the Proceedings, entitled “ The Causes of various Phenomena of Attraction and Adhesion as exhibited in Solid Bodies, Films, Vesicles, Liquid Globules, and Blood-corpuscles.” Since that time other departments of physiology have occupied my attention ; and I have only been induced to recur to the old theme because I find that, in some recent references to the history of this subject, my observations have not been mentioned, from which I am led to infer either that my views had not been sufficiently put forward, or that my experiments had failed to produce conviction in others. I therefore now present the result of a renewed investigation, in which I

believe I have established, by conclusive experiments, the correctness of my explanation of the phenomena.

Among the various modes of aggregation which the blood-corpuscles undergo, two typical forms stand prominently forward, of which all others are merely modifications. The one appears to be dependent upon the normal disk-shape, the other upon the globular or spherical form which the corpuscles assume on the addition of various substances to the blood, such as gum, gelatine, linseed mucilage, potash, &c.

With the first of these modes of aggregation, viz. into rouleaux, we are all sufficiently familiar; and an excellent notion of the character of the second form may be obtained by a careful examination of microphotographs of the blood-corpuscles which have been obtained instantaneously by exploding magnesium in heated oxygen*.

In order to leave as little as possible to hypothesis, it was desirable as a preliminary step to make sure that these differences in form of the corpuscles were the real cause of the diverse modes of arrangement—whether, in fact, we could safely predicate that disk-shaped bodies having an attraction for each other would arrange themselves so as to form rolls or cylindrical masses, and whether, on the other hand, attracting spheres of soft material would attach themselves together in such a fashion as to cause plane surfaces to be opposed to each other—in a word, to convert themselves by mutual attraction into polyhedral bodies just as they might do under mutual compression.

In the first place, we had to ascertain experimentally how disk-shaped bodies, having the utmost freedom of movement, and possessing an attraction for each other, would arrange themselves.

In casting about for the conditions to make such an experiment, I remembered a very familiar phenomenon which had often excited my curiosity, viz. the rapidity with which a bubble or a small floating fragment upon the surface of a cup of tea or other liquid rushes to the side of the containing vessel, or with which two such bubbles or fragments rush together, the moment they approach within a certain range. I determined to see if I could not make use of this attraction, the true nature of which I at the time imperfectly understood, and with this object prepared a number of circular disks of cork, which I accurately poised so that they should assume and maintain the vertical position when partially immersed in liquid. On throwing these disks into liquid, I had the satisfaction of seeing them run together and form themselves into the most perfect rouleaux after the fashion of the blood-disks.

This experiment has the value of demonstrating that if the blood-disks attracted each other, *their shape* would determine the formation of rouleaux.

As regards the behaviour of spherical vesicles or globules which attract

* Specimens of these, as well as the several experiments referred to in the paper, were exhibited to the Society.

each other, it is found that the moment any point in their convex surfaces is made to touch, these surfaces become flattened, and consequently bubbles in a group convert each other into polyhedral-shaped bodies. This effect is not due to compression, but to a *progressive mutual attraction* of the surfaces of these bodies for each other.

As soap-bubbles are vesicles with aërial contents, and are therefore physically unlike the blood-corpuscles, it became desirable to ascertain how vesicles with liquid contents would behave in regard to each other. This was accomplished by placing in a large test-tube a solution of soap, and upon its surface a stratum of petroleum an inch or so in depth; the petroleum does not mix with or injure the soap solution, which is the case with most other substances. A glass tube is now passed through the petroleum into the solution of soap below. On blowing down the tube, we succeed in forming innumerable small bodies or corpuscles of a spherical form, which are very plastic, and the contents of which consist of petroleum, and the external envelope or vesicle of soap. Corpuscles so produced float in the upper stratum of petroleum, and are found to unite themselves into groups and masses in precisely the manner of the air-bubbles, although they are entirely submerged in liquid.

These experiments show that disk-shaped bodies, having an attraction for each other, will arrange themselves in rolls or cylindrical masses, and that spherical bodies of a plastic character and vesicular structure, be their contents aërial or liquid, will attach themselves together in such a fashion as to cause plane surfaces to be opposed to each other—in a word, convert themselves by a progressive attraction, which commences at their points of mutual contact, into groups of polyhedral bodies.

The question now remaining is, do the blood-corpuscles possess such attractions for each other as those displayed by the objects with which we have been dealing? The reply is that their physical nature being analogous, if the same conditions exist, they cannot escape the influence of the same law. An examination of the photographs and of the drawings of blood-corpuscles exhibited will serve to show that these bodies are amenable to the law which is concerned in grouping together the bubbles and liquid vesicles.

But in the cases we have heretofore been considering, the disks, bubbles, and other factitious objects are not in precisely the same conditions as the blood-corpuscles—the former being only partially, or not at all submerged in liquid, while the latter are entirely so, and nevertheless they run together into rouleaux and groups. It may fairly be asked if the artificial bodies will do the same. The answer obtained by experiment is, that the moment these disks or bubbles are entirely submerged, they lose at once their attraction for each other and fall apart.

For several years I unceasingly asked myself the cause of this difference in behaviour. I at length found that when small bodies, such as disks of cork or gelatine, are first wetted with water, and then submerged in a liquid with

which water will not mix, such as oil of turpentine or petroleum, they will run together in piles or rouleaux, very much in the same way as the blood-disks.

To understand this result, a few simple primary principles must be called to mind. In the first place, the particles which compose any liquid have a mutual attraction for each other; but between the particles which compose different liquids a mutual repulsion may exist, *e. g.* water and oil, or chloroform and water. It is likewise true that there is a mutual attraction between certain liquid and rigid bodies, and also a mutual repulsion between others. Any rigid body which can be wetted by a liquid is regarded as having a cohesive attraction for it, while one which cannot be wetted is said to have no such attraction, or to exert a repulsive influence, as the case may be.

These phenomena therefore depend upon what might be justly termed *double cohesion*—cohesion in the first place between the rigid body and the liquid, and in the second place between the particles of the liquid itself.

If, now, we examine into the cases in which we have complete submergence, viz. the blood-rolls, the gelatine disks, and the loaded cork disks, we find the same law to be in operation. These bodies must all be regarded as localizers of liquids, either by their cohesive attraction for liquids, or, as in the case of the blood-corpuscles, by being receptacles containing liquids.

If the cork disks, bubbles, or other bodies are entirely submerged in water, all attraction ceases, and this because a cohesive equilibrium is established; there is no longer any differentiation such as exists between water and air. If, however, after having wetted these bodies in water, we completely submerge them in a liquid which has a cohesive antagonism to water, or even a liquid which has simply no cohesion for water, which may be known by the insolubility and immiscibility of one liquid in the other, such as turpentine or petroleum, we get the phenomena of attraction precisely as in the atmosphere. This fact is illustrated by taking the cork disks from the water in which they are non-adherent, and placing them in the vessel of petroleum, in which they become instantly attractive of each other.

This principle is further illustrated by the gelatine disks, which are first made to absorb as much water as possible, and are then submerged in petroleum.

In all these cases there are present, therefore, two dissimilar or antagonistic liquids; and upon the presence of these the phenomena depend.

My idea of the blood-corpuscle is that its contents are something essentially different, so far as cohesive attraction is concerned, from the liquor sanguinis—that is to say, not readily miscible with liquor sanguinis. This is of course self-evident, if, according to some modern views, we regard the corpuscles “as tiny lumps of a uniformly viscous matter,” inasmuch as such matter must be insoluble in and immiscible with the liquor sanguinis.

The explanation is equally easy if we accept the old and, I believe, the true view, of the vesicular character of these bodies, as we have only to assume that the envelope is so saturated with the corpuscular contents as practically to act as such contents would themselves act, *i. e.* to exhibit a greater cohesive attraction for their own particles than for those of the contiguous liquid.

The cohesive power of the blood-corpuscles varies with varying conditions of the liquor sanguinis; and this is doubtless due to the law of osmosis; for we can readily imagine that when the exosmotic tendency is in excess, the corpuscles will become more adhesive, and, on the contrary, when the endosmotic current prevails, less so. In any case the increased cohesiveness will be due to the increased extrusion of the corpuscular contents upon the surface.

All, then, that is required in the case of the blood-corpuscles, is a difference between their liquid contents and the plasma in which they are submerged. That this difference is not so great as between the liquids used in these experiments is probable; but it must also be remembered that the attraction is not so powerful. The power required to attach the blood-corpuscles together is, on account of their exceeding minuteness, extremely small, as they are thus so much more removed from the influence of gravitation, and brought under that of molecular attraction.

I shall conclude this paper by a brief reference to inflammatory stasis. In one of my papers (communicated to the Royal Society in 1862) I described no less than four distinct forms of stasis. I proposed to designate that induced by irritation homogeneous stasis, because the blood-corpuscles become so blended together as to entirely lose their outlines and present the appearance of a uniform and continuous plug filling up the capillaries.

This peculiar blending of the corpuscles is dependent upon the law I have been describing, *viz.* that of double cohesion, and is brought about by diminished quantity of liquor sanguinis in a part in proportion to the corpuscles, and by loss of fluidity in that which remains.

One of the primary effects of irritation is neural paralysis of the minute arteries which supply capillary tracts; and this paralysis gives rise to increased diosmotic action, in fact to exudation of liquor sanguinis; consequently there is a lagging behind of the corpuscles, and an increase of their numbers in the capillaries; the plasma, too, which still surrounds the corpuscles in the capillaries, is modified; and when a certain relation has been reached between the corpuscles and the plasma, the former blend together precisely in the same manner as the soap-bubbles, or as the blood-corpuscles exhibited in the photographs. This completely arrests the passage of blood through the capillaries, which become as much occluded as if blocked up by solid fibrin.

I have frequently had opportunities of watching in the transparent webs of frogs the mode in which this homogeneous stasis is resolved. In these creatures the restoration of the circulation commences some hours after the

application of the irritant. When the circulation is about to be resumed, the stagnating mass in the vessel appears to thaw as it were. The corpuscles are not pushed onwards in mass as a coherent plug; but the homogeneity of appearance is suddenly lost by the resumption of their normal form by the corpuscles and the reappearance of their differentiating outlines, which were previously obscured by their blending with one another and with the walls of the vessels. Before this takes place, the vessel very gradually assumes a lighter tint, passing in some instances from a deep red to a pale orange. This appears to be due to a washing away of extruded colouring-matter.

When this change from homogeneity to heterogeneity commences, although sufficiently progressive in its character as it traverses the vessel, it nevertheless takes place with considerable rapidity. It is evidently brought about by the gradual permeation of new liquor sanguinis among the corpuscles, and the contemporaneous abolition of their cohesive attraction for each other in accordance with the principles previously established.

II. "Researches on Turacine, an Animal Pigment containing Copper."

By A. W. CHURCH, M.A. Oxon., Professor of Chemistry in the Royal Agricultural College, Cirencester. Communicated by Dr. W. A. MILLER, Treas. R.S. Received May 4, 1869.

(Abstract.)

From four species of *Touraco*, or Plantain-eater, the author has extracted a remarkable red pigment. It occurs in about fifteen of the primary and secondary pinion feathers of the birds in question, and may be extracted by a dilute alkaline solution, and reprecipitated without change by an acid. It is distinguished from all other natural pigments yet isolated, by the presence of 5·9 per cent. of copper, which cannot be removed without the destruction of the colouring-matter itself. The author proposes the name *turacine* for this pigment. The spectrum of turacine shows two black absorption-bands, similar to those of scarlet cruorine; turacine, however, differs from cruorine in many particulars. It exhibits great constancy of composition, even when derived from different genera and species of Plantain-eater; as, for example, the *Musophaga violacea*, the *Corythaix albo-cristata*, and the *C. porphyreolopha*.

III. "On the Radiation of Heat from the Moon." By the EARL OF ROSSE, F.R.S. Received May 27, 1869.

The following experiments on Lunar Radiant Heat were undertaken with the view of ascertaining whether with more powerful and more suitable means than those previously employed by others, with little or no success, it would be possible to detect and estimate the amount of heat which reaches the earth's surface from the moon.

Professor Piazzi Smyth had conducted a series of experiments on the Peak of Teneriffe with a thermopile, but apparently without any means of concentrating the moon's heat beyond the ordinary polished metal cone.

Melloni had employed a glass lens of considerable diameter (I believe about three feet); but as glass absorbs rays of low refrangibility, it was not so well adapted to concentrate heat as a metallic mirror.

In the following experiments the point sought to be determined was, in what proportions the moon's heat consists of

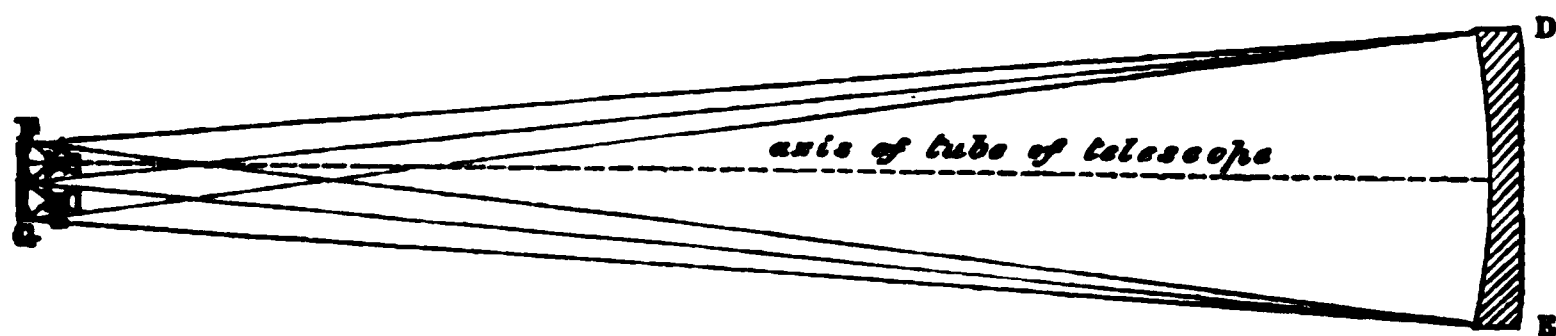
(1) That coming from the interior of the moon, which will not vary with the phase.

(2) That which falls from the sun on the moon's surface, and is at once reflected regularly and irregularly.

(3) That which, falling from the sun on the moon's surface, is absorbed, raises the temperature of the moon's surface, and is afterwards radiated as heat of low refrangibility.

The apparatus consisted of a thermopile of four elements, the faces half an inch square, on which all the moon's heat which falls on the large speculum of the 3-foot telescope is concentrated, by means of a concave mirror of $3\frac{1}{2}$ inches aperture, 2·8 inches focal length.

As it was found difficult to compensate the effects of unequal radiation on the anterior face of the pile, by exposing the posterior face also of the *same* pile to radiation from the sky, during the later experiments (beginning with March 23rd) two piles were used, and the following was the form of apparatus adopted.



D E is the large mirror of the telescope; F G the two small concave mirrors of $3\frac{1}{2}$ inches aperture, and 2·8 inches focal length, fixed in the plane of the image formed by the large mirror D E. The two thermopiles are placed respectively in the foci of F and G, their anterior faces shielded from wind and other disturbing causes by polished brass cones, and their posterior faces kept at a nearly uniform temperature by means of brass caps filled with water. The thermopiles and accompanying mirrors are supported by a bar screwed temporarily on the mouth of the tube. Two wires are connected with the two poles of each pile; and the ends of the wires are connected, two and two, close to the galvanometer, in such a manner that a given amount of heat on the anterior face of one pile will produce a deviation equal in amount, and opposite in direction, to that produced by an equal amount of heat on the anterior face of the other pile. Thomson's Reflecting Galvanometer was the one used.

This apparatus has not yet had a fair trial, as I was unable from Messrs. Elliot a pile ready made of similar dimensions to I already possessed. That which they sent had only one-fourth quired area of face.

The following is a summary of the results :—

Reference number.	Date of observation.	Mean error.	Mean deviation.	Deviation (calculated).	Observed deviation reduced to full moon.	180° - moon's distance from the sun.	Mean altitude of moon.	Number of readings.	
I.	1868. Dec. 30.	...	103.7	94.1	110	19			
II.	" 31.	..	85.1	85.8	99.2	33			
III.	1869. Jan. 1.	..	67.5	73.1	92.1	47			
IV.	" 21.	..	34	41.9	81.1	79	Occasional clouds
V.	" 26.	..	83	96.7	85.8	15	56		White frost. M dewed; but taken after th have been reje
VI.	Mar. 23.	34	57	67.7	84.2	57	...	40	Occasional clouds
VII.	" 27.	49	115	99.6	115	5	35	15	Occasional clou gusts of wind.
VIII.	" 28.	35	113	96.1	117	16	30	49	No note of clou breeze, general
IX.	" 31.	...	17	62.8	27.7	58	■	31	Moon low, sky hazy clouds, th the moon was a diminished bri
X.	April 14.	8.3	123	...	4	Very clear and moon low; no impulse impa needle.
XI.	" 17.	27	13.1	16.6	79	110	27	65	Wind blowing the mouth of th the whole time
XII.	" 19.	43	35.5	36.3	96	85	25	14	No note of clou the end of th tions.
XIII.	" 20.	85	33	48.8	68	72	35	51	A very little win clouds.
XIV.	" 22.	■	12.1	75.5	45	...	15	Halo with lu moon seen th with much-dim liancy.
XV.	" 24.	28	84	95.3	88.2	18	30	29	Frequent passin ring the latter observations.
XVI.	" 25.	45	88.4	90.4	88.8	6	25	66	No cloud visible suspected, as it at sunset and a
1	2	3	4	5	6	7	8	9	

In column 3 is given the mean of the deviations of all the single differences from the mean difference of all the readings taken with the moon on and with the moon off the apparatus.

In column 4 the arithmetic mean of all the observed deviations.

In column 5 the calculated deviation for each night at midnight, on the assumption that the deviation corresponding to full moon = 100, and that the moon is a smooth sphere. We have then

Q (quantity of heat coming from the moon's surface)

$$= C \int_{\epsilon - \frac{\pi}{2}}^{\frac{\pi}{2}} \cos \theta \cdot \cos (\epsilon - \theta) d\theta$$

$$= \frac{C}{2} \{ \pi - \epsilon \cdot \cos \epsilon + \sin \epsilon \}^*,$$

where $\epsilon = \pi -$ apparent distance between the centres of the sun and moon.

When $\epsilon = 0$ (full moon), $Q = \frac{C}{2} \cdot \pi$,

when $\epsilon = \frac{\pi}{2}$ (half moon), $Q = \frac{C}{2}$,

when $\epsilon = \pi$ (new moon), $Q = 0$;

\therefore if full moon = 100, Q in general

$$= 100 \left(1 - \frac{\epsilon}{\pi} \cos \epsilon + \sin \epsilon \right). \quad \dots \dots \dots (a)$$

In column 6 we have the deviation for full moon calculated from the observed mean deviation for each night.

In column 7 the supplement of the apparent distance between the centres of the sun and moon.

In column 8 the approximate mean altitude of the moon.

In column 9 the number of times the telescope was put on or off the moon during the observations included in the mean result.

In all these observations the deviations which have been measured are those due to the difference between the radiation from a circle of sky containing the moon's disk, and that from a similar circle of sky close to it not containing the moon's disk.

The annexed diagram will show approximately the rate at which the moon's light increases and diminishes with its phases as deduced from formula (a); and the ringed dots with the accompanying Roman figures (for reference) give the quantity of the moon's heat as determined by observation on different nights.

Although there is considerable discordance between some of the observed

* This formula is based on the assumption that the heat coming to the earth from an element (δS) of the moon's surface = $K \cdot \delta S \cdot \cos \theta \cdot \cos \phi$, θ and ϕ being respectively the inclinations of the lines to the sun and to the earth from the normal to that point of the moon's surface, and K a constant.

Owing to the extremely uncertain state of the weather, only one series of eighteen readings was obtained for the determination of the sun's heat. A beam of sunlight was thrown, by means of a plane mirror, alternately on and off a plate of polished metal with a hole $\cdot 175$ inch in diameter. At a short distance behind this the pile was placed. The deviation thus found was

connected with that previously found for Full Moon by using the radiation produced by a vessel of hot water as a term of comparison.

The relative amount of solar and lunar radiation thus found was

$$89819 : 1, \quad . \quad . \quad . \quad . \quad .$$

which is quite as near that given by (b) as we could expect when considering the roughness of the data.

As a further confirmation of the correctness of the two rough calculations to the value of the ratio existing between the sun's and the radiant heat already given, the subject was investigated from a theoretical point of view. It was assumed

(1) That the quantity of heat leaving the moon at any instant without much error be considered the same as that falling on it at that

(2) That the absorptive power of our atmosphere is the same for solar and lunar heat.

(3) That, as was already assumed in obtaining formula (a), the moon is a smooth sphere not capable of reflecting light regularly. Then the quantity of heat which leaves the moon in all directions = quantity which falls on the moon = $\frac{1}{13.55}$ of the quantity which falls on the earth from the sun

$$= K \cdot \int_0^\pi \{(\pi - e) \cdot \cos e + \sin e\} \sin e \cdot de = \frac{K}{4} 3\pi.$$

The part which falls on the earth

$$\begin{aligned} &= K \cdot \int_0^{59.964} \{(\pi - e) \cos e + \sin e\} \sin e \cdot de \\ &= \frac{K}{4} \times \left\{ -\pi \cdot \text{versin } (1^\circ 55') + \frac{2 + \cos (1^\circ 55')}{59.964} - \frac{1}{3} \sin (1^\circ 55') \right\} \\ &= \frac{K}{4} \cdot E \text{ suppose;} \end{aligned}$$

therefore (if we may be allowed the expression)

$$\begin{aligned} \frac{\text{sun-heat}}{\text{moon-heat}} &= \frac{13.55 \times 3\pi}{E} \\ &= \frac{79,000}{1} \text{ (quam proximè).} \quad . \quad . \quad . \end{aligned}$$

In the above, the proportion between the areas of surface presented to the moon and earth to the sun is taken = 13.55, and the angle subtended by the earth at the moon = $1^\circ 55'$.

The value of the readings of the galvanometer was determined by comparison with those obtained by using a vessel of hot water coated with lampblack and lampblack varnish as a source of heat. The vessel was of tin, circular, and subtended the same angle at the small concave reflectors as the mirror of the telescope. It was thus found that (the radiating power of the moon being supposed equal to that of the lampblack surface at earth's atmosphere not to influence the result) a deviation of 90 f

moon appears to indicate an elevation of temperature through 500° Fahr.* In deducing this result allowance has been made for the *imperfect* absorption of the sun's rays by the lunar surface.

In the present imperfect state of these observations it would be premature to discuss them at greater length ; but as some months must elapse before any more complete series can be obtained, and the present results are sufficient to show conclusively that the moon's heat is capable of being detected with certainty by the thermopile, I have thought it best to send this account to the Royal Society ; and I shall be most happy to receive suggestions as to improvements in the method of working, and as to the direction in which it may be most desirable to carry on future experiments.

IV. "On a New Arrangement of Binocular Spectrum-Microscope."

By WILLIAM CROOKES, F.R.S. &c. Received April 23, 1869.

The spectrum-microscope, as usually made, possesses several disadvantages : it is only adapted for one eye† ; the prisms having to be introduced over the eyepiece renders it necessary to remove the eye from the instrument, and alter the adjustment, before passing from the ordinary view of an object to that of its spectrum, and *vice versa* ; the field of view is limited, and the dispersion comparatively small.

I have devised, and for some time past have been working with, an instrument in which the above objections are obviated, although at the same time certain minor advantages possessed by the ordinary instrument, such as convenience of examining the light reflected from an object, and comparing its spectrum with a standard spectrum, are not so readily associated with the present form of arrangement.

The new spectrum-apparatus consists of two parts, which are readily attached to an ordinary single or binocular microscope ; and when attached they can be thrown in or out of adjustment by a touch of the finger, and may readily be used in conjunction with the polariscope or dichroscope ; object-glasses of high or low power can be used, although the appearances are more striking with a power of $\frac{1}{2}$ -inch focus or longer ; and an object as small as a single corpuscle of blood can be examined and its spectrum observed.

* This may seem a very large rise of temperature ; but it is quite in accordance with the views of Sir John Herschel on the subject (Outlines of Astronomy, section 432 and preceding sections), where he says that, in consequence of the long period of rotation of the moon on its axis, and still more the absence of an atmosphere, " The climate of the moon must be most extraordinary, the alternation being that of unmitigated and burning sunshine, fiercer than that of an equatorial noon ; and the keenest severity of frost, far exceeding that of our polar winters, for an equal time." And again, "... the surface of the full moon exposed to us must necessarily be very much heated, possibly to a degree much exceeding that of boiling water."

† Mr. Sorby in several of his papers (Proc. Roy. Soc. 1867, xv. p. 433 ; 'How to Work with the Microscope,' by L. Beale, F.R.S., 4th edition, p. 219) refers to a binocular spectrum-microscope ; but he gives no description of it, and in one part says that it is not suited for the examination of any substance less than $\frac{1}{16}$ of an inch in diameter.

The two additions to the microscope consist of the substage with slit &c., and the prisms in their box. The substage is of the ordinary construction, with screw adjustment for centring, and rackwork for bringing it nearer to or withdrawing it from the stage. Its general appearance is shown in fig. 1, which represents it in position. A B is a plate of brass,

Fig. 1.



sliding in grooves attached to the lower part of the substage; it carries an adjustable slit, C, a circular aperture, D, 0.6 inch in diameter, and an aperture, O, $\frac{1}{8}$ inch square. A spring top enables either the slit or one of the apertures to be brought into the centre of the field without moving the eye from the eyepiece. Screw adjustments enable the slit to be widened or narrowed at will, and also varied in length. At the upper part of the substage is a screw of the standard size, into which an object-glass of high power is fitted. E represents one in position. I generally prefer a $\frac{1}{4}$ -inch power; but it may sometimes be found advisable to use other powers here.

The slit C and the object glass E are about 2 inches apart; and if light is reflected by means of the mirror along the axis of the instrument, it is evident that the object-glass E will form a small image of the slit C, about 0·3 inch in front of it. The milled head F moves the whole substage up or down the axis of the microscope, whilst the screws G and H, at right angles to each other, will bring the image of the slit into any desired part of the field. If the slide A B is pushed in so as to bring the circular aperture D in the centre, the substage arrangement then becomes similar to the old form of achromatic condenser. Beneath the slit C is an arrangement for holding an object, in case its surface is too irregular, or substance too dense, to enable its spectrum to be properly viewed in the ordinary way*.

Supposing an object is on the upper stage of the microscope (shown in fig. 2) and viewed by light transmitted from the mirror through the large aperture D and the condenser E, by pushing in the slide A B so as to bring the slit C into the field, and then turning the milled head F, it is evident that a luminous image of the slit C can be projected on to the object; and by proper adjustment of the focus, the object and the slit can be seen together equally sharp. Also, since the whole of the light which illuminated the object has been cut off, except that portion which passes through the slit, all that is now visible in the instrument is a narrow luminous line, in which is to be seen just so much of the object as falls within the space this line covers. By altering the slit-adjustments the length or width of the luminous line can be varied, whilst by means of the rackwork attached to the upper stage, any part of the object may be superposed on the luminous line. The stage is supplied with a concentric movement, which permits the object to be rotated whilst in the field of view, so as to allow the image of the slit to fall on it in any direction. During this examination a touch with the finger will at any time bring the square aperture O, or the circular aperture D into the field, instead of the slit, so as to enable the observer to see the whole of the object; and in the same manner the slit can as easily be again brought into the field.

The other essential part of this spectrum-microscope consists of the prisms. These are enclosed in a box, shown at K (fig. 2). The prisms are of the direct-vision kind, consisting of three flint and two crown, and are altogether 1·6 inch long. The box screws into the end of the microscope-body at the place usually occupied by the object-glass; and the object-glass is attached by a screw in front of the prism-box. It is shown in its place at L. The prism-box is sufficiently wide to admit of the prisms being pushed to the side when not wanted, so as to allow the light, after passing

* In carrying out the experiments which were necessary before this spectrum-microscope could be made in its present complete form, I have been greatly assisted by Mr. C. Collins, Philosophical-Instrument Maker, 77 Great Tichfield Street, to whom I am also indebted for useful suggestions as to the most convenient arrangement of the different parts, so as to render them easily adapted to microscopes of ordinary construction.

of view, all that is necessary is to push the slit into adjustment with one hand, and the prisms with the other. The spectrum of any object which is superposed on the image of the slit is then seen.

The small square aperture at O (fig. 1) is for the examination of dichroic substances. When this is pushed into the field, by placing a double-image prism P between A B and E, two images of the aperture are seen in juxtaposition, oppositely polarized; and if a dichroic substance is on the stage, the differences of colour are easily seen.

When the spectrum of any substance is in the field and the double-image prism P is introduced, two spectra are seen, one above the other, oppositely polarized, and the variations in the absorption-lines, such as are shown by didymium, jargonium, &c., are at once seen.

A Nicol's prism, Q, as polarizer, is also arranged to slip into the same position as the double-image prism, and another, R, as analyzer, above the prism-box. The spectra of the brilliant colours exhibited by certain crystalline bodies, when seen by polarized light, can then be examined. Many curious effects are then produced, a description of which I propose to make the subject of another paper. Both the prisms P and Q are capable of rotation.

If the substance under examination is dark coloured, or the illumination is not brilliant, it is best not to divide the light by means of the Wenham prism at N, but to let the whole of it pass up the tube to one eye. If, however, the light is good, a very great advantage is gained by throwing the Wenham prism into adjustment and using both eyes. The appearance of the spectrum, and the power of grasping faint lines, are incomparably superior when both eyes are used; whilst the stereoscopic effect it confers on some absorption and interference spectra (especially those of opals) seem to throw entirely new light on the phenomena. No one who has worked with a stereoscopic spectrum-apparatus would willingly return to the old monocular spectroscop^e.

If the illumination in this instrument is taken from a white cloud or the sky, Fraunhofer's lines are beautifully visible; and when using direct sunlight they are seen with a perfection which leaves little to be desired. The dispersion is sufficient to cause the spectrum to fill the whole field of the microscope, instead of, as in the ordinary instrument, forming a small portion of it, the dispersion being four or five times as great; whilst, owing to the very perfect achromatism of the optical part of the microscope, all the lines from B to G are practically in the same focus.

As the only portion of the object examined is that part on which the image of the slit falls, and as this is very minute (varying from 0.01 to

* It is not difficult to convert an ordinary spectroscop into a binocular instrument. The rays after leaving the object-glass of the telescope are divided into two separate bundles and received on two eyepieces properly mounted. As it is immaterial whether the spectrum be stereoscopic or pseudoscopic, a simpler form of prism than Mr. Wenham's arrangement can be used.

0·001 inch, according to the actual width of the slit), it is evident that the spectrum of the smallest objects can be examined. If some blood is in the field, it is easy to reduce the size of the image of the slit to dimensions covered by one blood-disk, and then, by pushing in the prisms, to obtain its spectrum.

If the object under examination will not transmit a fair image of the slit (if it be a rough crystal of jargoon for instance), it must be fixed in the universal holder beneath the slit and the light concentrated on it before it reaches the slit. If the spectra of opaque objects are required, they can also be obtained in the same way, the light being concentrated on them either by a parabolic reflector or by other appropriate means.

By replacing the illuminating lamp by a spirit-lamp burning with a soda-flame, and pushing in the spectrum-apparatus, the yellow sodium-line is seen beautifully sharp; and by narrowing the slit sufficiently it may even be doubled. Upon introducing lithium or thallium compounds into the flame, the characteristic crimson or green line is obtained; in fact so readily does this form of instrument adapt itself to the examination of flame-spectra, that for general work I have almost ceased to use a spectroscope of the ordinary form. The only disadvantage I find is an occasional deficiency of light; but by an improved arrangement of condensers I hope soon to overcome this difficulty.

V. "On some Optical Phenomena of Opals."

By WILLIAM CROOKES, F.R.S. &c. Received April 23, 1869.

When a good fiery opal is examined in day-, sun-, or artificial light, it appears to emit vivid flashes of crimson, green, or blue light, according to the angle at which the incident light falls, and the relative position of the opal and the observer; for the direction of the path of the emitted beam bears no uniform proportion to the angle of the incident light. Examined more closely, the flashes of light are seen to proceed from planes or surfaces of irregular dimensions inside the stone, at different depths from the surface and at all angles to each other. Occasionally a plane emitting light of one colour overlaps a plane emitting light of another colour, the two colours becoming alternately visible upon slight variations of the angle of the stone; and sometimes a plane will be observed which emits crimson light at one end, changing to orange, yellow, green, &c., until the other end of the plane shines with a blue light, the whole forming a wonderfully beautiful solar spectrum in miniature. I need scarcely say that the colours are not due to the presence of any pigment, but are interference colours caused by minute striæ or fissures lying in different planes. By turning the opal round and observing it from different directions, it is generally possible to get a position in which it shows no colour whatever. Viewed by transmitted light, opals appear more or less deficient in transparency and have a slight greenish yellow or reddish tinge.

In order to better adapt them to the purposes of the jeweller, opals are almost always polished with rounded surfaces, back and front; but the flashes of coloured light are better seen and examined when the top and bottom of the gems are ground and polished flat and parallel.

A good opal is not injured by moderate heating in water, soaking in turpentine, or heating strongly in Canada balsam and mounting as a microscopic slide.

By the kindness of Mr. W. Chapman, of Frith Street, Soho, and other friends, I have been enabled to submit some thousands of opals to optical examination; and from these I have selected about a dozen which appeared worthy of further study.

If an opal which emits a fine broad crimson light is held in front of the slit of a spectroscope or spectrum-microscope, at the proper angle, the light is generally seen to be purely homogeneous, and all the spectrum that is visible is a brilliant luminous line or band, varying somewhat in width and more or less irregular in outline, but very sharp, and shining brightly on a perfectly black ground. If, now, the source of light is moved, so as to shine into the spectrum-apparatus *through* the opal, the above appearance is reversed, and we have a luminous spectrum with a jet-black band in the red, identical in position, form of outline, and sharpness with the luminous band previously observed. If instead of moving the first source of light (the one which gave the reflected luminous line in the red) another source of light be used for obtaining the spectrum, the two appearances, of a coloured line on a black ground, and a black line on a coloured ground, may be obtained simultaneously, and they will be seen to fit accurately.

Those parts of the opal which emit red light are therefore seen to be opaque to light of the same refrangibility as that which they emit; and upon examining in the same manner other opals which shine with green, yellow, or blue light, the same appearances are observed, showing that this rule holds good in these cases also. It is doubtless a general law, following of necessity the mode of production of the flashes of colour.

Having once satisfied myself that the above law held good in all the instances which came under my notice, I confined myself chiefly to the examination of the transmitted spectra, although the following descriptions will apply equally well, *mutatis mutandis*, to the reflected spectra. The examinations were made by means of the spectrum-microscope a description of which I have just had the honour of sending to the Society. This instrument is peculiarly adapted to examinations of this sort, both on account of the small size of the object which can be examined in it, and also as it permits the use of both eyes in viewing the spectrum.

The following is a brief description of some of the most curious transmission spectra shown by these opals. The accompanying figures, drawn with the camera lucida, convey as good an idea as possible of the different appearances. The exact description will of course only hold good for one

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portion of the opal; but the general character of each individual stone is well marked.

No. 1 shows a single black band in the red. When properly in focus this has a spiral structure. Examined with both eyes it appears in decided relief, and the arrangement of light and shade is such as to produce a striking resemblance to a twisted column.

No 2. gives an irregular line in the orange. Viewed binocularly, this exhibits the spiral structure in a marked manner, the different depths and distances standing well out; upon turning the milled head of the stage-adjustment, so as to carry the opal slowly from left to right, the spiral line is seen to revolve and roll over, altering its shape and position in the spectrum. It is not easy to retain the conviction that one is looking merely at a band of deficient light in the spectrum, and not at a solid body, possessing dimensions and in actual motion.

No. 3 has a line between the yellow and green, vanishing to a point at the top, and near the bottom having a loop, in the centre of which the green appears. Higher up, in the green, is a broad green band, indistinct on one side and branching out in different parts.

No. 4 has a broad, indistinct, and sloping band in the blue, and another, still more indistinct, in the violet.

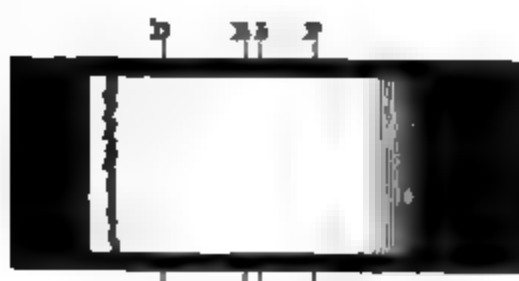
No. 5 has a band in the yellow, not very sharp on one side, and somewhat sloping. Upon moving the opal sideways, it moves about from one part of the yellow field to another. In one position it covers the line D, and is opaque to the sodium-flame of a spirit-lamp.

No. 6 shows a curiously shaped band in the red, very sharp and black, and terminating in one part at the line D. In the yellow there is a black dot. The spectrum of this opal showed by reflected light intensely bright red bands, of the shape of the transmission bands. On examining this opal with a power of 1 inch, in the ordinary manner, the portion giving this spectrum appeared to glow with intense red light, and was bounded with a tolerably definite outline. Without altering any other part of the microscope, the prisms were then pushed in so as to look at the whole surface of the opal through the prisms, but without the slit. The shape and appearance of the red patch were almost unaltered; and here and there over other parts of the opal were seen little patches of homogeneous light, which, not having been fanned out by the prisms, retained their original shape and appearance.

No. 7 shows a black patch in the red, only extending a little distance, and a line in the yellow. On moving the opal the line in the red vanishes, and the other line changes its position and form.

No. 8 shows the most striking example of a spiral rotating line which I have yet met with. On moving the opal sideways the line is seen to start from the red and roll over, like an irregularly shaped and somewhat hazy corkscrew, into the middle of the yellow. The drawing shows the appearance of this band in two positions.

No. 1.



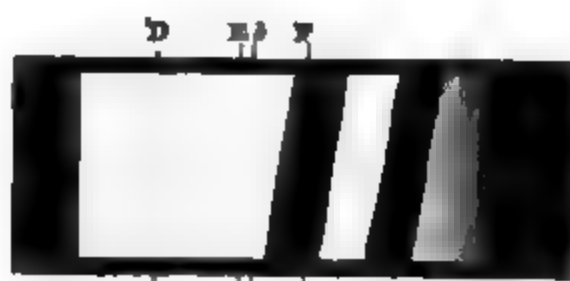
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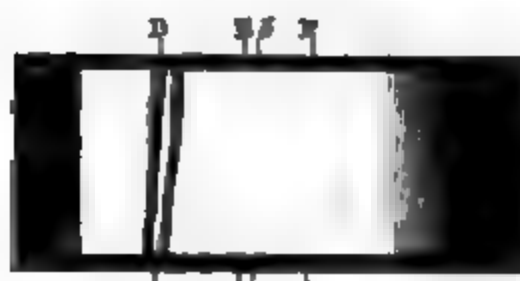
No. 3.



No. 4.



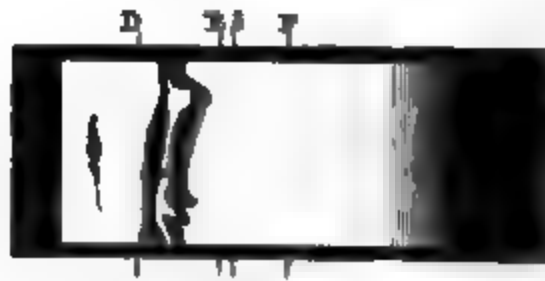
No. 5.



No. 6.



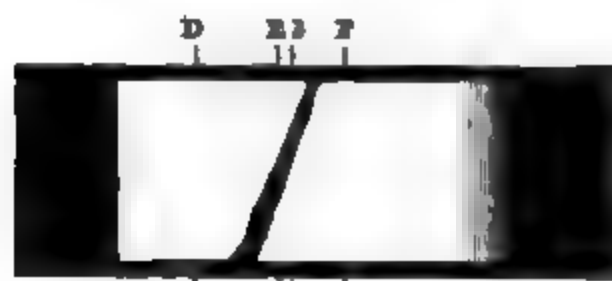
No. 7.



No. 8.



No. 9.



No. 10.

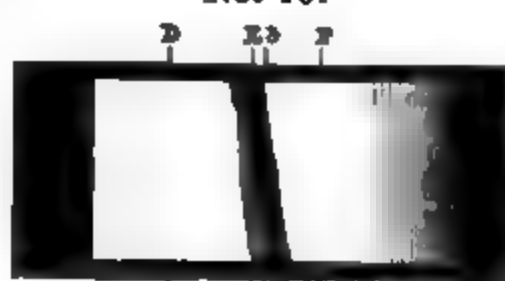
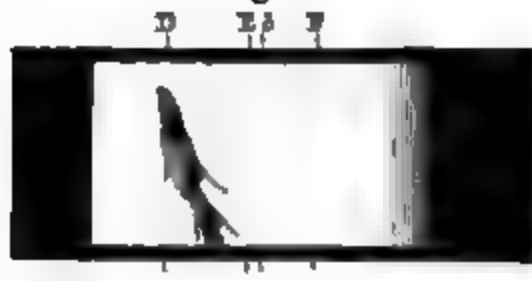
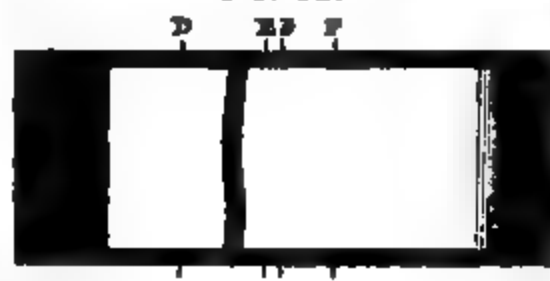


Fig. 11.



No. 12.



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No. 9 is one of the most curious. A broad black and sharp band stretches diagonally across the green, touching the blue at the top and the yellow at the bottom.

No. 10 gives a diagonal band, wide, but straight, and tolerably sharp across the green. By rotating these opals, 9 and 10, in azimuth, whilst in the field of the instrument, the lines can be made to alter in inclination until they are seen to slope in the opposite direction.

No. 11 gives another illustration of a diagonal line, across the yellow and green, not extending quite to the top.

No. 12 is one of the best examples I have met with of a narrow, straight, and sharply cut line. It is in the green, and might easily be mistaken for an absorption-band caused by an unknown chemical element.

Other opals are exhibited which show a dark band travelling along the spectrum, almost from one end to the other, as the opal is moved sideways.

It is scarcely necessary to say that the colour of the moving luminous line varies with the part of the spectrum to which it belongs. The appearance of a luminous line, slowly moving across the black field of the instrument, and assuming in turn all the colours of the spectrum, is very beautiful.

All these black bands can be reversed, and changed into luminous bands, by illuminating the opal with reflected light. They are, however, more difficult to see; for the coloured light is only emitted at a particular angle, whilst the special opacity to the ray of the same refrangibility as the emitted ray holds good for all angles.

The explanation of the phenomena is probably as follows:—In the case of the moving line, the light-emitting plane in the opal is somewhat broad, and has the property of giving out at one end, along its whole height and for a width equal to the breadth of the band, say, red light; this merges gradually into a space emitting orange, and so on throughout the entire length of the spectrum, or through that portion of it which is traversed by the moving line in the instrument, the successive pencils (or rather ribbons) of emitted light passing through all degrees of refrangibility. It is evident that if this opal is slowly passed across the slit of the spectrum-microscope, the slit will be successively illuminated with light of gradually increasing refrangibility, and the appearance of a moving luminous line will be produced; and if transmitted light is used for illumination, the reversal of the phenomena will cause the production of a black line moving along a coloured field. A diagonal line will be produced if an opal of this character is examined in a sloping position.

The phenomenon of a spiral line in relief, rolling along as the opal is moved, is doubtless caused by modifying planes at different depths and connected by cross planes; I can form a mental picture of a structure which would produce this effect, but not clear enough to enable me to describe it in words.

It is probable that similar phenomena may be seen in many, if not all, bodies which reflect coloured light after the manner of opals. A magnificent specimen of Lumacelli, or Fiery Limestone, from Italy, kindly presented to me by my friend David Forbes, shows two sharp narrow and parallel bands in the red. I have also observed similar appearances in mother-of-pearl. The effects can be imitated to a certain extent by examining "Newton's rings," formed between two plates of glass, in the spectrum-instrument.

June 3, 1869.

The Annual Meeting for the election of Fellows was held this day.

Lieut.-General SABINE, President, in the Chair.

The Statutes relating to the election of Fellows having been read, Mr. Balfour Stewart and Dr. Maxwell Simpson were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following Candidates were declared to be duly elected into the Society.

Sir Samuel White Baker, M.A.
John J. Bigsby, M.D.
Charles Chambers, Esq.
William Esson, Esq., M.A.
Prof. George Carey Foster, B.A.
William W. Gull, M.D.
J. Norman Lockyer, Esq.
John Robinson McClean, Esq.
St. George Mivart, Esq.

John Russell Reynolds, M.D.
Vice-Admiral Sir Robert Spencer
Robinson, K.C.B.
Major James Francis Tennant, R.E.
Prof. Wyville Thomson, LL.D.
Col. Henry Edward Landor Thuillier,
R.A.
Edward Walker, Esq., M.A.

Thanks were voted to the Scrutators.

June 10, 1869.

Lieut.-General SABINE, President, in the Chair.

Dr. J. J. Bigsby, Prof. G. Carey Foster, Mr. J. R. McClean, Mr. St. George Mivart, and Dr. J. Russell Reynolds were admitted into the Society.

The following communications were read :—

- I. "Researches on Gaseous Spectra in relation to the Physical Constitution of the Sun, Stars, and Nebulæ."—Second Note. By E. FRANKLAND, F.R.S., and J. N. LOCKYER. Received May 5, 1869.

We beg to lay before the Royal Society some further results of the researches on which we are engaged.

I. The Fraunhofer line on the solar spectrum, named λ by Ångström which is due to the absorption of hydrogen, is not visible in the spectrum obtained by the employment of low battery and Leyden-jar power; it may be looked upon therefore as an indication of relatively high temperature. As the question has been reversed by one of us in the spectrum of the chromosphere, it follows that the chromosphere, when cool enough to absorb the Fraunhofer line, is still of a relatively high temperature.

II. Under certain conditions of temperature and pressure, the very complicated spectrum of hydrogen is reduced in our instrument to one bright line in the green corresponding to F in the solar spectrum.

III. The equally complicated spectrum of nitrogen is similarly reduced to one bright line in the green, with traces of other more refrangible faint lines.

IV. From a mixture of the two gases we have obtained a combined spectrum of the two spectra in question, the relative brilliancy of the two bright lines varying with the amount of each gas present in the mixture.

V. By removing the experimental tube a little further away from the slit of the spectroscope, the combined spectra referred to in II. & III. were reduced to the two bright lines.

VI. By reducing the temperature all spectroscopic evidence of nitrogen vanished; and by increasing it, many new nitrogen-lines appeared on their appearance, the hydrogen-line always remaining visible.

The bearing of these latter observations on those made on the nebulae by Mr. Huggins, Father Secchi, and Lord Rosse is at once obvious. The visibility of a single line of nitrogen has been taken by Mr. Huggins to indicate possibly, first, "a form of matter more elementary than nitrogen and which our analysis has not yet enabled us to detect", and secondly, "a power of extinction existing in cosmical space"†.

Our experiments on the gases themselves show not only that the above assumptions are unnecessary, but that spectrum analysis here presents with a means of largely increasing our knowledge of the physical constitution of these heavenly bodies.

Already we can gather that the temperature of the nebulae is lower than that of our sun, and that their tenuity is excessive; it is also a question whether the continuous spectrum observed in some cases may not be due to gaseous compression.

II. "On the Molar Teeth, lower Jaw, of *Macrauchenia patachonica*, Ow." By Professor OWEN, F.R.S. Received April 21, 1866

(Abstract.)

The intraneural course of the vertebral arteries is limited, in the *Mammalia*, to the Ungulate Series, and is present in very few of these.

* Phil. Trans. 1864, p. 444.

† Ibid. 1868, p. 544.

existing species it characterizes the *Camelidæ*, occurring also, as shown in *Palauchenia*, in the fossil form of that family; but this rare disposition of the vertebral arteries was likewise met with in a large fossil Ungulate of South America, *Macrauchenia*, belonging to the Perissodactyle group*.

The author therefore communicates, as an appendix to his former paper on *Palauchenia*, a description, with drawings, of the mandibular dentition of *Macrauchenia patachonicha*, of the natural size, the lower jaw of that fossil animal being still a unique specimen in the British Museum. It displays the entire molar series, with the exception of the first small premolar: the several teeth in place are described in detail and compared with those of other Perissodactyles. The grinding-surface of the true molars presents the bilobed or bicrescentic type, as in *Palæotherium* and *Rhinoceros*; but *Macrauchenia* differs from both those genera in the limitation of the assumption of the molar type to the last premolar, the antecedent ones retaining the single-lobed crown. From *Palæotherium* it further differs in the last molar being bilobed, as in *Rhinoceros*, not trilobed. In *Palauchenia* all the premolars have the simpler structure, as in Artiodactyles generally. *Macrauchenia* resembles *Anoplotherium* and *Dichodon* in retaining the typical dentition, $i \frac{3-3}{3-3}$, $c \frac{1-1}{1-1}$, $p \frac{4-4}{4-4}$, $m \frac{3-3}{3-3} = 44$, and in the uninterrupted course of the dental series, not any of the teeth having a crown much higher or longer than the rest.

The paper is illustrated by drawings.

III. "Researches into the Chemical Constitution of the Opium Bases. Part I.—On the Action of Hydrochloric Acid on Morphia." By AUGUSTUS MATTHIESSEN, F.R.S., Lecturer on Chemistry in St. Bartholomew's Hospital, and C. R. A. WRIGHT, B.Sc. Received May 6, 1869.

It has been shown that when narcotine is heated with an excess of concentrated hydrochloric or hydriodic acid, one, two, or three molecules of methyl are successively eliminated, and a series of new bases homologous with narcotine obtained. It appeared interesting to see if any similar reactions took place with morphia; and for this purpose a quantity of that base, in a perfectly pure state, kindly furnished by Messrs. M'Farlane, of Edinburgh, was submitted to experiment. The purity of the substance was shown by the following analysis.

It was found that although crystallized morphia does not lose its water of crystallization in an ordinary steam drying-closet (*i. e.* slightly below 100°), yet it readily loses the whole when placed in a Liebig's drying-tube immersed in boiling water, dry air being aspirated over it.

* Odontography, 1846, p. 602.

or chloroform solution with a very small quantity of strong hydrochloric acid, the sides of the vessel become covered with crystals of the hydrochlorate of the new base. These may be drained from the mother-liquor, washed with a little cold water, in which the salt is sparingly soluble, and recrystallized from hot water and dried on bibulous paper or over sulphuric acid. No difference in the result appeared to be produced by continuing the digestion at 150° for six or twelve hours. The new base may also be formed by digesting morphia and excess of hydrochloric acid under paraffin on the water-bath for some days.

This hydrochlorate contains no water of crystallization. After drying in the water-bath, it yielded the following results on combustion with chromate of lead and oxygen:—

(I.) 0.4300 gramme gave 1.0600 carbonic acid and 0.237 water.

(II.) Sample (I.), recrystallized, and again dried in water-bath. 0.3270 gramme gave 0.8045 carbonic acid and 0.1830 water.

(III.) 0.3830 gramme, burnt with soda-lime, gave 0.1240 metallic platinum.

(IV.) 0.4720 gramme, burnt with soda-lime, and the ammonia estimated volumetrically, gave 0.0234 nitrogen.

(V.) 0.4680 gramme, precipitated by nitrate of silver and nitric acid, gave 0.2170 chloride of silver.

(VI.) 0.3410 gramme, burnt with lime, gave 0.1645 chloride of silver.

	Calculated.		Found.					
			(I.)	(II.)	(III.)	(IV.)	(V.)	(VI.)
C ₁₇	204	67.22	67.23	67.10				
H ₁₈	18	5.93	6.12	6.21				
N	14	4.61			4.60	4.95		
O ₂	32	10.54						
Cl	35.5	11.70					11.50	11.93
<hr/> C ₁₇ H ₁₇ NO ₂ HCl	<hr/> 303.5	<hr/> 100.00						

From a solution of the hydrochlorate in water, bicarbonate of sodium precipitates a snow-white non-crystalline mass, which speedily turns green on the surface by exposure to air, and is therefore difficult to obtain dry in a state of purity. The following combustion of a portion washed with water, and dried at 100° as rapidly as possible, shows that this precipitate is the base itself.

0.3310 gramme gave 0.9250 carbonic acid and 0.1830 water.

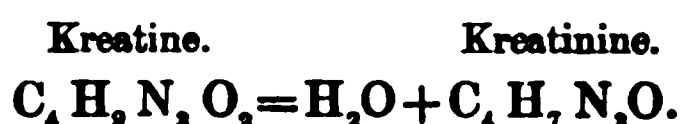
	Calculated		Found.
C ₁₇	204	76.40	76.22
H ₁₇	17	6.37	6.15
N	14	5.24	
O ₂	32	11.99	
<hr/> C ₁₇ H ₁₇ NO ₂	<hr/> 267	<hr/> 100.00	

This substance was free from chlorine, as shown by its giving no precipitate with nitrate of silver after heating with nitric acid.

It hence appears that the new base is simply formed from morphia by the abstraction of the elements of water.



the reaction under the influence of hydrochloric acid being perfectly analogous to that by which kreatine, under the influence of strong acid, splits up into water and kreatinine.



We propose to call the new base apomorphia, for reasons given subsequently.

When the hydrochlorate of apomorphia in a moist state is exposed to the air for some time, or if the dry salt is heated, it turns green, probably from oxidation, as the change of colour is accompanied by an increase of weight. The base itself, newly precipitated, is white, but it speedily turns green on exposure to air. The green mass is partly soluble in water, communicating to it a fine emerald colour—in alcohol yielding also a green, in ether and benzole giving a magnificent rose-purple, and in chloroform producing a fine violet tint.

The following Tables show the most marked properties and reactions of apomorphia as contrasted with morphia.

	Water.	Alcohol.	Ether.	Chloroform.
Morphia	Almost insoluble.	Sparingly soluble cold, more soluble boiling.	Almost insoluble.	Almost insoluble.
Apomorphia ...	Slightly soluble, especially if charged with carbonic acid.	Soluble.	Soluble.	Soluble.

The following comparative reactions were made with solutions containing each 1 per cent. of the hydrochlorate of the base :—

Reagent.....	Cautic Potash.	Ammonia.	Lime-water.	Bicarbonate of Sodium.	Strong Nitric Acid.	Neutral Ferric Chloride.
Morphia	No precipitate. Stronger solutions give a white precipitate readily soluble in excess, without undergoing decomposition.	No precipitate. Stronger solutions give a crystalline white precipitate, insoluble in excess.	No precipitate. Morphia dissolves readily in lime-water.	No precipitate. Stronger solutions yield a white unalterable precipitate slightly soluble in excess.	Yellow orange-colour, almost bleached on warming.	Greenish-blue colour. Morphia alone gives a pure blue colour.
Apomorphia	White precipitate, soluble in excess, speedily blackening.	White precipitate, soluble in excess, very speedily blackening.	White precipitate, soluble in excess, slowly darkening.	White precipitate, slightly soluble in excess, turning green.	Blood-red colour, becoming paler on warming.	Dark amethyst-colour.
Reagent.....	Bichromate of Potassium.	Bichromate of Potassium and strong Sulphuric Acid.	Nitrate of Silver.	Iodide of Potassium.	Platinic Chloride.	Mercuric Chloride, Phosphate of Sodium, Oxalate of Ammonium.
Morphia	—	—	Very slowly reduced.	No precipitate with concentrated solution.	Yellow crystalline precipitate in stronger solutions.	The morphia precipitates with these reagents are much more soluble than the corresponding apomorphia ones.
Apomorphia	Dense yellow orange precipitate, soon decomposing.	Dark-red coloration.	Quickly reduced, even in the cold.	White non-crystalline precipitate, speedily becoming green.	Yellow precipitate, decomposes on warming.	

The physiological effects of apomorphia are very different from those of morphia; a very small dose produces speedy vomiting and considerable depression, but this soon passes off, leaving no after ill effects,—facts of which we have repeatedly had disagreeable proof while working with it.

Dr. Gee is now studying these effects, and has found that $\frac{1}{10}$ of a grain of the hydrochlorate subcutaneously injected, or $\frac{1}{4}$ grain taken by the mouth, produces vomiting in from four to ten minutes. Our friend Mr. Prus allowed himself to be injected with $\frac{1}{10}$ grain, which produced vomiting in less than ten minutes. From Dr. Gee's experiments on himself and others, he concludes that the hydrochlorate is a non-irritant emetic and powerful anti-stimulant. As from these properties it appears probable that it may come into use in medicine, we have called it apomorphia, rather than morphinine, to avoid any possible mistakes in writing prescriptions.

Apomorphia is likewise formed by heating morphia and dilute sulphuric acid (1 vol. acid to 8 or 10 of water) in sealed tubes to 140° – 150° for three hours. It appears possible that the substance obtained by Arppe *, subsequently named sulphomorphide by Laurent and Gerhardt †, is an impure sulphate of apomorphia, as the formula deduced by these latter chemists from their analysis, $C_{34}H_{36}N_2O_8S$, is identical with that of this sulphate, $(C_{17}H_{17}NO_2)_2H_2SO_4$. They, however, considered it a species of amide. On repeating Arppe's experiments, we have obtained apomorphia from the product. The physical characters ascribed to sulphomorphide (of becoming green on keeping, especially on heating, of communicating this green tint to water, and of solubility in caustic alkalies, producing a brown substance by decomposition) are precisely those of the hydrochlorate of apomorphia. It appears probable that the class of analogous bodies produced from other alkaloids by similar means, such as sulphonarcotide, may possibly be the sulphates of new bases. We propose to submit these to experiment, and to prosecute our researches on the opium bases.

On sealing up codeia with hydrochloric acid and digesting it at 150° , we find some permanent gas is evolved, probably chloride of methyl, in which case the new base, if any, will be morphia or apomorphia or their isomers, as codeia differs from morphia only by CH_2 .

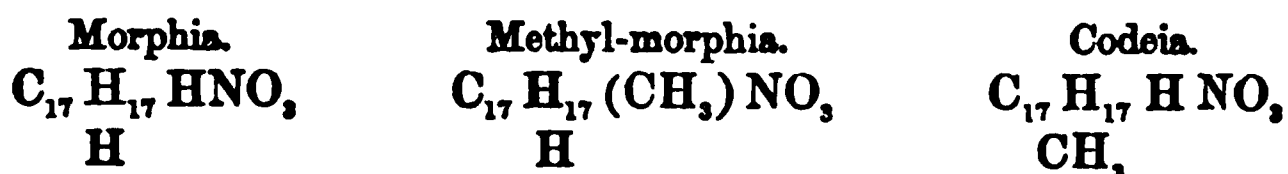
IV. "Researches into the Constitution of the Opium Bases. Part II.—On the Action of Hydrochloric Acid on Codeia." By AUGUSTUS MATTHIESSEN, F.R.S., Lecturer on Chemistry in St. Bartholomew's Hospital, and C. R. A. WRIGHT, B.Sc. Received June 2, 1869.

Codeia and morphia are, as is well known, homologous, only differing in composition by CH_2 . Both of them contain one atom of hydrogen replaceable by organic radicals, from which it appears that methyl-morphia

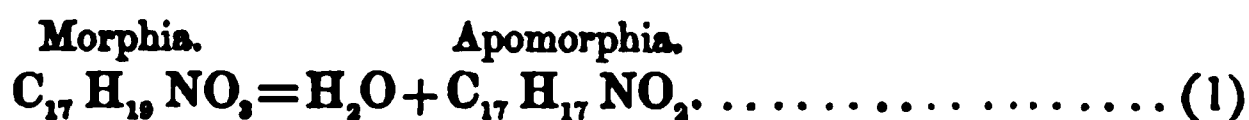
* 1845. Ann. der Chem und Pharm. vol. lv. p. 96.

† Ann. de Chimie et de Phys. [3] vol. xxiv. p. 112.

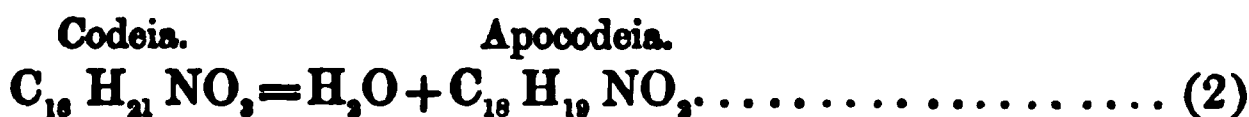
(which does not contain a replaceable atom of hydrogen) is only isomeric with codeia, and not identical. If, therefore, codeia be morphia where H is replaced by CH_3 , the atom of hydrogen so replaced must be one of those contained in one of the radicals which enter into the composition of morphia. Thus—



The action of hydrochloric acid on morphia * leads to the elimination of H_2O , and the formation of a new base, apomorphia, thus—



Codeia under the same circumstances might yield a similar base, apocodeia, thus—



Or it might behave like narcotine †, which, under the same conditions, splits up into chloride of methyl and the hydrochlorate of a new base. Thus with codeia—



but, as has already been shown, if morphia were thus produced, it would be converted, under the circumstances, into water and apomorphia, so that the whole reaction would be—



In order to examine the nature of the reaction taking place, some codeia (forming part of a supply of 10 oz. given to us by the eminent manufacturing chemists, Messrs. M'Farlane, of Edinburgh) was submitted to experiment. The substance used when examined for morphia failed to indicate the presence of the smallest trace, was wholly soluble in ether, and, on combustion with oxide of copper and oxygen, after drying at 120° , yielded the following numbers :—

0.3720 gramme gave 0.9830 carbonic acid and 0.2450 water.

	Calculated.		Found.
C_{18}	216	72.25	72.07
H_{21}	21	7.02	7.32
N	14	4.68	
O_3	48	16.05	
<hr/> $\text{C}_{18} \text{H}_{21} \text{NO}_3$	<hr/> 299	<hr/> 100.00	

Codeia was sealed up with from twelve to twenty times its weight of

* Proc. Roy. Soc. vol. xvii. p. 455.

† Proc. Roy. Soc. vol. xvii. p. 337.

strong hydrochloric acid, and heated to about 140° for two or three hours. After cooling, a layer of colourless liquid was observed floating on the top of the brown tarry contents. It immediately became gaseous on opening the tubes, and was presumedly chloride of methyl, as the issuing gases were found to be free from carbonic acid. The residue in the tubes, when dissolved in water and precipitated by carbonate of sodium, yielded, on extraction with ether and agitation with hydrochloric acid, a crystalline chloride having, when purified by recrystallization, all the properties of the chloride of apomorphia derived from morphia. It gave the same qualitative reactions, produced the same remarkable physiological effects, and yielded the following numbers on combustion with chromate of lead and oxygen :—

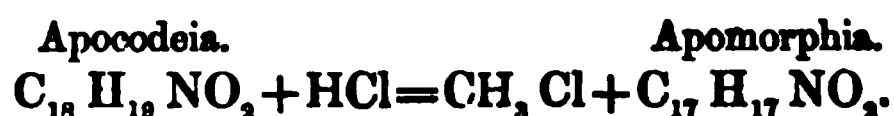
0.3120 gramme gave 0.7680 carbonic acid and 0.1740 water.

	Calculated.		Found.
C ₁₇	204	67.22	67.13
H ₁₈	18	5.93	6.19
N	14	4.61	
O ₂	32	10.54	
Cl	35.5	11.70	
<hr/> C ₁₇ H ₁₇ NO ₂ HCl		<hr/> 303.5	<hr/> 100.00

Hence the reaction which takes place is in accordance with formula (4) above, viz.



Doubtless there is an intermediate reaction, viz. either that indicated by formula (3), where morphia is the intermediate product, or that in accordance with (2), where a base homologous with apomorphia, and thence called apocodeia, is first produced, and subsequently split up into apomorphia and chloride of methyl, thus—



We are at present engaged in investigating the nature of this intermediate reaction.

V. "A Preliminary Investigation into the Laws regulating the Peaks and Hollows exhibited in the Kew Magnetic Curves for the first two years of their production." By BALFOUR STEWART, LL.D., F.R.S., Superintendent of the Kew Observatory. Received May 20, 1869.

The Kew magnetographs began to be in regular operation in May 1858, and have continued so up to the present date. The curves derived from these instruments, representing the changes which take place in the three components of the earth's magnetism at Kew, are often found to be studded with small serrated appearances, which have been denominated peaks and

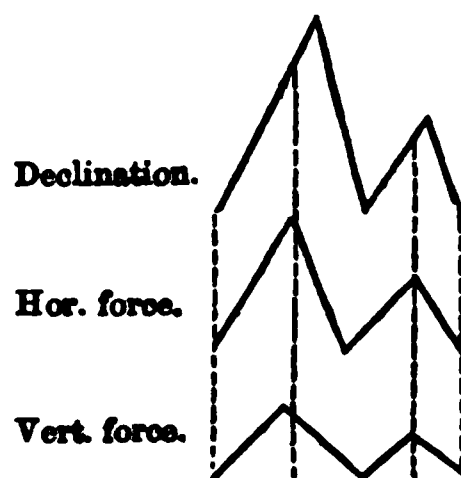
hollows; and the following remarks will serve to show that the study of these may be attended with considerable advantage.

The labours of General Sabine have been instrumental in showing that there are at least two forces concerned in producing disturbances; and this conclusion is confirmed by the appearance of the Kew curves, from which it may be seen that no disturbance of any magnitude is due to the action of a single force merely varying in amount and not in direction; for if this were the case the distance at any moment of a point in the curve of one of the elements from its normal position should bear throughout such a disturbance an invariable proportion to the distance of a corresponding point in the curve of another of the elements from its normal; but this is by no means the case.

It becomes therefore a question of interest to endeavour to find the elementary forces concerned in producing a disturbance; and it is thought that this knowledge may to some extent be attained by a study of those small and rapid changes of force which are denoted by peaks and hollows. For if several independent forces are at work, it may be thought unlikely that at the same moment a sudden change should take place in all; there is thus a probability that sudden changes of force, as exhibited in peaks and hollows, are changes in one of the elementary forces concerned, which may thus enable us to determine the nature of that force. Even if the change is not a very abrupt one, provided that we confine ourselves to such peaks and hollows as present a similar appearance for all the curves, we may suppose that we are observing changes in one only of the elementary disturbing forces; for it is unlikely that two or more independent forces, changing independently, should produce similar appearances in all of the three curves. Thus what we have to look for is similar appearances; and the precise meaning attached to this expression will be rendered clear by means of the annexed graphical representation.

We see here that (time being reckoned horizontally) we have a disturbance commencing at the same moment in each of the three elements, that for the declination being throughout three times as large, and that for the annexed force twice as large as the corresponding vertical-force disturbance.

In a paper communicated by me to the Royal Society, and published in the Transactions (1862, page 621), it was stated that, as a rule, small and abrupt disturbances at Kew tend either to increase at the same moment both components of magnetic force and the westerly declination, or to decrease these elements, as the case may be. As in the Kew curves of 1862, increasing ordinates represented decreasing horizontal force, decreasing vertical force, and decreasing declination, the above statement is the same as saying that, as a rule, peaks and hollows in one element correspond to peaks and hollows in the other two.



Nevertheless one notable exception to this rule was mentioned in the above paper, namely, that at the beginning of the great disturbance of August–September 1859, an abrupt fall of the declination curve corresponded to a rise of the other two components. It was also shown in this paper that while the horizontal-force peaks are always as nearly as possible double in size of the vertical-force peaks, the proportion between the declination peaks and those of the other components appeared to be variable. Some light was thrown upon this variability in a subsequent paper by Senhor Capello and myself, in which the peaks and hollows at Lisbon and at Kew were compared together (Proc. Roy. Soc. 1864, p. 111). It was found that these phenomena occurred simultaneously at these two observatories; and it was stated that, as far as Kew is concerned, the proportion of the declination peaks and hollows to those of the horizontal and vertical force presents the appearance of a daily range, being great at the early morning hours and small in those of the afternoon.

Thus the type of small and abrupt changes, judging from the behaviour of the declination, seemed to vary from two causes, being in the first place subject to a diurnal variation, and in the second place appearing to vary with the disturbance, inasmuch as that for the great disturbance August–September 1859 was, as above stated, entirely different from the usual type.

This complexity seems puzzling; but the results of a preliminary comparison between the Stonyhurst and Kew declination magnetographs (Sidgreaves and Stewart, Proc. Roy. Soc. 1869, p. 236) appear to throw some light upon its cause. It was there stated that when the declination-curves of Stonyhurst and Kew are compared together during rather slow disturbances, the scales are such that the traces seem exactly to coincide even to their most minute features; but, on the other hand, when the disturbance is abrupt, there is an excess of Stonyhurst over Kew, which appears to vary with the abruptness of the disturbance, being great when this is great. In fine, there appears to be superimposed upon a disturbance, which is mainly cosmical, a comparatively small effect, which appears to be of a more local nature, and may perhaps be caused by earth-currents. This circumstance renders it prudent, in discussing the laws of the small and abrupt changes of force (peaks and hollows) at Kew, to avoid all great and excessively abrupt disturbances, confining ourselves to those cases in which there is only a moderate abruptness. The result obtained for the great disturbance August–September 1859 may therefore be dismissed as probably effected by this local cause, inasmuch as the disturbance measured was very abrupt. The question then arises—Rejecting very great and abrupt disturbances, has the peak-and-hollow force only a regular diurnal variation, or is it subject besides to other changes of type?

Mr. Whipple, magnetical assistant at Kew, has carefully selected and measured all the similar peaks and hollows for the first two years of the Kew curves; and the result exhibits a manifest diurnal variation in the type of the peak-and-hollow force.

In the following Table we have these various measurements ranged in order of date, the unit of the scale adopted being $\frac{1}{200}$ of an inch.

TABLE I.
Measurements of the Peaks and Hollows in the Kew Magnetograph Curves.

Date.	Time.	Declination.	Horizontal force.	Vertical force.	Date.	Time.	Declination.	Horizontal force.	Vertical force.
1858.	h m				1858.	h m			
May 22.	15 15	30	21	10	Dec. 13.	21 10	24	16	8
22.	18 30	35	15	7	16.	17 15	39	18	10
29.	16 15	49	28	16	16.	23 0	17	13	9
June 3.	16 10	22	9	5	17.	15 12	55	45	20
3.	17 25	45	19	11	18.	20 6	64	58	25
4.	16 15	42	23	11	23.	19 18	23	14	6
4.	19 10	24	11	6	1859.				
July 6.	18 17	23	11	5	Jan. 8.	23 45	42	32	14
23.	15 10	20	13	6	10.	20 0	103	82	32
24.	17 10	35	19	6	17.	15 40	19	9	4
26.	13 55	52	51	25	17.	21 45	23	15	7
30.	17 55	20	13	7	18.	23 15	36	22	12
Aug. 1.	2 0	17	23	14	19.	18 20	17	13	5
10.	2 16	14	11	5	19.	20 10	47	34	12
12.	4 20	15	18	10	29.	14 55	23	14	7
13.	17 30	24	17	8	30.	14 10	9	7	4
13.	18 43	19	12	5	30.	21 48	21	17	9
17.	12 54	14	18	9	Feb. 1.	20 50	54	36	14
24.	18 14	19	11	5	10.	2 27	55	41	19
26.	1	16	25	11	10.	15 50	20	13	7
28.	20 20	33	23	10	14.	18 0	32	21	7
Sept. 2.	18 0	20	10	5	15.	23 30	21	15	7
11.	16 2	11	7	4	16.	19 45	18	11	5
13.	21 47	16	12	4	21.	2 50	37	40	17
23.	10 50	8	19	6	22.	14 50	35	41	17
23.	11 55	9	15	7	22.	16 10	82	55	24
23.	12 56	10	18	6	March 2.	21 30	25	16	7
25.	23 10	40	39	20	3.	21 40	21	15	5
26.	0 30	19	18	10	9.	18 40	27	14	8
26.	3 20	32	38	23	10.	20 40	42	22	10
29.	19 35	21	13	4	10.	22 10	25	21	10
29.	20 10	16	11	6	11.	0 55	18	18	8
Oct. 2.	18 10	15	9	5	11.	1 30	17	18	8
8.	20 20	28	22	10	11.	16 50	63	40	17
15.	1 22	14	14	5	11.	18 30	19	10	4
15.	3 48	10	15	4	12.	22 10	21	13	6
18.	23 52	108	70	31	13.	18 45	18	9	4
21.	19 27	27	17	7	16.	17 50	15	10	5
24.	18 56	28	16	7	16.	20 50	54	42	18
27.	22 40	43	40	19	16.	22 35	93	83	40
27.	23 7	44	38	27	17.	20 30	45	28	13
28.	19 20	12	7	5	22.	5 30	11	12	7
29.	14 0	28	32	15	26.	19 30	28	20	10
Nov. 2.	12 50	20	31	10	27.	23 40	18	15	6
2.	22 50	26	18	7	29.	20 10	61	38	22
12.	3 20	43	47	19	30.	20 0	59	38	18
19.	21 17	30	20	7	31.	19 30	101	53	26
23.	1 28	32	27	12	April 6.	18 55	25	16	8
24.	0 50	20	19	11	7.	23 4	23	21	8

TABLE I. (continued).

Date.	Time.	Declination.	Horizontal force.	Vertical force.	Date.	Time.	Declination.	Horizontal force.	Vertical force.
1859.	h m				1860.	h m			
April 11.	17 55	36	26	11	Jan. 10.	22 5	29	17	9
12.	16 20	13	9	4	16.	17 50	34	18	9
12.	16 50	12	13	4	20.	20 45	24	18	9
12.	18 50	29	17	7	21.	20 35	50	29	14
13.	17 30	22	16	8	26.	23 15	21	16	7
13.	18 10	42	20	10	27.	17 50	21	9	4
13.	20 30	34	21	10	28.	15 20	13	5	2
13.	22 45	25	23	13	28.	21 40	21	10	5
13.	23 25	22	21	10	29.	21 35	41	22	9
14.	13 50	14	13	7	Feb. 12.	15 55	12	8	4
14.	17 40	22	15	7	15.	21 10	31	21	9
14.	18 5	34	15	7	16.	11 20	13	23	12
15.	17 25	20	14	5	16.	16 10	21	11	5
16.	18 50	25	12	5	Mar. 5.	18 25	25	13	5
17.	2 10	35	49	27	7.	15 10	22	37	19
20.	8 0	8	11	5	7.	19 30	50	22	11
20.	20 33	22	11	6	8.	21 5	36	17	9
May 4.	19 0	24	15	7	9.	3 50	13	15	8
5.	16 25	24	13	5	9.	19 2	69	32	10
6.	17 55	17	7	4	12.	20 10	129	59	27
8.	17 30	43	23	10	12.	21 10	34	23	11
9.	16 20	27	19	6	14.	18 5	112	64	29
Aug. 4.	17 15	13	7	3	14.	19 15	41	19	10
7.	1 35	14	24	12	14.	21 45	28	23	9
9.	16 30	19	13	5	15.	19 40	71	34	13
10.	2 50	13	18	10	19.	18 40	35	19	9
10.	7 45	49	39	24	19.	19 0	56	26	13
22.	17 58	20	8	4	20.	1 20	24	16	8
Sept. 15.	18 10	27	11	5	20.	2 55	17	20	10
15.	21 55	25	20	9	22.	19 45	19	10	5
17.	23 10	15	16	8	22.	20 0	39	29	13
26.	4 10	44	56	37	April 2.	0 55	21	21	10
30.	18 55	31	21	11	2.	2 45	15	17	7
Oct. 1.	0 10	21	25	11	2.	3 30	16	21	10
2.	16 20	59	34	17	3.	4 15	15	22	11
21.	18 50	38	23	11	4.	18 15	22	17	5
Nov. 2.	18 30	23	12	6	11.	17 40	17	13	(6)
6.	4 20	11	14	7	11.	20 45	15	9	(4)
6.	19 5	21	9	5	11.	23 10	29	35	(17)
12.	3 58	29	23	14	17.	3 35	15	20	9
16.	17 40	27	15	7	19.	22 5	20	9	4
17.	15 20	24	23	10	20.	2 20	32	33	18
17.	16 50	21	15	8	21.	16 40	15	10	5
Dec. 4.	20 50	33	18	7	29.	14 5	19	21	10
4.	21 20	30	21	10	29.	20 30	41	38	19
5.	20 10	38	18	8	30.	0 50	30	20	10
6.	2 25	17	19	9	30.	2 30	18	16	8
6.	17 5	63	41	20	30.	21 10	111	98	43
10.	18 10	97	55	28	May 1.	1 10	13	18	9
15.	22 10	37	27	14	1.	17 50	17	4	2
1860.					4.	11 10	8	17	4
Jan. 5.	20 40	21	15	7	4.	15 10	19	10	5
10.	19 10	30	20	9	27.	19 5	22	12	5

In the following Table each disturbance is entered under its appropriate hour.

59.

[To face page 466.

[illegible]

59 60.

	14-15.	15-16.	22-23.	23-0.
1. VI	D. HF. VF.	D. HF. VF.	1 D. HF. VF.	D. HF. VF.
2.	19 21 10	24 23 10	1 37 27 14	15 16 8
3.		13 5 2	5 29 17 9	21 16 7
4.		12 8 4	4 20 9 4	29 35 (17)
5.		22 37 19	2	
6.	..	19 10 5	1	..
7.		..		
8.		..		
9.		..		
10.		..		
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99.		..		
100.		..		

It will be seen from this Table that there is great constancy in the type of the peak-and-hollow force for the same hour. Bearing in mind the difficulty of finding exactly similar appearances denoting an unmixed force, and remembering also the small size of many of the peaks and hollows observed, it is not too much to say that, as far as these two years' observations are concerned, there is no trace of anything else than a diurnal change in the type of the peak-and-hollow force. But this question cannot be finally decided until more observations are discussed.

In the following Table the final results of Table II. are brought before the eye in a condensed form.

TABLE III.

Hourly Ratios and Frequency of the Peaks and Hollows, the vertical force being taken as unity.

Hour.	Declination.		Horizontal force.		Mean of both years.		Number of observations.
	1858-59.	1859-60.	1858-59.	1859-60.	Declination.	Horizontal force.	
0-1	1'97	2'32	2'00	2'13	2'14	2'06	7
1-2	2'19	1'76	2'33	2'00	1'97	2'16	7
2-3	1'92	1'81	2'00	1'98	1'86	1'99	11
3-4	1'84	1'78	2'17	1'93	1'81	2'05	7
4-5	1'50	1'27	1'80	1'67	1'38	1'73	4
5-6	1'57	1'71				
6-7							
7-8	1'60	2'04	2'20	1'62			
8-9	1'60	2'20				
9-10							
10-11	1'33	3'16				
11-12	1'29	1'31	3'14	2'50			
12-13	1'76	2'68				
13-14	2'00	3'04				
14-15	2'21	1'90	2'18	2'10	2'10	2'14	5
15-16	3'06	2'25	2'15	2'07	2'65	2'11	10
16-17	3'59	3'37	2'25	2'07	3'48	2'16	15
17-18	3'75	3'85	2'19	2'09	3'80	2'14	22
18-19	4'06	3'82	2'22	2'14	3'94	2'18	28
19-20	3'49	4'45	2'23	2'27	3'97	2'25	21
20-21	3'21	3'61	2'26	2'16	3'41	2'21	23
21-22	3'40	3'13	2'36	2'24	3'26	2'30	16
22-23	2'40	3'19	2'03	1'96	2'79	2'00	10
23-0	2'58	2'03	1'99	2'10	2'30	2'04	13

From this Table it will be seen that, as was formerly stated, the ratio between simultaneous peaks and hollows of the two components of the force is very nearly constant, the horizontal force disturbance being very nearly double of that of the vertical force.

It will also be seen that there is a very marked diurnal range in the ratio which the declination peak or hollow bears to that of the vertical force, this ratio being greatest about 7 A.M. About this hour we have also most peaks and hollows, while in the evening and very early morning

hours there is a comparative absence of these phenomena. So much is this the case that for the two years investigated I have not succeeded in finding a single example of a peak or hollow, suitable for this research, between the hours of 6 and 7 P.M., or between those of 9 and 10 P.M.

I forbear to make further remarks on this subject, but hope in a short time to extend the investigation up to the present date, and to bring the results before this Society.

VI. "On a new Astronomical Clock, and a Pendulum Governor for Uniform Motion." By Sir WILLIAM THOMSON, LL.D., F.R.S.
Received June 10, 1869.

It seems strange that the dead-beat escapement should still hold its place in the astronomical clock, when its geometrical transformation, the cylinder escapement of the same inventor, Graham, only survives in Geneva watches of the cheaper class. For better portable time-keepers, it has been altered (through the rack-and-pinion movement) into the detached lever, which has proved much more accurate. If it is possible to make astronomical clocks go better than at present by merely giving them a better escapement, it is quite certain that one on the same principle as the detached lever, or as the ship-chronometer escapement, would improve their time-keeping.

But the inaccuracies hitherto tolerated in astronomical clocks may be due more to the faultiness of the mercury compensation pendulum, and of the mode in which it is hung, and of the instability of the supporting clock-case or framework, than to imperfection of the escapement and the greatness of the arc of vibration which it requires; therefore it would be wrong to expect confidently much improvement in the time-keeping merely from improvement of the escapement. I have therefore endeavoured to improve both the compensation for change of temperature in the pendulum, and the mode of its support, in a clock which I have recently made with an escapement on a new principle, in which the simplicity of the dead-beat escapement of Graham is retained, while its great defect, the stopping of the whole train of wheels by pressure of a tooth upon a surface moving with the pendulum, is remedied.

Imagine the escapement-wheel of a common dead-beat clock to be mounted on a collar fitting easily upon a shaft, instead of being rigidly attached to it. Let friction be properly applied between the shaft and the collar, so that the wheel shall be carried round by the shaft unless resisted by a force exceeding some small definite amount, and let a governor giving uniform motion be applied to the train of wheel-work connected with this shaft, and so adjusted that, when the escapement-wheel is unresisted, it will move faster by a small percentage than it ought to move when the clock is keeping time properly. Now let the escapement-wheel, thus mounted and carried round, act upon the escapement, just as it does in the ordinary clock. It will keep the pendulum vibrating, and will, just as in the ordinary

clock, be held back every time it touches the escapement during the interval required to set it right again from having gone too fast during the preceding interval of motion. But in the ordinary clock the interval of rest is considerable, generally greater than the interval of motion. In the new clock it is equal to a small fraction of the interval of motion: $\frac{1}{300}$ in the clock as now working, but to be reduced probably to something much smaller yet. The simplest appliance to count the turns of this escapement-wheel (a worm, for instance, working upon a wheel with thirty teeth, carrying a hand round, which will correspond to the seconds' hand of the clock) completes the instrument; for minute and hour-hands are a superfluity in an astronomical clock.

In various trials which I have made since the year 1865, when this plan of escapement first occurred to me, I have used several different forms, all answering to the preceding description, although differing widely in their geometrical and mechanical characters. In all of them the escapement-wheel is reduced to a single tooth or arm, to diminish as much as possible the moment of inertia of the mass stopped by the pendulum. This arm revolves in the period of the pendulum (two seconds for a one second's pendulum), or some multiple of it. Thus the pendulum may execute one or more complete periods of vibration without being touched by the escapement.

I look forward to carrying the principle of the governed motion for the escapement-shaft much further than hitherto, and adjusting it to gain only about $\frac{1}{100}$ per cent. on the pendulum; and then I shall probably arrange that each pallet of the escapement be touched only once a minute (and the counter may be dispensed with). The only other point of detail which I need mention at present is that the pallets have been, in all my trials, attached to the bottom of the pendulum, projecting below it, in order that satisfactory action with a very small arc of vibration (not more on each side than $\frac{1}{100}$ of the radius, or 1 centimetre for the seconds' pendulum) may be secured.

My trials were rendered practically abortive from 1865 until a few months ago by the difficulty of obtaining a satisfactory governor for the uniform motion of the escapement-shaft; this difficulty is quite overcome in the pendulum governor, which I now proceed to describe.

Imagine a pendulum with single-tooth escapement mounted on a collar loose on the escapement-shaft just as described above—the shaft, however, being vertical in this case. A square-threaded screw is cut on the upper quarter of the length of the shaft, this being the part of it on which the collar works, and a pin fixed to the collar projects inwards to the furrow of the screw, so that, if the collar is turned relatively to the shaft, it will be carried along, as the nut of a screw, but with less friction than an ordinary nut. The main escapement-shaft just described is mounted vertically. The lower screw and long nut collar, three-quarters of the length of the escapement-shaft, are surrounded by a tube which, by wheel-work, is carried round about five per

cent. faster than the central shaft. This outer shaft, by means of friction produced by the pressure of proper springs, carries the nut collar round along with it, except when the escapement-tooth is stopped by either of the pallets attached to the pendulum. A stiff cross piece (like the head of a T), projecting each way from the top of the tubular shaft, carries, hanging down from it, the governing masses of a centrifugal friction governor. These masses are drawn towards the axis by springs, the inner ends of which are acted on by the nut collar, so that the higher or the lower the latter is in its range, the springs pull the masses inwards with less or more force. A fixed metal ring coaxial with the main shaft holds the governing masses in when their centrifugal forces exceed the forces of the springs, and resists the motion by forces of friction increasing approximately in simple proportion to the excess of the speed above that which just balances the forces of the springs. As long as the escapement-tooth is unresisted, the nut collar is carried round with the quicker motion of the outer tubular shaft, and so it *screws upwards*, diminishing the force of the springs. Once every semiperiod of the pendulum it is held back by either pallet, and the nut collar screws *down* as much as it rose during the preceding interval of freedom when the action is regular; and the central or main escapement-shaft turns in the same period as the tooth, being the period of the pendulum. If through increase or diminution of the driving-power, or diminution or increase of the coefficient of friction between the governing masses and the ring on which they press, the shaft tends to turn faster or slower, the nut collar works its way down or up the screw, until the governor is again regulated, and gives the same speed in the altered circumstances. It is easy to arrange that a large amount of regulating power shall be implied in a single turn of the nut collar relatively to the central shaft, and yet that the periodic application and removal of about $\frac{1}{80}$ of this amount in the half period of the pendulum shall cause but a *very small* periodic variation in the speed. The latter important condition is secured by the great moment of inertia of the governing masses themselves round the main shaft. I hope, after a few months' trial, to be able to present a satisfactory report of the performance of the clock now completed according to the principles explained above. As many of the details of execution may become modified after practical trial, it is unnecessary that I should describe them minutely at present. Its general appearance, and the arrangement of its characteristic parts, may be understood from the photograph now laid before the Society.

VII. "On the Effect of Changes of Temperature on the Specific Inductive Capacity of Dielectrics." By Sir W. THOMSON, LL.D., F.R.S.

[The publication of the text of this paper is postponed.]

June 17, 1869.

Lieut.-General SABINE, President, in the Chair.

Mr. J. Ball, Mr. J. N. Lockyer, and Vice-Admiral Sir Spencer Robinson were admitted into the Society.

The following communications were read :—

- I. "Note on Professor Sylvester's representation of the Motion of a free rigid Body by that of a material Ellipsoid rolling on a rough Plane." By the Rev. N. M. FERRERS, Fellow and Tutor of Caius College, Cambridge. Communicated by Professor J. J. SYLVESTER. Received May 29, 1869.

(Abstract.)

This paper is intended as a sequel to Professor Sylvester's paper above mentioned, which was published in the Philosophical Transactions for 1866. The notation, so far it differs from Professor Sylvester's, is as follows :—

p is the distance from the centre of the ellipsoid to the rough plane.

λ the (constant) component angular velocity of the ellipsoid about the diameter normal to the rough plane. μ the component angular velocity of the ellipsoid about the diameter parallel to the projection of the instantaneous axis on the rough plane.

h_λ , h_p are the component angular momenta about these diameters respectively.

h , about the diameter at right angles to both.

n the angular velocity, in space, of the plane through the instantaneous axis perpendicular to the rough plane.

Then the mass of the ellipsoid being taken, as in Professor Sylvester's paper, to be unity, it is proved that

$$h_\lambda = (a^2 + b^2 + c^2 - p^2)\lambda - \frac{p^2}{\lambda} \mu^2.$$

The following theorem is then established :—"The component angular momentum of the ellipsoid about any diameter parallel to the rough plane is equal to p , multiplied into the component velocity of the point of contact of the ellipsoid and rough plane, in the direction at right angles to this diameter."

It hence follows that

$$h_t = \frac{p^2}{\lambda} \frac{d\mu}{dt}, \quad h_p = \frac{p^2}{\lambda} n\mu,$$

whence it is proved that

$$F = -2p \frac{d\mu}{dt} = -\frac{2\lambda}{p} h_t,$$

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and that

$$P = p \left(\frac{1}{\mu} \frac{d^2 \mu}{dt^2} - n^2 \right).$$

These results are then reduced into the following form :—

$$P = p \left\{ -2\mu^2 + (1 - \beta\gamma - \gamma\alpha - \alpha\beta)\lambda^2 + 2\alpha\beta\gamma \frac{\lambda^4}{\mu^2} - \alpha^2\beta^2\gamma^2 \frac{\lambda^6}{\mu^4} \right\},$$

$$F = -\frac{2p}{\mu} \left\{ -(\mu^2 + \beta\gamma\lambda^2)(\mu^2 + \gamma\alpha\lambda^2)(\mu^2 + \alpha\beta\lambda^2) \right\}^{\frac{1}{2}},$$

where α, β, γ are written for $1 - \frac{a^2}{p^2}, 1 - \frac{b^2}{p^2}, 1 - \frac{c^2}{p^2}$ respectively.

In the last clause of the paper it is pointed out that Poinso't's "rolling and sliding cone" is a particular case of Professor Sylvester's "correlated and contrarelated bodies."

II. "On the Origin of a Cyclone." By HENRY F. BLANFORD, F.G.S., Meteorological Reporter to the Government of Bengal. Communicated by Dr. T. THOMSON. Received May 21, 1869.

It has long been an object to the completion of our knowledge of vortical storms to trace out their early history, and to show, by the comparison of a sufficient number of local observations, by what wind-currents the vortex is generated in each storm-region, and by what agency these currents are directed to the spot at which the storm originates.

With this object in view, I endeavoured, immediately after the great Calcutta storm of the 1st of November 1867, to obtain, through the assistance of Captain Howe (then officiating as Master Attendant of the Port), the logs of as many ships as possible that had been in the Bay of Bengal or anywhere to the north of the Equator during any part of the last week of October. A similar application was made to the Meteorological Department of the Board of Trade and readily granted. The meteorological stations recently established in Bengal, and the observatories of Calcutta and Madras, contributed a number of observations, for the most part fairly trustworthy; and I was thus placed in possession of data which, although far from sufficient to the complete solution of the problem for the storm in question, have at least enabled me to elucidate its origin to a greater extent than has been accomplished, as far as I am aware, for any previous storm in these seas or elsewhere.

The following Tables give the noon barometric pressures at several stations in Bengal* and on the shores of the bay, and those of a few ships

* These are calculated from the observations at 10 A.M. and 4 P.M., but so regular is

from the 23rd to the 27th of October ; also the temperatures and humidities of the atmosphere at land stations at the hours of observation, and the prevalent wind-directions for the same period. The barometric readings are throughout the paper reduced for temperature and sea-level, and, with one exception* (noticed below), the instruments have been compared and corrected to the Calcutta standard.

Noon Barometric pressures.

	23rd.	24th.	25th.	26th.	27th.
Patna	29·912 in.	?	30·014 in.	29·939 in.	29·926 in.
Calcutta.....	·913	29·967 in.	29·983	·940	·965
Dacca	·963	·999	30·005	·943	·965
Chittagong.....	·965	·972	·008	·932	·953
False Point	·901	·940	29·939	·908	·928
Akyab	·910	·933	·939	·900	·901
Madras	·897	·926	·949	·993	·930
‘Prince Arthur,’ S.S.	lat. S.S. long. bar.	Between Calcutta and Port Blair.			Port Blair.
‘Winchester’ ...	lat. 15° 34' N. long. 89° 43' E. bar. 29·894 in.	17° 50' N. 89° 14' E. 29·834 in.	18° 36' N. 89° 8' E. 29·966 in.	19° 6' N. 88° 46' E. 29·931 in.	20° 10' N. ? 29·972 in.
‘St. Marnock’ ...	lat. 8° 12' N. long. 88° 54' E. bar. 29·833 in.	9° 53' N. 88° 49' E. 29·901 in.	12° 17' N. 88° 55' E. 29·903 in.	14° 3' N. 88° 31' E. 29·936 in.	14° 47' N. 88° 36' E. 29·896 in.
‘J. C. Botelbhoë.’	lat. 5° 47' N. long. 91° 20' E. bar.† 29·770 in.	7° 44' N. 91° 30' E. 29·772 in.	8° 20' N. 92° E. 29·777 in.	10° 16' N. 91° 24' E. 29·740 in.	11° 21' N. 91° 38' E. 29·750 in.
‘Mongolia,’ S.S.	lat. Galle. long. harbour. bar. 29·928 in.	6° 27' N. 81° 25' E. 29·904 in.	10° 21' N. 81° 32' E. 29·946 in.	Madras. Roads. 29·918 in.	15° 43' N. 82° 51' E. 29·986 in.
‘Gauntlet’	lat. long..... bar.	2° 14' S. 89° 47' E. 29·804 in.	0° 29' S. ? 29·828 in.	3° 44' N. 92° 25' E. 29·707 in.	7° 5' N. 92° 6' E. 29·562 in.
‘Astracan’	lat. long..... bar.	4° 56' S. 89° 27' E. 29·886 in.	2° 59' S. 89° 24' E. 29·816 in.	0° 8' S. 89° 56' E. 29·786 in.
‘Iron King’	lat. long..... bar.	7° 0' S. 87° 4' E. 29·834 in.	4° 31' S. 87° 30' E. 29·842 in.

the march of the barometer that they may safely be accepted as within ·02 of the truth.

* The Madras barometer should be included in this remark, but being an excellent standard instrument, it may fairly be assumed that its difference, if any, is insignificant.

† The barometer of this ship has not been compared with the standard, but from a comparison of its reading at a later date, at the Sandheads, with that of Saugor Island, it may be inferred that its error is small, not exceeding ·05 inch, and probably less. For the purpose of comparison, I have added ·04 to the reported readings.

Observed Temperatures.

	23rd.		24th.		25th.		26th.		27th.	
	10 ^h .	4 ^h .	10 ^h .	4 ^h .	10 ^h .	4 ^h .	10 ^h .	4 ^h .	10 ^h .	4 ^h .
Patna	80°	83°	80°	83°	84°	90°	84°	87°	81°	86°
Calcutta	85	87	83	85	83	80	82	84	81	85
Dacca	81	86	83	83	82	83	82	82	81	81
Chittagong	84	82	83	85	83	86	83	85	81	84
False Point	85	85	85	85	85	86	84	84	84	84
Akyab	83	87	84	87	84	87	83	86	83	85
Madras	84	85	86	84	84	84	83	83	83	84

Humidities.

Saturation = 100.

	23rd.		24th.		25th.		26th.		27th.	
	10 ^h .	4 ^h .	10 ^h .	4 ^h .	10 ^h .	4 ^h .	10 ^h .	4 ^h .	10 ^h .	4 ^h .
Patna	78	68	70	63	67	50	54	42	55	35
Calcutta	77	70	78	77	85	86	80	67	68	64
Dacca	91	91	87	87	91	87	91	91	86	81
Chittagong	93	85	83	89	86	85	86	83	84	87
False Point	79	75	79	79	79	79	79	75	71	71
Akyab	87	79	79	76	87	72	83	75	83	71
Madras	75	71	72	71	75	71	71	75	71	64

[See Table, Prevalent Winds, p. 475.]

A comparison of the above data* shows as follows:—

On the 23rd of October the barometric pressure was about 0·005 higher at Chittagong and Dacca than elsewhere around or on the bay. From Calcutta to Galle, at Akyab over the northern and down the western part of the bay, it was nearly uniform, being slightly lower at Madras; but to the west of Acheen and the Nicobars it was from 0·15 to 0·2 inch lower than around the coasts of India and Arakan. In Bengal and down the west coast of the bay the winds were light from between S. and E., and the same was the case over the bay down to the latitude of the Nicobars. In lat. 4° to 6° 30' N., in the region of barometric depression, the 'J. C. Botelbhoë' experienced rain and cloudy weather, with a moderate breeze from W.N.W. during the latter part of the day, and the 'St. Marnock' about 2° further north had similar weather and a heavy sea from W.S.W., but the breeze was light from E. and S.E. It appears from the log of the last-named vessel, and that of the 'Léonie'†, that from the Equator up to lat. 5°, W.N.W. winds with rain and squally weather had prevailed for many days previously (at least, from the 11th of October); and this current,

* Some additional data are given from ships' logs, &c.

† Of which I have received only a cursory abstract.

	23rd.	24th.	25th.	26th.	27th.
Patna.....	E. S.W. to S.E.	E. N.W. to N.E.	E. N.N.E. to E.N.E.	E. & N.W. N. & N.N.W.	N.W. & W. N.N.W. to N. by N.E.
Calcutta.....	S. & S.W. & N. S.S.W.	N. S.S.W.	N. N.	N.W. & N. N.W.	N.W. & N. N.W.
Hazareebaugh	Var. E.S.E. & N.N.E.	N. N. to W.	N. N. to W.S.W.	N. & E.S.E. N. to W.	N. & N.W. N. to W.
Berhampore	S.E. Regular	Var. land and	N. to N.E. sea	N.W. to N.E. breezes.	N.N.E. & N.E. Southerly.
Dacca.....	E. S.E.	N.E. E. & S.E.	E. by N.E. Easterly.	E.S.E. to N.E. Var.	N. & N.N.E. N.E. to E.
Chittagong	lat. long. wind	Between Calcutta and Port Blair.	Port Blair.	Port Blair.
False Point	lat. long. wind	E.N.E. & W.N.W.	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
Akyab	lat. long. wind	17° 50' N. 89° 14' E.	18° 36' N. 89° 8' E.	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
Madras	lat. long. wind	E.N.E. to N.E. 9° 53' N.	E.N.E. 12° 17' N.	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
Cuttack	lat. long. wind	88° 49' E. E.N.E. to E.	88° 55' E. N.E. to E.N.E.	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'Prince Arthur'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'Winchester'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'St. Marnock'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'Timoor Shah'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'Comorin'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'J. C. Botelboe'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'Mongolia'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'Gaundlet'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'Astracan'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.
'Iron King'	lat. long. wind	E.S.E. 19° 6' N.	E.N.E. to S.E. 20° 10' N.

as appears from the accounts furnished by other vessels, eventually contributed in a great degree to produce the cyclone, being diverted from its previous direction towards the place of low barometer in the south of the bay *.

On the 24th and 25th the barometric pressure increased slightly over the north and west of the bay, but chiefly at Patna. The increase was from 0·04 to 0·07 inch at stations in Lower Bengal, 0·05 at Madras, and 0·03 at Akyab. The difference of pressures at Chittagong and Madras amounted on the latter day to about 0·06 inch, and between Chittagong and Akyab to about 0·05 inch. In the region to the west of the Nicobars the pressure seems to have remained much the same as on the 23rd, and was about 0·23 inch less than at Chittagong, and 0·19 less than at Madras. At the same time, in lat. 5° S., the pressure was about 0·1 inch higher than in the south of the bay. The figures given in the Table thus indicate a region of slight but distinct barometric depression running from Sumatra up towards Arakan, with a minimum to the west of the Nicobars, or, more probably, somewhat further to the south.

Meanwhile the northerly or north-easterly wind, which was first felt at Chittagong on the afternoon of the 23rd, extended over Lower Bengal and down the western half of the bay as far as the northern extremity of Ceylon. It prevailed also over the northern part of the bay from N.E. and E.N.E. as far down as lat. 12°, and was accompanied with fine and clear weather. Below this latitude to the west and north-west of the Nicobars the winds were light and variable, with rain and squally weather ('Léonie,' 'J. C. Botelbhoe'). Still further south, on the Equator ('Gauntlet'), and probably for some degrees to the north, the W.N.W. winds, already noticed, prevailed with squally weather, while the S.E. trade was blowing ('Gauntlet,' 'Astracan') up to 2° 30' or 3° south latitude.

On the 26th there was a general fall of the barometer, greatest in Bengal, and the pressure became nearly equal over Bengal, down the west coast to Madras and over the bay ('Winchester,' 'St. Marnock') down to lat. 14°. In the eastern part of the bay, between lats. 14° and 10°, there was a barometric dip of 0·17 inch ('St. Marnock,' 'J. C. Botelbhoe'), and the barometer stood at about the same height in N. lats. 10° and 4° ('J. C. Botelbhoe,' 'Gauntlet'). The area of maximum depression lay evidently between these latitudes, since in S. lat. 3° the pressure was 0·1 inch and in S. lat. 7° 0·13 inch higher ('Astracan,' 'Iron King').

The state of the wind and weather appears to have been much the same as on the previous day; but there is some evidence in the logs of the 'J. C.

* This W.N.W. current is very prevalent in the winter months, as is well known to mariners. Its prevalence is clearly shown in the Board of Trade charts, and it is especially noticed by Maury (Phys. Geog. of the Sea, 12th edit. p. 375) as the winter or westerly monsoon of the line. It is usually accompanied by rain and squally weather, and not improbably plays an important part in the production of all the cyclones that originate in the south of the bay to the west of the Andamans and Nicobars. On this point compare the Report on the Calcutta cyclone of 1864, especially pp. 79, 85, 105. See *infra*.

Botelbhoë,' 'Timoor Shah,' and 'Gauntlet' that the wind was beginning to circulate around the area of greatest depression and of variable winds. The first of these ships, whose course was northward and on the east of the area in question, had a moderate breeze veering from N.E. to E. with heavy squalls; the second, moving slowly up on the west of the area, had northerly freshening breezes and an overcast sky with rain and a heavy S.W. swell. The 'Gauntlet,' at 200 to 250 miles to the south, had the wind at W.N.W. with hard rain-squalls. There is, however, no evidence of the wind having attained to anything like hurricane violence until the following day.

On the 27th the barometric pressure remained much the same as on the previous day over the greater part of our area. The barometric difference between Chittagong and Akyab amounted to about 0·05 inch, and to an equal amount between the latter place and Port Blair. At 70 miles to the west of the Andamans the barometer of the 'J. C. Botelbhoë' stood 0·1 inch lower; but the lowest pressure recorded on this day was experienced by the 'Gauntlet' in lat. $7^{\circ} 5'$ at about 100 miles due west of the Nicobars. The reading of this ship's barometer was 29·562, or 0·29 less than at Port Blair, and 0·22 less than on the Equator to the south, 0·4 less than at Calcutta and Dacca, and nearly 0·36 less than at Madras. There can be little doubt that to the west of the Nicobars there had been a rapid fall during the two previous days. On the morning of the 24th the 'Jamsetjee Cursetjee Botelbhoë' had passed within 40 miles of the 'Gauntlet's' noon position of the 27th, her barometer standing at 29·772 at noon of the 25th; when her barometric reading was nearly the same, she was at a distance of little more than a degree to the north.

The form of the area of depression would seem to have been a very elongated ellipse, or a trough, stretching from south to north, and of no great width. That the rise was rapid to the eastward, we have evidence in the observations of the 'Prince Arthur' and the 'Jamsetjee Cursetjee Botelbhoë.' On the other hand, the barometer of the 'Comorin,' at 200 miles to the west of the Little Andaman, showed a reduced reading of not less than 29·9. It is true that the barometer of this ship has not been compared, and it is not improbable that its readings are somewhat high, but that its error is so great as to produce the whole of the apparent difference of its reading and that of the 'J. C. Botelbhoë' barometer is highly improbable.

In Bengal the winds were from the N. and N.W. (the usual directions during the cold-weather months), and N.E. down the coast of Orissa and the Carnatic. Over the north of the bay, as on the previous day, the prevalent directions were N.E. and E.N.E. ('Winchester,' 'St. Marnock,' 'Léonie,' 'Mongolia'). But at Akyab the wind was southerly, and at Port Blair veering to S.E. The W.N.W. winds that had hitherto prevailed between the line and 5° or 6° N. lat. were now drawing round to the place of maximum depression, since the 'Astracan,' coming up from the Equator across this belt on the 27th and the following day, experienced strong breezes with hard squalls from W.S.W., the sky overcast with cirro-

stratus and scud moving rapidly from the westward. To the S.E., between Sumatra and the Malacca peninsula, the 'T. A. Gibb' encountered cloudy weather with occasional squalls and variable or southerly winds. Her barometer has not been compared with the Calcutta standards*, but, as far as can be judged by a comparison of its reading at the Sandheads with that of the Saugor Island instrument at a later date, the actual barometric pressure on the 27th, in lat. $2^{\circ} 18' N.$, long. $101^{\circ} 56' E.$, would seem to be about 29.8, or nearly the same as that recorded by the 'Astracan' on the Equator, 12 degrees to the westward, on the same date.

In and around the area of maximum depression a cyclone had already formed. Its centre was probably somewhere between the Andamans and Nicobars, as indicated by the wind-directions of the 'J. C. Botelbhoë,' the 'Timoor Shah' and 'Comorin,' and the 'Gauntlet;' and that its force was considerable may be inferred from the fact that the 'Feroze Shah,' bound from Carical to Penang, was dismasted on the 27th and driven on a bank near the Little Andaman, known as the South Brother. The four ships above mentioned experienced hard squalls and heavy rain, and the 'Timoor Shah' describes the wind as blowing a hard gale in the after part of the day.

During the five days under notice there appears to have been little change in the prevalent temperatures. A general fall of from 1 to 2 degrees is the utmost shown by the temperature Table given on a previous page. The decrease in the humidity is more marked at all the land stations, but especially at Patna, owing probably to the increasing prevalence of a northerly or north-westerly wind. It is much to be regretted that, owing to Mr. Barnes's departure from Ceylon, the valuable meteorological record which that gentleman used to keep, and an extract from which he was able to furnish for the discussion of the storm in 1864, is no longer available; and I am unable to ascertain whether the humidity of the atmosphere in Ceylon was as high as before the cyclone of 1864.

The principal facts exhibited in the foregoing description may be summed up as follows:—

For at least four days previously to the formation of the cyclone vortex the barometric pressure to westward of the Nicobars and the northern extremity of Sumatra was lower than elsewhere in or around the bay. It was also lower (on the 24th of October certainly, and probably on the previous day also) than on the open sea to the southward. The depression was gradually intensified up to the 27th, when it began to blow a hurricane on the northern limit of this area. It then amounted to -0.4 of the pressure in Bengal, -0.36 of that in Madras, and -0.22 of that on the Equator. It would appear, however, that over the greater part of the bay the pressure was nearly equable, and that the depression was local and bounded by a much higher barometric gradient than would be indicated by the figures above given. Thus the 'Gauntlet' reading was 0.29 less

* It was sent to me for comparison, but was injured in the carriage, so that no comparison could be made, and the tube had to be replaced.

than that at Port Blair, equal to a gradient of 1 inch in 1034 miles, while that of the 'J. C. Botelbhoë' showed a gradient of 1 inch in 700 miles.

Around the north and west coasts of the bay the differences of pressure and its changes were inconsiderable. On the 23rd there was a slightly higher pressure (0·05 in.) in the N.E. corner, which difference remained unaltered on the two following days. In Ceylon also the pressure was 0·03 greater than at Madras; at the same time there was a general rise of the barometer, small in amount, over Bengal and the northern and western coasts. On the 27th there was a general fall, and the pressures were nearly equalized.

Coincident with these changes were those of the winds. For many days previously to the 24th * light south-easterly winds prevailed on the west coasts of the bay, while in Bengal the wind was variable, with a predominance of easterly components. To the south, between the Equator and N. lat. 5°, a squally damp W.N.W. wind blew continuously, having prevailed at least from the 11th of October. On the 27th it became W.S.W., drawing round towards the area of depression. With the barometric rise on the 24th and 25th a N.E. wind set in in Bengal and down the western half of the bay displacing the S.E. wind, which, however, continued to be felt in the immediate neighbourhood of the Nicobars. *The cyclone vortex was formed by the indraught of these three currents to the preexisting area of barometric depression.*

The storm chart of the Bay of Bengal drawn up by Mr. Piddington shows that the majority of the cyclones, the tracks of which are there laid down, proceed from a line running from south to north by the Nicobars, Andamans, and the islands of the Arracan coast, following the westward side of the mountain-axis, which, in part submarine, is a prolongation of that of the Sunda Islands. Of these storms, several appear to have originated in the neighbourhood of the Andamans; but none of them have been traced back to a sufficiently early period to admit of a comparison of the circumstances of their origin with those of the storm now under discussion. The data for the great Calcutta cyclone of October 1864 discussed by Colonel Gastrell and myself in the report published by the Bengal Government, were insufficient for a satisfactory determination of the conditions under which it originated, but they offer several points of similarity to those detailed in the preceding pages.

The two storms agree approximately in their place of origin, in their course up the bay and over Bengal, and in their termination; and as regards period, both occurred at the close of the S.W. monsoon. The chief noticeable differences are, that the cyclone of 1864 originated about N. lat. 10°, and therefore 3° or 4° further north, and on the morning of the 2nd of October, or nearly a month earlier. Previously to this date, for at least five days, the wind in Ceylon had been from the west † or W.S.W.,

* I have tabulated them for the six days previous.

† We had no data to show how far to the south or south-east this direction prevailed,

with occasional squalls, especially on the latter days; and the same stormy damp wind prevailed over the south of the bay up to the Great Andaman on the 1st and 2nd. The greater northern extension of this current is, doubtless, connected with the above-mentioned difference in the position of the storm's birthplace. At Port Blair, on the 29th and 30th, and over the greater part of the bay as far south as Madras, southerly and east-south-easterly winds prevailed up to the 3rd of October. In Eastern Bengal alone the wind was northerly, becoming N.E. on the 1st and 2nd; but it was not until the 3rd of October that a N.E. wind established itself over the north and west of the bay. It may, however, be noticed that during the five days preceding the cyclone, an unusually high barometer prevailed in Bengal, and this may have been due to the existence of an upper northerly current. A north-east wind was also felt in lat. 15° on the north-west limb of the vortex, on the 2nd, but was evidently merely the indraught of the south-east current.

The barometer data for the storm of 1864 were few in number, and not comparable *inter se*; but we adduced some reason for the inference that a low barometric pressure prevailed near the Andamans for some days previously to the 2nd of October.

It appears, then, that the same three wind-currents eventually took part in the formation of both storms, viz. a south-east wind in the south-east of the bay, a north-east wind along the west coast, and a westerly wind to the south; but that while in the storm of 1864 the north-east wind did not prevail until after the formation of the vortex, up to which time the south-east current held possession of the bay, in that of 1867 the former current had established itself three days prior to the commencement of the cyclone. These facts, coupled with the further fact that neither the north-east nor (at this time of year) the south-east currents are stormy winds capable of feeding the vortex and increasing the barometric depression, tend to confirm the view enunciated in the Report on the Calcutta Cyclone of 1864, viz. that the formation of the vortex was mainly determined by the inrush of a saturated westerly current towards the place of low barometer.

The fact above mentioned, that the majority of the Bay of Bengal cyclones arise along a line parallel to, and immediately to the west of, the chain of islands that form the eastern boundary of the bay, indicates the operation of some general cause tending to produce a low atmospheric pressure in that region at those seasons at which cyclones are most prevalent. Such a cause may be suggested, but the data available to me are not sufficiently precise to establish its existence. If it can be shown that, either owing to a predominance of marine currents from the south, or to any other cause, the water along the eastern side of the bay has a higher temperature than that of the western side during those months at which cyclones prevail, the increased evaporation thus arising, together with the

but despite the difference in mean direction, the wet squally character of this wind permits of its identification with Maury's westerly monsoon of the line.

more elevated temperature it would impart to the atmosphere, would give rise to a diminution of barometric pressure, small at first, but becoming more marked with the continued operation of the producing cause, so that after several days it might become capable of causing that extensive indraught of air which appears to be the immediate antecedent of the formation of a cyclone vortex.

According to Horsburgh, from April to the early part or middle of October, a current generally sets to the north or north-east all over the bay in the open sea, but governed in its direction and strength by the prevailing winds. "In the eastern side of the bay, and about the entrance to the Malacca Strait more particularly, it sometimes sets to the southward. The current begins to set along the coast of Coromandel to the southward in October, sometimes about the middle of the month. Near the end of this month, or early in November, it begins to run very strong to the southward." He adds, however, that the period of the currents or monsoons changing in the Bay of Bengal is not always the same. These changes happen in some years nearly a month sooner or later than in others.

From this and the remainder of the description, it is to be gathered that the set of the current changes with the monsoon, and that in general its direction and strength are governed by the prevailing winds. Now it is well known that at the change from the south-west to the north-east monsoon, the latter is first felt down the western part of the bay, and that at this season ships bound to Calcutta from the southward keep up the east of the bay, with a view to catching a favourable wind. It might be expected, therefore, that there would be a tendency to northerly currents in the east of the bay, and to southerly currents in the west; and, so far as can be gathered from Horsburgh's description, such indeed appears to be the case; but facts are at present wanting to establish it, and also the existence of those differences of water-temperature which would seem to be the necessary consequence.

It would appear from Horsburgh's description that, at the commencement of the south-east monsoon in May (the minor of the two cyclone seasons), the tendency of the currents is the opposite of the above, that from January to June a northerly current sets strongly up the Coromandel coast, and that from April there is a current to the north or north-east all over the bay, but on the eastern side of the bay, and particularly the entrance to Malacca Strait, it sometimes sets to the south. Now from April to August the sun is vertical over the northern part of the bay; but no data are available to show how the temperature of the currents is affected by this change in its declination, and I am unable therefore to ascertain how far the supposed condition of a higher temperature in the currents of the east of the bay holds good in the case of those barometric depressions which determine the cyclones of the beginning of the south-west monsoon, and which Piddington's chart shows to originate in that region. I may, however, remark that, as far as I can gather from the recorded cyclones I

482 Dr. W. A. Miller on a Self-registering Thermometer [June 17,

have tabulated, these storms appear to be about half as frequent only at the beginning as at the end of the south-west monsoon.

It is probable that in the course of a few years, if not already, the observations of currents and sea temperatures, collected by the Meteorological Department of the Board of Trade, will afford data for a satisfactory discussion of this subject, which is one of great importance to the comprehension of the meteorology of the Bay.

POSTSCRIPT. Received June 29, 1869*.

Since the above was written, I have visited Chittagong, and have found that the elevation of the barometer-cistern at that place above sea-level (which had been reported as 166·46 feet) is actually about 108 ft. only. This correction requires an alteration of the reduced barometric pressures for Chittagong (given on p. 473), which will consequently stand as follows:—

	23rd.	24th.	25th.	26th.	27th.
Chittagong ..	29·906	29·913	29·949	29·873	29·894

A few corrections must also be made in the text. The excess of pressure at Chittagong, as compared with certain other stations on the 23rd and 25th, disappears, and on the 26th and 27th the noon pressure at this place becomes lower than at any other station. The conclusions arrived at in the foregoing paper are, however, unaffected by the correction.

III. "Note upon a Self-registering Thermometer adapted to Deep-sea Soundings." By W. A. MILLER, M.D., Treas. and V.P.R.S. Received June 3, 1869.

The Fellows of the Royal Society are already aware that the Admiralty, at the request of the Council of the Society, have placed a surveying-vessel at the disposal of Dr. Carpenter and his coadjutors for some weeks during the present summer, to enable them to institute certain scientific inquiries in the North Sea. Among the objects which the expedition has in view is the determination of deep-sea temperatures.

Now it is well known that self-registering thermometers of the ordinary construction are liable to error when sunk to considerable depths in water, in consequence of the diminution produced for the time in the capacity of the bulb under the increased pressure to which it is subjected. The index, from this cause, is carried forward beyond the point due to the effect of mere temperature, and the records furnished by the instrument rise too high†.

A simple expedient occurred to me as being likely to remove the diffi-

* A chart, with wind arrows, showing the limits of the cyclone, accompanies the paper, and is preserved for reference in the Archives of the Society.

† In sea-water of sp. gr. 1·027, the pressure in descending increases at the rate of 280 lbs. upon the square inch for every 100 fathoms, or exactly one ton for every 800 fathoms.

culty; and as upon trial it was found to be perfectly successful, I have thought that a notice of the plan pursued might not be unacceptable to future observers.

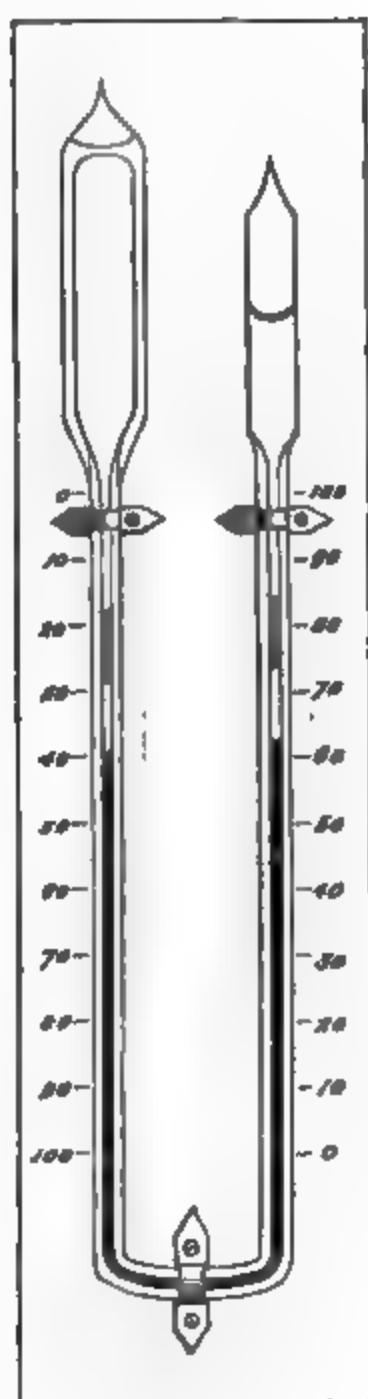
The form of self-registering thermometer which it was decided to employ is one constructed upon Six's plan. Much care is requisite in adjusting the strength of index-spring, and the size of the pin, so as to allow it to move with sufficient freedom when pressed by the mercury, without running any risk of displacement in the ordinary use of the instrument while raising or lowering it into the water. Several of these thermometers have been prepared for the purpose with unusual care by Mr. Casella, who has determined the conditions of strength in the spring and diameter of tube most favourable to accuracy. He has also himself had an hydraulic press constructed expressly with the view of testing these instruments. By means of this press the experiments hereafter to be described were made.

The expedient adopted for protecting the thermometers from the effects of pressure consisted simply in enclosing the bulb of such a Six's thermometer in a second or outer glass tube, which was fused upon the stem of the instrument in the manner shown in the accompanying figure. This outer tube was nearly filled with alcohol, leaving a little space to allow of variation in bulk due to expansion. The spirit was heated to displace part of the air by means of its vapour, and the outer tube and its contents were sealed hermetically.

In this way, variations in external pressure are prevented from affecting the bulb of the thermometer within, whilst changes of temperature in the surrounding medium are speedily transmitted through the thin stratum of interposed alcohol. The thermometer is protected from external injury by enclosing it in a suitably constructed copper case, open at top and bottom, for the free passage of the water.

In order to test the efficacy of this plan, the instruments to be tried were enclosed in a strong wrought-iron cylinder filled with water, and submitted to hydraulic pressure, which could be raised gradually till it reached three tons upon the square inch, and the amount of pressure could be read as the experiment proceeded upon a gauge attached to the apparatus.

Some preliminary trials made upon the 5th of May showed that the



press would work satisfactorily, and that the form of thermometer proposed would answer the purpose.

These preliminary trials showed that, even in the thermometers with protected bulbs, a forward movement of the index of from $0^{\circ}\cdot5$ to 1° F. occurred during each experiment. This, however, I believed was caused, not by any compression of the bulb, but by a real rise of temperature, due to the heat developed by the compression of the water in the cavity of the press.

This surmise was shown to be correct by some additional experiments made last week to determine the point. On this occasion the following thermometers were employed:—

No. 9645. A mercurial maximum thermometer, on Prof. Phillips's plan, enclosed in a strong outer tube containing a little spirit of wine, and hermetically sealed.

No. 2. A Six's thermometer, with the bulb *protected*, as proposed by myself, with an outer tube.

No. 5. A Six's thermometer, with a long recurved cylindrical bulb, also *protected* in a similar manner.

No. 1. Six's thermometer, with cylindrical bulb of extra thickness, *not protected*.

No. 3. Six's thermometer, with spherical bulb, extra thick glass, *not protected*.

No. 6. Admiralty instrument, Six's thermometer, ebonite scale, bulb *not protected*.

No. 9651. An ordinary Phillips's maximum mercurial thermometer, spherical bulb, *not protected*.

The hydraulic press was exposed in an open yard, and had been filled with water several hours before. A maximum thermometer, introduced into a wrought-iron tube filled with water, open at one end to the outer air, closed at the other, where it passed into the water contained in the press, registered $46^{\circ}\cdot7$ at the commencement, and 47° at the end of the experiment. Temperature of the external air 49° F.

In commencing the experiment, the seven thermometers under trial were introduced into the water in the cavity of the press, and after a lapse of ten minutes the indices of each were set, carefully read, and each instrument was immediately replaced in the press, which was then closed, and by working the pump the pressure was gradually raised to $2\frac{1}{2}$ tons upon the inch. It was maintained at this point for forty minutes, in order to allow time for the slight elevation of temperature caused by the compression of the water to equalize itself with that of the body of the apparatus. At the end of the forty minutes the pressure was rapidly relaxed. A corresponding depression of temperature was thus occasioned, the press was opened immediately, and the position of the indices of each thermometer was again read carefully; and the water was found to be at a temperature sensibly lower than before the experiment began, by about $0^{\circ}\cdot6$ F. By this means it was proved that the forward movement of the index in the protected thermometers, amounting to $0^{\circ}\cdot9$, was really due to temperature,

and not to any temporary change in the capacity of the bulb produced by pressure.

This will be rendered evident by an examination of the subjoined Table of observed temperatures ;—

First Series : Pressure 2½ tons per square inch.

Number of Thermometer.	Minimum index.		Maximum index.		Maximum mercury. After.
	Before.	After.	Before.	After.	
Protected ... 9645	47·0	47·7	46·5 46·0
„ ... 2	47·0	46·5	46·7	47·6	
„ ... 5	47·0	46·3	46·5	47·6	
Mean	47·6	
Unprotected. 1	46·7	46·4	46·5	54·0	46
„ 3	47·0	46·5	46·5	56·5	46
„ 56	47·0	46·0	47·0	55·5	46
„ 9651	46·7	118·5	
Mean	46·9	46·3	46·7	46·1
Temperature of external air.....			49	49	
Temperature of thermometer } in press			46·7	47	

In the Phillips's maximum thermometer, with unprotected spherical bulb, No. 9651, the bulb had experienced so great a degree of compression as to drive the index almost to the top of the tube. In all the other unprotected instruments, which had been made with bulbs of unusual thickness, the index had been driven beyond its proper position from 6°·4 to 8°·9 F.; and it is obvious that the amount of this error must vary in each instrument with the varying thickness of the bulb and its power of resisting compression.

Notwithstanding the great pressure to which these instruments had been subjected, all of them, without exception, recovered their original scale-readings as soon as the pressure was removed.

It will be seen that the mean rise of temperature indicated by the three protected instruments was 0°·9 F., whilst the mean depression registered on removing the pressure amounted upon all the instruments which admitted of its measurement to 0°·6, an agreement as close as was to be expected from the conditions of the experiment.

A second set of experiments was made upon the same set of instruments, with the exception of 9651; but the pressure was now raised to 3 tons upon the inch; this was maintained for ten minutes. When it had risen to 2½ tons a slight report was heard in the press, indicating the fracture of one of the thermometers. On examining the contents of the press afterwards it was found that No. 2 was broken, the others were uninjured.

The broken thermometer was the earliest constructed upon the plan now proposed, and it was consequently not quite so well finished as subsequent practice has secured for those of later construction. The results of the trial under the higher pressures showed an increase in the amount of compression experienced by the unprotected instruments rising in one instance to as much as $11^{\circ} \cdot 5$ F. With the protected instruments the rise did not exceed $1^{\circ} \cdot 5$, due, as before, to the heat evolved from the water by its compression.

A pressure of 3 tons, it may be observed, would be equal to that of 448 atmospheres of 15 lb. upon the square inch; and if it be assumed that the diminution in bulk of water under compression continues uniformly at the rate of 47 millionths of its bulk for each additional atmosphere, the reduction in bulk of water under a pressure of 3 tons upon the square inch will amount to about $\frac{1}{7}$ of its original volume. This probably is too high an estimate, as the rate of diminution would most likely decrease as the pressure increases.

IV. "Magnetic Survey of the West of France." By the Rev. STEPHEN J. PERRY, F.R.A.S., F.M.S. Communicated by the President.
Received June 3, 1869.

(Abstract.)

This survey was undertaken by the Rev. W. Sidgreaves and myself in connexion with the Observatory at Stonyhurst College. The instruments employed were those in constant use for the monthly observations of the magnetic elements at this observatory, *i. e.* Barrow's dip-circle, No. 33, a unifilar by Jones, and Frodsham's chronometer, No. 3148. A portable altazimuth and an aneroid barometer were kindly placed at our disposal by the late Mr. Cooke.

A complete set of observations of the dip, declination, and horizontal intensity were taken at the following stations:—Paris, Laval, Brest, Vannes, Angers, Poitiers, Bordeaux, Abbadia (near Hendaye), Loyola, Bayonne, Pau, Toulouse, Périgueux, Bourges, Paris (a second time), and Amiens. The chronometer was compared on every possible occasion, and its rate was found to be nearly always 2" per day.

The dip was observed according to the description of the observation given by the President of the Royal Society in the 'Manual of Scientific Inquiry.'

The method of vibrations and deflections was invariably adopted for determining the horizontal component of the intensity. For the declination it was deemed most convenient to find the azimuth of a fixed mark by observing transits of the sun with Cooke's altazimuth, and then to measure the azimuthal angle between the magnet and the fixed mark with Jones's unifilar. Dr. Lloyd's method, by reflection, was made use of only at Brest. The results of these observations, reduced to the epoch January 1st, 1869, are contained in the following Table:—

	Dip.	Decl.	H. F.
Paris	65° 875	17° 841	4° 1133
Laval	65° 802	19° 073	4° 1245
Brest	66° 460	21° 005	4° 0442
Vannes	65° 585	20° 225	4° 1328
Angers	65° 140	19° 093	4° 2106
Poitiers	64° 468	18° 306	4° 2955
Bordeaux	63° 383	18° 209	4° 4110
Abbadia	62° 463	18° 235	4° 5456
Bayonne	62° 503	18° 391	4° 5520
Pau	61° 970	17° 825	4° 5823
Toulouse	62° 018	17° 122	4° 5883
Périgueux	63° 398	17° 682	4° 4268
Bourges	64° 543	17° 003	4° 2845
Amiens	66° 672	18° 316	4° 0143
Secular Variation . . .	—3'·68	$\left(\begin{array}{c} +0\cdot0050 \\ 0\cdot00002 \end{array} \right)_2 \left(\begin{array}{c} -9'\cdot1 \\ 0\cdot19 \end{array} \right)_1$	
Acceleration	0·043		

The secular variation has been obtained by comparing the observations of this survey with those of Dr. Lamont, taken about ten years previously.

Maps of the isodynamic, isoclinal, and isogonic lines of the epoch, September 1st, 1868, are drawn from the following data, Paris being chosen as the central station for reasons given in the paper:—

For the isoclinals the direction is

N. 73° 25' 10" E. to S. 73° 25' 10" W.,

the distance between the lines being 44·25 geographical miles for a change of 30' of dip.

The direction for the isogonics is

N. 20° 31' 16" E. to S. 20° 31' 16" W.,

and the distance only slightly greater than for the isoclinals, *i. e.* 44·35 geographical miles for 30' of angle.

The isodynamics lie in the direction

N. 70° 34' 13" E. to S. 70° 34' 13" W.,

the distance in this case being 115 geographical miles for a change of 0·1 in the intensity.

For the lines of equal horizontal force the direction is

N. 74° 19' 30" E. to S. 74° 19' 30" W.,

and 72 geographical miles the distance separating lines where the horizontal intensity differs by 0·1.

An attempt has been made to apply a correction for the magnetic disturbances at the times of observation by means of the magnetograms obtained at Stonyhurst Observatory during the Survey; but these corrections have not been taken into account in forming the equations of condition from which the final results have been obtained.

The probable error of any single observation of the dip, declination, total force, and horizontal component are found to be respectively

3'·13; 0'·95; 0·0144; and 0·0067.

V. "An Account of Experiments made at the Kew Observatory for determining the true Vacuum- and Temperature-Corrections to Pendulum Observations." By BALFOUR STEWART, Esq., F.R.S., and BENJAMIN LOEWY, Esq., F.R.A.S. Received May 27, 1869.

1. Pendulum-observations, whether undertaken for the purpose of obtaining unalterable standards of length or for physical and geodetic objects, are usually made in air or in a receiver, from which the air is partially or almost entirely withdrawn; and in order to render such observations, made at different places and by different observers, capable of intercomparison, they are, by means of a "correction for buoyancy," reduced to a vacuum. It is well known that the most illustrious physicists and mathematicians have given a great deal of attention to a correct determination of the principles on which this reduction to a vacuum ought to be based, and of the actual resistance which such a body as a pendulum meets during its vibrations in a fluid body. Until some years ago, especially since the researches of General Sabine and Bessel, it was thought best to determine for every pendulum a certain constant by finding its vibrations in air at the usual pressure, and also in a receiver from which the air is as much as possible withdrawn; from the difference in the number of vibrations thus found the correction was then calculated on the assumption that this difference is proportional to the difference of density of the air.

2. In the pendulum-observations made at the Kew Observatory in connexion with the Great Trigonometrical Survey of India (*vide* Proceedings of the Royal Society for 1865, No. 78) we adopted, for determining the necessary constant, the method first carried out by General Sabine, and of which a detailed account is given in the Philosophical Transactions for 1829, Part I. page 207 &c. But since our account has been published, two eminent physicists, Professor Clerk Maxwell and Professor O. E. Meyer in Breslau, have independently investigated the internal friction in gases, and its effect upon bodies moving in them; and among the prominent results obtained by them is this, that the influence of the internal friction of a fluid on a moving body is not proportional to its density. However, for small differences of pressure, such as those experienced by General Sabine in his researches, the old method for determining the correction is sufficiently accurate; or again, if a series of such experiments as our own fundamental Kew observations for India be made at a very low pressure, say from $\frac{1}{2}$ an inch to $1\frac{1}{2}$ inch, the correction is itself a very small quantity; and the application of a more correct principle of reduction will not sensibly affect the ultimate results, because the difference between the true and approximate correction is in such a case extremely small. But if, as is the case in the Indian observations, experiments are made at higher and varying pressures, it is very desirable to apply experimental methods which will give the true correction.

3. With a view to collect for the theory of the subject a great many

carefully conducted experiments, and also to supply those who are actually engaged in pendulum-experiments at the present time with practically valuable results, we proposed to ourselves to observe the behaviour of pendulums of the different forms hitherto used in such researches in which the pendulum is employed, at pressures varying through the whole range, from the lowest obtainable in a receiver to the usual atmospheric pressure. The carrying out of our intentions met, however, with many delays through unavoidable circumstances, and there is, indeed, at present little prospect of our being able to complete the whole of the original plan. We give, therefore, here an account of some preliminary results which are, in our opinion, not without practical importance, and which will certainly find their use in the reduction of observations made with pendulums of a form similar to that used by ourselves, viz. that form of reversible pendulum known as "Kater's pendulum."

4. The following is an account of the operations:—The pendulum was swung in the Kew receiver, made of five pieces, two of metal and three of glass, the parts fitting closely, and the whole being connected with siphon-gauge and air-pump by tubes. One of the metal pieces is perforated behind and in front, and the apertures are covered by plate glass for the observation of the coincidences.

The pendulum was swung at the following pressures:—

I. At about $\frac{1}{4}$ of an inch (lowest obtainable).	V. Between 4 and 5 inches.	X. At about 20 inches.
II. Between 1 and 2 inches.	VI. " 5 " 6 "	XI. " " 25 "
III. " 2 " 3 "	VII. " 7 " 8 "	XII. At the full atmospheric pressure.
IV. " 3 " 4 "	VIII. At about 10 inches.	
	IX. " " 15 "	

At each pressure a good many observations were made, in order to ensure reliable mean results.

5. With reference to the *registration of the observations*, we have strictly adhered to the method previously adopted after careful consideration, and explained in our former account; hence we need not here enter upon this part again. Instead of registering *one* coincidence at the beginning, during the progress, and at the end of an experiment, we have this time in most cases observed *three* successive coincidences, and the arithmetical mean of these, together with the mean of the corresponding registrations of arc, temperature, and pressure, stands for one observation; we think that this method ensures greater correctness, although it is more laborious than that previously adopted.

6. The *reduction of the observations* comprises, as shown in the previous paper alluded to above, several corrections to be applied to the number of observed vibrations; we shall mention here only those points which differ numerically or experimentally from the numbers or methods explained in that paper, which contains also an experiment with its full reductions. By referring to these and the following remarks our method of procedure will be so abundantly clear, we hope, that we shall be able to proceed immediately afterwards to the statement of the final results.

A. Correction of the observed arc-readings and reduction of the vibrations to infinitely small arcs.

We have previously shown that if

D = distance of scale from the object-glass of the telescope,

d = distance of scale from the tailpiece of the pendulum,

O = observed scale-reading for the whole arc of vibration,

S = distance of indicating-point of the tailpiece from the knife-edge,

α = true semiarc of vibration,

$$\text{then } \tan \alpha = \frac{O(D-d)}{2DS},$$

expressing all distances in inches into which the scale is divided.

In our case repeated measurements gave the following mean values for these quantities :—

$$D = 101.86 \text{ inches ; } d = 0.56 \text{ inch ; } S = 47.55 \text{ inches.}$$

$$\text{Hence we have to add } \log\left(\frac{D-d}{2DS}\right) = \log\left(\frac{101.3}{2 \times 101.86 \times 47.55}\right) = 2.0194252$$

to the logarithm of the observed scale-readings to obtain the semiarc of vibration.

For the reduction to infinitely small arcs, we have again used the well-known formula number of infinitely small vibrations

$$= n + n \cdot \frac{M \sin(\alpha + \alpha') \sin(\alpha - \alpha')}{32 (\log \sin \alpha - \log \sin \alpha')},$$

the symbols having the same meaning as previously stated.

We are well aware that more convenient formulæ, and more correct methods, have been used or proposed by different observers for this correction; but we thought it best to adhere to a uniform method in the reductions, in order to facilitate any future rediscussion of our original observations (which are preserved at the Kew Observatory), should such appear desirable when the results of the Indian pendulum observations will be published.

B. The precise determination of the *rate of the clock* might have been of minor importance in our experiments, an approximate uniformity of rate being the chief desideratum. We expected, however, that very small differences in the number of vibrations would result in those experiments where the pressure differed only by an inch or even less. We considered it hence of the utmost value to have a precise record of the behaviour of the clock during these experiments, so as to discover at once changes in the rate, and to make our reductions depending on it for each experiment. A great number of transit-observations were accordingly made, and during these not only the pendulum-clock, but also the behaviour of a chronometer by Dent and a meantime-clock by Shelton was accurately determined. These two latter served during those days when no transits could be obtained for deducing the rate of the pendulum-clock by intercomparison. From the whole of these observations the following Table of the number of vibrations during a mean solar day has been calculated for every day of

the four months during which the experiments were carried on, each result being of course employed for the pendulum-experiments of the corresponding day. The Table shows that our plan was the safest, as differences of nearly one second are observable; these differences have, however, apparently no connexion with changes of temperature, as the rate of the clock during the artificial heating of the pendulum-room, of which we shall soon have to speak, showed hardly any difference from the mean rate, proving that the compensation was not faulty.

The pendulum-clock showing sidereal time, the Table is calculated from the formula:—Number of vibrations in a mean solar day = $N' = 86636.5554 \left(1 - \frac{r}{86400}\right)$; and hence: Number of vibrations of detached pendulum = $N = \frac{VN'}{V'}$, where V, V' are respectively the number of vibrations of the detached and clock-pendulum from beginning to the end of our experiment.

TABLE I. *Number of Vibrations made by the clock-pendulum during a mean solar day.*

1866.	September.	October.	November.	December.
1.	76577.40	86577.38	86577.43	86577.50
2.	.31	.25	.32	.40
3.	.50	.14	.20	.39
4.	.29	.09	86576.99	.48
5.	86576.97	.21	86577.02	.31
6.	.83	.22	86576.94	.39
7.	.81	.06	.95	.50
8.	.88	86576.91	.99	.54
9.	.90	86577.00	86577.11	.54
10.	86577.04	.11	.17	.51
11.	.00	.13	.24	.61
12.	.07	.28	.33	.68
13.	.26	.10	.28	.70
14.	.16	.04	.40	.70
15.	.11	.22	.53	.64
16.	.08	.29	.61	.64
17.	86576.98	.40	.60	.60
18.	.90	.50	.70	.70
19.	.70	.54	.75	.71
20.	.90	.60	.71	.66
21.	.99	.61	.80	.59
22.	.93	.43	.74	.40
23.	.85	.28	.70	.47
24.	.83	.27	.62	.44
25.	.99	.47	.51	.46
26.	86577.20	.40	.49	.48
27.	.21	.29	.44	.47
28.	.33	.33	.64	.59
29.	.30	.35	.56	.61
30.	86577.24	.29	86577.56	.51
31.	86577.40	86577.56

C. *Correction for temperature.*—The method of suspending the ther-

mometers was precisely the same as that used in our previous experiments and described in the account we gave of them to the Royal Society. The formula employed for deducing the most probable mean temperature for each experiment was as before, using the same symbols:—

$$t^o = \frac{n\left(\frac{t+t'}{2}\right) + n'\left(\frac{t'+t''}{2}\right) + n''\left(\frac{t''+t'''}{2}\right) \dots}{n+n'+n'' \dots}$$

As explained in the paper mentioned above, we had then no time at our disposal to swing the pendulums used at extremes of temperature, and hence we availed ourselves of the elaborate series of experiments on the temperature-correction of pendulums made by General Sabine (*vide* Phil. Trans. 1830, p. 251), adopting the mean of his entire results, viz. 0.435 vibrations per diem, as correction for 1° of Fahrenheit's scale for our reductions.

In our present investigation we thought it indispensable to obtain the utmost accuracy, by ascertaining the temperature-correction for each pendulum intended to be used by an independent series of experiments in an artificially heated room, the natural annual range of temperature in the Kew pendulum-room being insufficient for our purposes. The arrangement consisted in erecting an iron stove in the vicinity of the pendulum-apparatus, and carrying a long pipe through the whole height of the room. By several preliminary trials, it was soon found that up to about 80° the temperature could be maintained constant for several hours, but that the difficulties increased with the rise of the temperature, and became almost unsurmountable when the temperature was above 100°. Besides the maintenance of a pretty equable temperature during the duration of an experiment, another difficulty arose. The pillar of masonry which carries the apparatus on one side, and the wall of the room on the other, prevented us giving to the heating-apparatus such a lateral position as to bring the bar which carried the thermometers and the pendulum in equal proximity to it. The stove had to be placed in front, somewhat to the left of the apparatus, and hence the brass bar which carried the thermometers was nearer to the source of heat than the pendulum. In order to arrive at the most exact measurement of the real temperature of the pendulum, two additional thermometers were suspended behind it, at about the same distance from it as those in front, and all four were read during the experiments.

Seeing from the preliminary trials that an approximately equal distribution of temperature throughout the apparatus could only be relied upon up to about 70°, and that after that point the differences in the readings of the thermometers, behind and in front, increased to an extraordinary degree, we decided upon making two different classes of experiments, viz. one set confined to temperatures of about 70°, and another comprising higher temperatures; and we further, during the pro-

cess of reducing our observations, came to the conclusion that it would be best to exclude all experiments made at temperatures above 100° , and also those where great differences in the readings for temperature occurred, from our final results. The principle which guided us was not to vitiate good observations by doubtful ones; and the following small Table, showing the temperature-readings during four experiments, taken quite at random, will show best how we proceeded:—

I.			
Thermometer in front.		Thermometer behind.	
Upper therm.	Lower therm.	Upper therm.	Lower therm.
71.20	70.20	70.0	68.5
71.15	70.20	70.4	69.4
71.30	70.00	70.6	69.7
71.85	69.60	71.4	71.4
71.00	69.55	71.4	71.6
71.90	69.50	71.4	70.7
Mean 70.9		70.6	
70.8			
Experiment good.			

II.			
Thermometer in front.		Thermometer behind.	
Upper therm.	Lower therm.	Upper therm.	Lower therm.
85.6	83.7	84.8	83.5
86.1	83.8	84.0	83.0
86.7	83.6	83.3	82.6
78.4	78.8	82.2	82.1
79.4	80.4	80.8	81.2
81.2	82.0	79.4	82.8
82.5		82.45	
82.5			
Experiment good.			

III.			
99.5	98.7	92.4	91.5
97.4	97.0	91.6	90.6
95.5	94.3	90.3	88.1
82.5	79.9	78.5	76.2
81.6	79.3	77.4	76.0
80.9	79.2	75.5	74.1
Mean 88.8		83.7	
86.2			
Experiment rejected.			

IV.			
108.4	107.3	100.6	99.5
107.6	106.5	99.7	99.0
105.7	103.8	98.5	97.3
97.4	96.5	93.1	91.3
96.3	95.2	92.2	90.9
95.9	94.7	91.8	90.3
101.3		95.3	
98.3			
Experiment rejected.			

Although in the rejected experiments the means of all readings of both sets of thermometers approach each other, still there occurs a fall of nearly 20° at the end of an experiment, as compared with that temperature which is recorded at the beginning, besides differences of nearly 8° between the thermometers in front and those at the back of the pendulum. That the temperature of the latter during an experiment is represented by the arithmetical mean of such discordant readings, we think most unlikely; and hence these experiments and similar ones were not used, although, of course, the number of available experiments was thereby reduced.

The following experiments, which represent the final results of this temperature-investigation, deserve at least some confidence, although we

should have liked to see their number much increased. To avoid a correction for pressure, we took care to correct at once, before beginning an experiment, the reading of the gauge to 32° of temperature, and to regulate by a few strokes of the pump the pressure, so as to assimilate it to the mean pressure (also reduced to 32°) of the experiments previously made in cold air. All pressures are reduced to 32°, and the small difference of pressure which still resulted, comparing the mean of the hot-air experiments with those in cold air, amounting to about $\frac{1}{100}$ of an inch, has been disregarded in the final reduction.

An attempt to test the constancy of the temperature-correction in *vacuo*, with reference to a suggestion made by Colonel Walker, Superintendent of the Great Indian Survey, who suspects that the coefficient of expansion of a pendulum in air varies slightly from that in a vacuum, proved a failure. The pomatum which is used for tightening the different parts of the receiver melted by the heat of the stove, and rendered it impossible to reduce the pressure in the receiver sufficiently for the purpose of the experiments.

I. Experiments made in cold air.				II. Experiments made in hot air.			
No. of exp.	Temperature.	Pressure.	No. of vibrations per day.		No. of exp.	Temperature.	No. of vibrations per day.
	°	inches.				°	
1.	47.84	30.052	86013.60	First set.	1.	70.6	86002.72
2.	47.77	30.112	86013.76		2.	71.3	86002.64
3.	46.33	30.182	86013.82		3.	72.1	86002.50
4.	46.25	29.938	86014.02		4.	70.8	86002.90
5.	47.72	30.460	86013.58		5.	73.8	86002.43
6.	47.91	29.498	86013.64		1.	82.5	85997.84
7.	48.43	29.582	86013.98		2.	82.5	85997.10
8.	47.76	29.328	86014.40		3.	83.9	85995.65
9.	45.09	30.202	86013.90		4.	88.3	85992.23
10.	46.19	30.011	86013.99		5.	90.8	85992.73
11.	47.50	30.107	86013.61		6.	99.9	85988.43

The following are the *mean results*, with their respective *differences* :—

	Temp.		Pressure.		Vibra-
	°		inches.		tions.
A. Experiments in cold air	47.16	29.952	86013.85
B. Experiments in hot air, first set	71.64	29.960	86002.64
C. Experiments in hot air, second set ...	88.00	29.959	85994.00

Resulting differences of temperature and number of vibrations :—

	Temperature.		Vibrations.
A ~ B	= 24.48	11.21
A ~ C	= 40.84	19.85
B ~ C	= 16.36	8.64

Hence we find a correction between

$$\left. \begin{array}{l} 47^{\circ}16 \text{ and } 71^{\circ}64 \text{ of } \frac{11.21}{24.48} = .458 \text{ vibrations} \\ 47^{\circ}16 \text{ ,, } 88^{\circ}00 \text{ ,, } \frac{19.85}{40.84} = .486 \text{ ,,} \\ 71^{\circ}64 \text{ ,, } 88^{\circ}00 \text{ ,, } \frac{8.64}{16.36} = .528 \text{ ,,} \end{array} \right\} \begin{array}{l} \text{per diem for one} \\ \text{degree of Fahr-} \\ \text{enheit's scale.} \end{array}$$

Comparing these results with those obtained by General Sabine (Phil. Trans. 1830, p. 251, &c.), we find that the pendulums employed by him gave a correction of 0.44 of a vibration per diem for each degree of Fahrenheit between 30° and 60°, a result which agrees well with that found by ourselves between 40° and 70°, the small difference being probably referable to a difference in the composition of the metal of which the pendulums were made. But a considerable difference appears in the experiments made at the higher temperature. General Sabine made some experiments, previously to those discussed in the above-mentioned paper, with two different pendulums in a chamber artificially heated to between 80° and 90°, which gave for the correction for each degree of Fahrenheit, respectively for the two pendulums, 0.432 and 0.430 vibrations, corresponding to that part of the thermometer-scale which is included between 45° and 85°. These results are somewhat different from those which are obtained for the scale-reading between 30° and 60°, and General Sabine points to this difference in the following words * :—

“In the experiments in the chamber artificially heated, the fluctuations of temperature, in spite of every precaution, were considerable, and rendered the determination of the mean temperature more difficult, and probably less exact than in the natural temperatures; hence it would be unsafe to conclude in favour of the inference to which these facts would otherwise lead, that the correction at high temperatures is less than at low temperatures, or that the metal expands a smaller proportion of its length for one degree between 85° and 45° than for one degree between 60° and 30°.”

Our own experiments, on the other hand, seem to agree with the general fact that the coefficient of expansion increases with the temperature, and that in a series of experiments a lower range of temperature will give a lower, a higher range a greater value for the expansion for one degree. Nevertheless the values resulting from our high temperature-experiments appear decidedly too large to be explained solely by this general behaviour of bodies; and in our reductions of the pressure-experiments, where the differences of temperature, as will be seen in the following paragraph, are inconsiderable, we have adopted that value for the temperature-correction which results from the experiments between 45° and 70°, viz. 0.458 of a vibration for one degree, a result which not only well agrees with those found by General Sabine, but also appeared to our

* Phil. Trans. 1830, p. 252.

own considerations the most reliable, for reasons which will appear presently.

Speaking generally of the subject of the temperature-correction, we must admit that our experiments do not tend to remove the difficulties that seem to surround it. Our experience goes to prove, what the Indian officers, entrusted with the pendulum-experiments and their reduction, have also suspected, that the thermometers fixed to a so-called dummy-bar (in order to place them in conditions similar to the swinging pendulum) do not give a true indication of the real temperature of the pendulum. If this is the case, the differences found by ourselves between the result of the lower and that of the higher range can easily be understood. Indeed, during the progress of these experiments it has always appeared to us that not only the fluctuations indicated by the thermometers are *greater* in range than those to which the pendulum itself is subjected, but that they are also more rapid, and that the heavy and substantial pendulum cannot keep time in these changes with the light and delicate thermometers which are not absolutely sealed up into the substance itself. In our experiments, each of which lasted from one to two hours, a high temperature was usually produced at the beginning, and we attempted to maintain the heat as much as possible by keeping the pendulum-room closely shut on all sides during the progress of the experiment. The inrush of cold currents can, however, obviously not be wholly prevented, and a steady, more or less considerable fall of the temperature is recorded in each of the experiments beyond 70° . This fall affects, in our opinion, chiefly the thermometers themselves, while probably the pendulum maintains its higher temperature much longer. Thus we are inclined to think that the mean temperature of the pendulum, if it could by some means be exactly ascertained, might perhaps appear considerably higher than the mean of the thermometer-readings recorded; and to this circumstance we ascribe it mainly that the high temperature-experiments give too large a correction, for in these experiments a greater difference in the number of vibrations corresponds to an apparently smaller difference in temperature.

The question will, we have reason to hope, find its best solution by the labours of Colonel Walker and Captain Basevi in India, where these gentlemen can avail themselves of a great natural range, which will free the experiments from the doubts and difficulties met by ourselves; but we cannot conclude this part without reminding experimenters of the words with which, nearly forty years ago, General Sabine concluded the account of his own experiments, and which have gained new force by the shortcomings of our own investigations*.

“It seems therefore desirable, for the sake of experiments, which are becoming greatly multiplied, and which are daily increasing in accuracy, that means should be devised of obtaining the rates of pendulums in

* Phil. Trans. 1830, p. 253.

artificial temperatures, embracing a wider range than the natural temperatures, but capable of being determined with equal accuracy.”

7. There remains now only to give the results of the experiments made for determining the changes in the number of vibrations of our pendulum produced by varying pressures, and hence the correction necessary to reduce experiments made at any pressure to a vacuum. These results, as given by each separate experiment, are contained in the following Table :—

TABLE II. *Experiments for determining the number of vibrations made by Kater’s invariable pendulum at different pressures.*

No. of experiment.	Full atmospheric pressure.			About 25 inches.			About 20 inches.		
	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.
	°	inches.		°	inches.		°	inches.	
I.	47.84	30.052	86013.60	45.82	24.632	86014.57	47.66	19.895	86015.87
II.	47.77	30.112	86013.76	45.54	24.634	86014.70	47.21	19.852	86015.99
III.	46.33	30.182	86013.82	46.03	24.620	86014.71	51.04	19.902	86016.01
IV.	46.25	29.938	86014.02	47.19	24.599	86014.49	48.00	19.914	86016.07
V.	47.72	30.460	86013.58	51.01	24.651	86014.60	46.47	19.921	86016.10
VI.	47.91	29.498	86013.64	49.84	24.646	86014.70	49.00	19.864	86015.94
VII.	48.43	29.582	86013.98						
VIII.	47.76	29.328	86014.40						
IX.	45.09	30.202	86013.90						
X.	46.19	30.011	86013.99						
XI.	47.50	30.107	86013.61						

No. of experiment.	About 15 inches.			About 10 inches.			Between 7 and 8 inches.		
	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.
	°	inches.		°	inches.		°	inches.	
I.	43.47	14.563	86017.91	50.75	9.998	86019.65	49.37	7.586	86021.61
II.	44.30	14.567	86017.84	45.54	9.868	86019.29	54.11	7.491	86021.41
III.	50.07	14.532	86018.20	47.13	9.900	86019.43	51.29	7.303	86021.30
IV.	51.71	14.680	86017.95	48.24	9.870	86019.60	50.06	7.554	86021.44
V.	47.09	14.575	86018.00	49.01	10.015	86019.35	53.42	7.601	86021.19
VI.	46.88	14.499	86017.95	51.77	10.064	86019.51	48.73	7.384	86021.58
VII.	54.33	14.555	86017.93						

No. of experiment.	Between 5 and 6 inches.			Between 4 and 5 inches.			Between 3 and 4 inches.		
	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.
	°	inches.		°	inches.		°	inches.	
I.	52.13	5.461	86022.10	51.07	4.241	86022.48	51.34	3.144	86023.03
II.	47.86	5.420	86022.11	51.17	4.245	86022.71	50.95	3.155	86022.90
III.	48.90	5.510	86021.97	54.06	4.440	86022.69	50.80	3.266	86022.94
IV.	51.30	5.403	86021.84	50.39	4.530	86022.69	52.09	3.204	86022.70
V.	49.63	5.390	86022.19	49.84	4.107	86022.37	54.16	3.170	86022.71
VI.	54.17	5.489	86022.15	54.77	4.298	86022.54	53.70	3.104	86022.95

TABLE II. (continued).

No. of experiment.	Between 2 and 3 inches.			Between 1 and 2 inches.			Below 1 inch.		
	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.	Temp.	Pressure.	Vibrations.
	°	inches.		°	inch.		°	inch.	
I.	51.50	2.373	86023.23	60.97	1.393	86023.35	62.76	0.472	86023.26
II.	48.93	2.462	86023.09	59.47	1.416	86023.24	60.85	0.431	86023.47
III.	55.55	2.501	86023.05	58.32	1.411	86023.04	60.79	0.444	86023.60
IV.	54.76	2.389	86023.21	61.15	1.430	86023.45	61.57	0.451	86023.74
V.	52.38	2.417	86023.08	60.83	1.471	86023.65	62.36	0.425	86023.79
VI.	54.14	2.451	86023.23	57.58	1.319	86023.17	60.77	0.389	86023.31
VII.	49.72	0.427	86023.55

TABLE III. Mean results of Pressure-experiments.

Mean pressure. inches.		Mean number of vibrations per diem.		Mean pressure. inches.		Mean number of vibrations per diem.	
I.	0.434	86023.53	VII.	7.486	86021.42
II.	1.407	86023.32	VIII.	9.953	86019.47
III.	2.432	86023.15	IX.	14.569	86017.97
IV.	3.174	86022.87	X.	19.891	86016.00
V.	4.310	86022.58	XI.	24.630	86014.63
VI.	5.445	86022.06	XII.	29.952	86013.85

while Table III. contains the resulting means for the different sets, as specified in paragraph 4.

The experiments made at a full atmospheric pressure are the same as those given previously in connexion with the temperature-experiments, but they are here repeated for the sake of comparison. Their mean temperature being 47°·16, the whole of the other experiments has been reduced to the same temperature by means of the coefficient adopted in accordance with our preceding statement.

The results as given in Table III. do not require any special remarks. It will be seen that the resistance of the air to the motion of a pendulum, as measured by the number of its vibrations, increases very slowly up to 7 or 8 inches of pressure; a more energetic action is exerted up to about 20 inches, and after that point the resistance increases very slowly up to the full atmospheric pressure.

This behaviour is represented in a more impressive manner on the accompanying curves. One of them, marked A, shows simply the resulting number of vibrations at the given pressures, which latter form the abscissæ, while the former are the ordinates. The second curve, B, is derived from A, by assuming the whole correction necessary to reduce the pendulum observations made in air to a vacuum as unity, and expressing the correction for intermediate pressures as fractions. The ordinate representing unity has been divided into forty parts, each representing 0.025, enabling us to represent the correction to three decimals with great precision.

VI. "Additional Observations on *Hydrogenium*." By THOMAS GRAHAM, F.R.S., Master of the Mint. Received June 10, 1869.

From the elongation of a palladium wire, caused by the occlusion of hydrogen, the density of hydrogenium was inferred to be a little under 2. But it is now to be remarked that another number of half that amount may be deduced with equal probability from the same experimental data. This double result is a consequence of the singular permanent shortening of the palladium wire observed after the expulsion of hydrogen. In a particular observation formerly described, for instance, a wire of 609·14 millims. increased in length to 618·92 millims. when charged with hydrogen, and fell to 599·44 millims. when the hydrogen was extracted. The elongation was 9·78 millims., and the absolute shortening or retraction 9·7 millims., making the extreme difference in length 19·48 millims. The elongation and retraction would appear, indeed, to be equal in amount. Now it is by no means impossible that the volume added to the wire by the hydrogenium is represented by the elongation and retraction taken together, and not by the elongation alone, as hitherto assumed. It is only necessary to suppose that the retraction of the palladium molecules takes place the moment the hydrogen is first absorbed, instead of being deferred till the latter is expelled; for the righting of the particles of the palladium wire (which are in a state of excessive tension in the direction of the length of the wire) may as well take place in the act of the absorption of the hydrogen as in the expulsion of that element. It may indeed appear most probable in the abstract that the mobility of the palladium particle is determined by the first entrance of the hydrogen. The hydrogenium will then be assumed to occupy double the space previously allotted to it, and the density of the metal will be reduced to one half of the former estimate. In the experiment referred to the volume of hydrogenium in the alloy will rise from 4·68 per cent. to 9·36 per cent., and the density of hydrogenium will fall from 1·708 to 0·854, according to the new calculation. In a series of four observations upon the same wire, previously recorded, the whole retractions rather exceeded the whole elongations, the first amounting to 23·99 millims., and the last to 21·38 millims. Their united amount would justify a still greater reduction in the density of hydrogenium, namely to 0·8051.

The first experiment, however, in hydrogenating any palladium wire appears to be the most uniform in its results. The expulsion of the hydrogen afterwards by heat always injures the structure of the wire more or less, and probably affects the regularity of the expansion afterwards in different directions. The equality of the expansion and the retraction in a first experiment appears also to be a matter of certainty. This is a curious molecular fact of which we are unable as yet to see the full import. In illustration, another experiment upon a pure palladium wire

may be detailed. This wire, which was new, took up a full charge of hydrogen, namely 956·3 volumes, and increased in length from 609·585 to 619·354 millims. The elongation was therefore 9·769 millims. With the expulsion of the hydrogen afterwards, the wire was permanently shortened to 600·115 millims. It thus fell 9·470 millims. below its normal or first length. The elongation and retraction are here within 0·3 millim. of equality. The two changes taken together amount to 19·239 millims., and their sum represents the increase of the wire in length due to the addition of hydrogenium. It represents a linear expansion of 3·205 on 100, with a cubic expansion of 9·827 on 100. The composition of the wire comes to be represented as being,

	In volume.	
Palladium	100·000	or 90·895
Hydrogenium	9·827	or 9·105
	<hr/>	
	109·827	or 100·000

The specific gravity of the palladium was 12·3, the weight of the wire 1·554 grm., and its volume 0·126 cub. centim. The occluded hydrogen measured 120·5 cub. centims. The weight of the same would be 0·0108 grm., and the volume of the hydrogenium 0·012382 cub. centim. (100 : 9·827 :: 0·126 : 0·01238). The density of the hydrogenium is therefore

$$\frac{0·0108}{0·01238} = 0·872.$$

This is a near approach to the preceding result, 0·854. Calculated on the old method, the last experiment would give a density of 1·708.

It was incidentally observed on a former occasion that palladium alloyed with silver continues to occlude hydrogen. This property is now found to belong generally to palladium alloys, when the second metal does not much exceed one half of the mixture. These alloys are all enlarged in dimensions when they acquire hydrogenium. It was interesting to perceive that the expansion was greater than happens to pure palladium (about twice as much), and that, on afterwards expelling the hydrogen by heat, the fixed alloy returned to its original length *without any further shortening of the wire*. The embarrassing retraction of the palladium has, in fact, disappeared.

The fusion of the alloys employed was kindly effected for me by Messrs. Matthey and Sellon, when the proportion of palladium was considerable, by the instrumentality of M. Deville's gas-furnace, in which coal-gas is burned with pure oxygen—or by means of a coke-furnace when the metals yielded to a moderate temperature. The alloy was always drawn out into wire if possible, but if not sufficiently ductile, it was extended by rolling into the form of a thin ribbon. The elongation caused by the addition of hydrogenium was ascertained by measuring the wire or ribbon stretched over a graduated scale, as in the former experiments.

1. *Palladium, Platinum, and Hydrogenium*.—Palladium was fused with platinum, a metal of its own class, and gave an alloy consisting, according to analysis, of 76·03 parts of the former and 23·97 parts of the latter. This alloy was very malleable and ductile; its specific gravity was 12·64. Like pure palladium, it absorbed hydrogen, evolved on its surface in the acid fluid of the galvanometer, with great avidity.

A wire 601·845 millims. in length (23·69 inches) was increased to 618·288 millims., on occluding 701·9 volumes of hydrogen gas measured at 0° C. and 0·76 barom. This is a linear elongation of 16·443 millims. (0·6472 inch), or 2·732 on a length of 100. It corresponds with a cubic expansion of 8·423 volumes on 100 volumes; and the product may be represented—

	In volume.	
Fixed metals	100·000 or	92·225
Hydrogenium	8·423 or	7·775
	<hr/>	<hr/>
	108·423 or	100·000

The elements for the calculation of the density of hydrogenium are the following, the assumption being made as formerly, that the metals are united without condensation:—

Original weight of the wire 4·722 grms.

Original volume of the wire 0·373 cub. centim.

Volume of the hydrogen extracted 264·5 cub. centims.

Weight of the hydrogen extracted, by calculation, 0·0237 grm.

The volume of the hydrogenium will be to the volume of the wire (0·373 cub. centim.) as 100 is to 8·423—that is, 0·03141 cub. centim. Finally, dividing the weight of the hydrogenium by its bulk, 0·0237 by 0·03141, the density of hydrogenium is found to be 0·7545.

On expelling all hydrogen from the wire at a red heat, the latter returned to its first dimensions as exactly as could be measured. The platinum present appears to sustain the palladium, so that no retraction of that metal is allowed to take place. This alloy therefore displays the true increase of volume following the acquisition of hydrogenium, without the singular complication of the retraction of the fixed metal. It now appears clear that the retraction of pure palladium must occur on the first entrance of hydrogen into the metal. The elongation of the wire due to the hydrogenium is negatived thereby to the extent of about one half, and the apparent bulk of the hydrogenium is reduced to the same extent. Hydrogenium came in consequence to be represented of double its true density.

The compound alloy returns to its original density (12·64) upon the expulsion of the hydrogen, showing that hydrogen leaves without producing porosity in the metal. No absorptive power for vapours, like that of charcoal, was acquired.

A wire of the present alloy, and another of pure palladium, were charged

with hydrogen, and the diameters of both measured by a micrometer. The wire of alloy increased sensibly more in thickness than the pure palladium, about twice as much; the reason is, that the latter while expanding retracts in length at the same time. The expansion of both wires may be familiarly compared to the enlargement of the body of a leech on absorbing blood. The enlargement is uniform in all dimensions with the palladium-platinum alloy; the leech becomes larger, but remains symmetrical. But the retraction in the pure palladium wire has its analogy in a muscular contraction of the leech, by which its body becomes shorter but thicker in a corresponding measure.

The same wire of palladium and platinum charged a second time with hydrogen, underwent an increase in length from 601·845 to 618·2, or sensibly the same as before. The gas measured 258·0 cub. centims., or 619·6 times the volume of the wire. The product may be represented as consisting of

	By volume.
Fixed metals	92·272
Hydrogenium	7·728
	<hr/>
	100·000

The density of hydrogenium deducible from this experiment is 0·7401. The mean of the two experiments is 0·7473.

2. *Palladium, Gold, and Hydrogenium*.—Palladium fused with gold formed a malleable alloy, consisting of 75·21 parts of the former and 24·79 parts of the latter, of a white colour, which could be drawn into wire. Its specific gravity was 13·1. Of this wire 601·85 millims. occluded 464·2 volumes of hydrogen with an increase in length of 11·5 millims. This is a linear elongation of 1·91 on 100, and a cubic expansion of 5·84 on 100. The resulting composition was therefore as follows :—

	In volume.
Alloy of palladium and gold	100 or 94·48
Hydrogenium	5·84 or 5·52
	<hr/>
	105·84 100·00

- The weight of the wire was 5·334 grms.
- The volume of the wire was 0·4071 cub. centim.
- The volume of hydrogen extracted, 189·0 cub. centims.
- The weight of the hydrogen, 0·01693 grm.
- The volume of the hydrogenium, 0·02378 cub. centim.
- Consequently the density of the hydrogenium is 0·711.

The wire returned to its original length after the extraction of the hydrogen, and there was no retraction.

The results of a second experiment on the same wire were almost identical with the preceding.

The elongation on 601·85 millims. of wire was 11·45 millims., with the occlusion of 463·7 volumes of hydrogen. This is a linear expansion of

1.902 on 100, and a cubic expansion of 5.81 on 100. The volume of hydrogen gas extracted was 188.8 cub. centims., of which the weight is 0.016916 grm. The volume of the hydrogenium was 0.02365 cub. centim., that of the palladium-gold alloy being 0.4071 cub. centim. Hence the density of the hydrogenium is 0.715.

In a third experiment made on a shorter length of the same wire, namely 241.2 millims., the amount of gas occluded was very similar, namely 468 volumes, and was not increased by protracting the exposure of the wire for the long period of twenty hours. There can be little doubt, then, of the uniformity of the hydrogenium combination, the volume of gas occluded in the three experiments being 464.2, 463.7, and 468 volumes. The linear expansion was 1.9 on 100 in the third experiment, and therefore similar also to the preceding experiments.

The hydrogenium may be supposed to be in direct combination with the palladium only, as gold by itself shows no attraction for the former element. In the first experiment the hydrogenium is in the proportion of 0.3151 to 100 palladium and gold together. This gives 0.3939 hydrogenium to 100 palladium; while a whole equivalent of hydrogenium is 0.939 to 100 palladium*. The hydrogenium found is by calculation 0.4195 equivalent, or 1 equivalent hydrogenium to 2.383 equivalents palladium, which comes nearer to 2 equivalents of the former with 5 of the latter than to any other proportion.

To ascertain the smallest proportion of gold which prevents retraction, an alloy was made by fusing 7 parts of that metal with 93 parts of palladium, which had a specific gravity of 13.05. The button was rolled into a thin strip and charged with hydrogen by the wet method. An occlusion of 585.44 volumes of gas took place, with a linear expansion of 1.7 on 100. A retraction followed to nearly the same extent on afterwards expelling the hydrogen by heat.

With another alloy, produced by fusing 10 of gold with 90 of palladium, the occlusion of gas was 475 volumes, the linear expansion 1.65 on 100. The retraction on expelling the gas afterwards was extremely slight. To nullify the retraction of the palladium, about 10 per cent. of gold appears therefore to be required in the alloy.

Another alloy of palladium of sp. gr. 13.1, and containing 14.79 per cent. of gold, underwent no retraction on losing hydrogen, as already stated.

The presence of so much gold in the alloy as half its weight did no materially reduce the occluding power of the palladium. Such an alloy was capable of holding 459.9 times its volume of hydrogen, with a linear expansion of 1.67 per cent.

3. *Palladium, Silver, and Hydrogenium*.—The occluding power of palladium appeared to be entirely lost when that metal was alloyed with much more than its own weight of any fixed metal. Palladium alloys con-

* $H=1$; $Pd=106.5$.

taining 80, 75, and 70 per cent. of silver occluded no hydrogen whatever.

With about 50 per cent. of silver, palladium rolled into a thin strip occluded 400·6 volumes of hydrogen. It expanded 1·64 part in 100 in length, and returned to its original dimensions without retraction upon the expulsion of the gas. The specific gravity of this silver-palladium alloy was 11·8; the density of the hydrogenium 0·727.

An alloy which was formed of 66 parts of palladium and 34 parts of silver had the specific gravity 11·45. It was drawn into wire and found to absorb 511·37 volumes of hydrogen. The length of the wire increased from 609·601 to 619·532 millims. This is a linear elongation of 1·629 on 100, or cubic expansion of 4·97 on 100. The weight of the wire was 3·483 grms., its volume 0·3041 cub. centim. The absolute volume of occluded hydrogen was 125·1 cub. centims., of which the weight is 0·01120896. The volume of the hydrogenium was 0·015105 cub. centim. The resulting density of hydrogenium is 0·742.

In a repetition of the experiment upon another portion of the same wire, 407·7 volumes of hydrogen were occluded, and the wire increased in length from 609·601 millims. to 619·44 millims. This is a linear expansion of 1·614 part on 100, and a cubic expansion of 4·92 on 100. The absolute volume of hydrogen gas occluded was 124·0 cub. centims., and its calculated weight 0·011111 gram. The volume of the hydrogenium being 0·1496 cub. centim., the density of hydrogenium indicated is 0·741. The two experiments are indeed almost identical. The wire returned in both experiments to its original length exactly after the extraction of the gas.

4. *Palladium, Nickel, and Hydrogenium*.—The alloy, consisting of equal parts of palladium and nickel, was white, hard, and readily extensible. Its specific gravity was 11·22. This alloy occluded 69·76 volumes of hydrogen, with a linear expansion of 0·2 per cent. It suffered no retraction below its normal length on the expulsion of the gas by heat.

An alloy of equal parts of *bismuth* and palladium was a brittle mass that did not admit of being rolled. It occluded no hydrogen, after exposure to that gas as the negative electrode in an acid fluid for a period of 18 hours. It seems probable that malleability and the colloid character, which are wanting in this bismuth alloy, are essential to the occlusion of hydrogen by a palladium alloy.

An alloy of 1 part of *copper* and 6 parts of palladium proved moderately extensible, but absorbed no sensible amount of hydrogen. The metallic laminæ which remain on digesting this alloy in hydrochloric acid, and which were found by M. Debray to be a definite alloy of palladium and copper (Pd Cu), exhibited no sensible occluding power.

The conclusions suggested as to the density of hydrogenium, by the compound with palladium alone and by the compounds with palladium alloys, are as follows:—

	Density of Hydrogenium observed.
When united with palladium	0·854 to 0·872
When united with palladium and platinum	0·7401 to 0·7545
When united with palladium and gold	0·711 to 0·715
When united with palladium and silver	0·727 to 0·742

The results, it will be observed, are most uniform with the compound alloys, in which retraction is avoided, and they lie between 0·711 and 0·7545. It may be argued that hydrogenium is likely to be condensed somewhat in combination, and that consequently the smallest number (0·711) is likely to be the nearest to the truth. But the mean of the two extreme numbers will probably be admitted as a more legitimate deduction from the experiments on the compound alloys, and 0·733 be accepted provisionally as the approximate density of hydrogenium.

I have the pleasure to repeat my acknowledgments to Mr. W. C. Roberts for his valuable assistance in this inquiry.

Could the density of hydrogenium be more exactly determined, it would be interesting to compare its atomic volume with the atomic volumes of other metals. With the imperfect information we possess, one or two points may be still worthy of notice. It will be observed that palladium is 16·78 times denser than hydrogenium taken as 0·733, and 17·3 times denser than hydrogenium taken as 0·711. Hence, as the equivalent of palladium is 106·5, the atomic volume of palladium is 6·342 times greater than the atomic volume of hydrogenium having the first density mentioned, and 6·156 greater with the second density. To give an atomic volume to palladium exactly six times greater than that of hydrogenium, the latter element would require to have the density 0·693.

Taking the density of hydrogenium at 0·7, and its atomic volume equal to 1, then the following results may be deduced by calculation. The atomic volume of lithium is found to be 0·826; or it is less even than that of hydrogenium (1). The atomic volume of iron is 5·026, of magnesium 4·827, of copper 4·976, of manganese 4·81, and of nickel 4·67. Of these five metals, the atomic volume is nearly 5 times that of hydrogenium. Palladium has already appeared to be nearly 6 times. The atomic volume of aluminium on the same scale is 7·39, of sodium 16·56, and of potassium 31·63.

VII. "Spectroscopic Observations of the Sun" (continued). By Lient. J. HERSCHEL, in a Letter addressed to W. HUGGINS, F.R.S., Communicated by Mr. HUGGINS. Received June 10, 1869.

Bangalore, May 7, 1869.

MY DEAR SIR,—After what I wrote to you last week you will scarcely be surprised to hear again from me on the same subject; and indeed I feel in some measure bound to communicate without delay results of fur-

ther and more successful observations. Should you think fit to publish them, I hope you will do so, as I cannot command the necessary leisure to follow them up myself to their legitimate conclusion.

On the 3rd instant I learnt (as I informed you) that the spectrum of the solar envelope was visible with the spectroscope at my command, apparently without difficulty. On the following day I saw the same phenomena, and was enabled to form a fair mental picture of the distribution of the luminous regions surrounding the sun. Two very fine prominences were particularly examined, one of which was evidently a large cloud floating 1' to 2' above the surface.

On the 5th, while traversing over a new prominence to learn its shape and dimensions, I became aware of a fourth line in the neighbourhood of G. Its position was determined without difficulty with reference to the rest of that crowded group of solar lines. It was identical with the thick line at 2796 of Kirchhoff's chart. I have seen the same line repeatedly since, and have satisfied myself of the identity stated.

It rained pretty heavily on the night of the 5th; and the next morning I was disappointed by seeing no remarkable prominences, and but faint indications, in many parts, of the solar envelope. In the afternoon, however, the air, I suppose, being clearer, I could again see the luminous spectrum in nearly every part. But there were very few elevated masses. Having traversed round the whole circumference, I returned, after perhaps an hour's search, to examine again a moderately striking elevation which I had noticed at setting out; and for this purpose I directed the slit as a tangent to the surface at the place—the most favourable position for getting a good view of the lines.

Immediately I remarked that the red line was very brilliant, and glancing up the spectrum saw that the orange and blue lines were also much more intense than usual. My eye was next caught by a fainter red flash, which I soon succeeded in seeing more steadily. Concluding that it was the line which Mr. Lockyer had seen "occasionally," I only staid to estimate its position, and proceeded to bring the violet end into the field to have another look at the line in that region and to see F to better advantage. In so doing I noticed another line (the sixth) between F and G.

Before going any further I must take the liberty of christening these lines for reference. Leaving α and β for C and F if wanted, I would call the orange line near D δ , the violet one γ , the red line near C ϵ , and the last mentioned ζ . The solar bright-line series is then as follows:—

$\alpha = C$	Kirchhoff's	694
$\beta = F$	„	2080
δ near D	„	1014 very nearly.
γ near G	„	2796
ϵ near C	„	655 about.
ζ between F and G	„	2596 nearly.

The position of ϵ is estimated ; for there are no visible solar lines between B and C (in my spectroscope), and because by the time I was ready to go back and measure it (ζ having already faded) it was no longer to be seen ; and as the other lines had visibly decreased in brilliancy, I could only conclude that I had been a fortunate witness of the effects of a violent and spasmodic action or eruption of vapour lasting only a few minutes. Nor did I see any recurrence of this spectacle, though I watched for some time.

I should here state that I was looking at this time at a very low stratum, and that the line γ is rarely visible except quite close to the sun's limb ; F also is not generally brilliant, except near the limb. δ is never (so far as I have seen) so brilliant as either C or F.

I have said that these appearances are to be seen without any defence from excessive light ; and this is strictly true, for I have seen them readily, even when the paper cap which I at first used over the object-glass was removed ; but as a precaution against the heating-effects of the sun's image, I have latterly used metal diaphragms, one of $\frac{1}{2}$ -inch diameter, 12 inches, and a second of $\frac{1}{4}$ *-inch diameter, $1\frac{1}{2}$ to 2 inches distant from the slit. When these had been inserted I dispensed with any cover for the object-glass. I was in hopes that, by carefully stopping out all unnecessary light in this way, I should be able to dispense with a slit, and view the monochromatic images of a protuberance on the white background (so to speak) of the atmospheric illumination. But so far I have been disappointed. Nevertheless I still believe that whoever will go a step further and use a *red glass prism without a slit* will see the *actual* "red flames." [When writing my eclipse-report I was under the impression that the orange was the principal light (v. § 43).] This much of foundation I have for this belief, that I actually have seen the *form* of a solar cloud through a widely distended slit—not a luminous line of varying length and position, but a view such as you may obtain through a partly open shutter by moving the head slightly to and fro, only that the movement was in this case effected by a gentle pressure up and down of the telescope itself—a movement rendered possible by the absence of perfect rigidity of the instrument. In this way I could see clearly that the solar clouds were very similar to terrestrial ones, fleecy, irregularly shaped, and illuminated, &c., just as eclipses have told us they are. The opening through which I viewed them was about $\frac{1}{4}$ of a minute in width, and the height and length of the mass $1\frac{1}{2}$ and 3 minutes respectively, or thereabouts.

After this I need not describe the appearances of the lines, the less so as I fully expect that, once the ready visibility of these appearances becomes realized, numerous accounts of such eruptions as I saw yesterday, as well as of the real forms and appearances which they present, will be forthcoming from observers who can better spare the necessary daylight hours.

* This is too large.

May 8.—This morning I received, and read with deep interest, your article in the 'Journal of Science.' Had I received it a week ago, my note of the 3rd would have been differently worded. On the other hand, it is clear that some of the facts I have stated above are still legitimate subjects for communication, and will probably lead to further discoveries. The new lines may or may not have been since seen by Mr. Lockyer. In the former case the corroboration will be worth something—the more so as his secondary red line, as mentioned in Mr. Crookes's article in the January No. of the 'Journal of Science,' is apparently misplaced. This line, as also those I have called δ and ζ , does not appear to coincide with any known solar line. The elementary substances to which these belong remain yet to be declared. As for γ , I suppose the strong solar line to which it corresponds belongs to some known element.

I have not remarked any tendency in F to vary in width.

It cannot but be considered strange that no traces have been seen of M. Rayet's and Major Tennant's green lines. I have watched that part of the spectrum very closely on purpose, but, even where the four principal lines were more than ordinarily bright, I have failed to distinguish even the slightest fading of the strong magnesium lines, or of others in that neighbourhood. This *fading* invariably precedes the substitution of a bright for a dark line: thus, if the slit admits to view the tops of several adjacent prominences, the line F (for instance) is broken into detached bright portions, between which there is in each case a more or less complete hiatus, which may or may not amount to the original dark solar line. The dark line, being only a *less intense light*, is susceptible of all degrees of darkness, just as the bright line, being only a *more intense light*, may appear of all lower degrees of brightness. This intermediate condition between dark and bright is constantly to be recognized, more or less strongly marked, within the sun's border; but I cannot say I have seen a continuation of the bright line inwards. I often see this absence of the dark line γ when the light is not intense enough to show it as a "bright" line. So far, then, as regards the *magnesium* element I have strong negative evidence, which is strengthened by the consideration of the improbability that such a very marked group should not have been recognized by the above-mentioned observers if it was actually present, on the one hand, and that the element should have been represented by a single member only of this group, on the other. The confident way in which this metal has been accepted as recognized, by more than one speculator, seems to challenge question on the evidence.

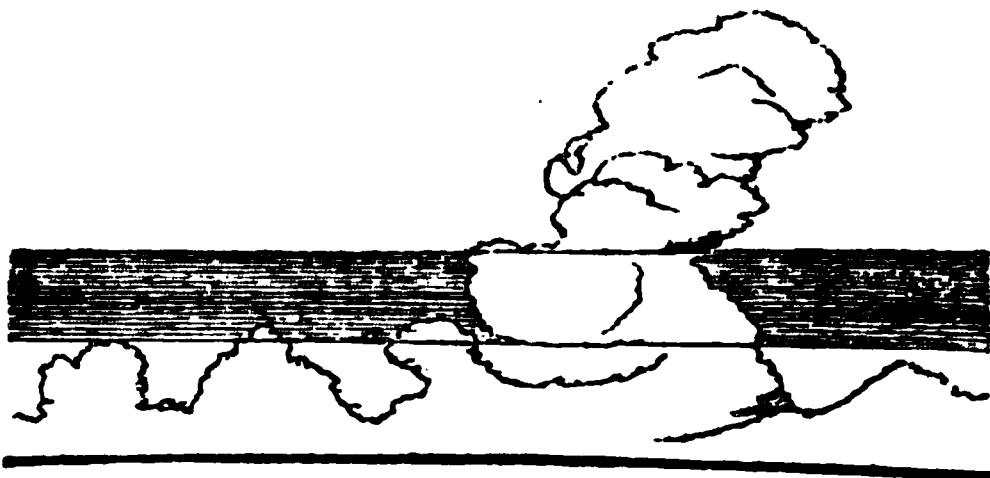
And I would take the present opportunity to remark that, after the studied avoidance (on my part) of such hasty conclusions, I have felt it rather hard to be set right where I had not erred, as in the case of the orange line. I never said that a line, apparently identical though it was with D, represented sodium. One writer, if I recollect right, made *me* responsible, not only for sodium and hydrogen, but for magnesium as well!

Neither word even *occurs* in my report, except incidentally in the words "a sodium-flame." The exceeding care which I observe in your own writings in this respect will, I hope, make it unnecessary for me to apologize for this protest.

To return : I do not question the presence of an element giving a green line ; the testimony of three observers must be taken as conclusive ; but I am slow to believe that either E or *b* is connected with it.

This afternoon I made a slight advance towards seeing the actual forms with greater comfort. Taking a hint from your figure (p. 216 of the above article), I introduced one of the compound prisms of the hand-spectroscopes into the eye-tube, thus increasing the dispersion by about one-fourth, which enabled me to open the slit a little wider. Finding a suitable prominence, I was able to examine it with an aperture of about 20" to 25". As it was not much more than 1' in height, I could, by a slight pressure one way or the other,

view the whole. It is of course wholly unimportant what the actual form was, but, for the sake of illustration, I attempt a drawing. It is not easy to convey the impression of a fleecy cloud such as



I saw. I looked at one or two others in the same way, and left off eventually quite satisfied that with a suitable battery the whole of any prominence or eruption might be seen with comfort (either the red, or the orange, or the blue, or any other principal image being examined at will) by limiting the field of view, and with it the unnecessary diffuse light, to the actual dimensions of the object. The portion of a cloud-shape which is due to one element will thus be artificially separated from the form which is due to another, and the regions or strata to which the various elements are confined will become known with certainty*.

It is unfortunately impossible for me to prosecute these researches any further. I have neither the leisure nor the opportunity to devise and use suitable instruments except at rare intervals, for which such discoveries will not wait.

Yours very truly,

J. HERSCHEL, Lt. R.E.

* As an instance of this kind, I may point to Captain Haig's observations with the hand-spectroscope. As this instrument *has no slit*, his "bands" mean the coloured repetitions of the line of sierra or low clouds fringing the moon's limb at the point, only that with so low a power, and amid the confusion of images, he did not recognize (apparently) the similarity of general outline of the differently coloured images. Hence the term "bands," which has misled at least one reviewer into inferring a slit, and thereby immensely overrating the scope of these instruments.

VIII. "On Jargonium, a new Elementary Substance associated with Zirconium." By H. C. SORBY, F.R.S. &c. Received June 4, 1869.

At the Soirée of the President of the Royal Society on March 6th, I exhibited various spectra, differing so much from those characteristic of any known substance, that I considered myself warranted in concluding that they were evidence of a new element. Since this may be studied to the greatest advantage in the jargons of Ceylon, it appeared to me that, like as the name zirconium has been adopted for the principal constituent of zircons, so that of jargonium would be very suitable for this constituent of jargons.

At the above-named Soirée I gave away a printed account of the objects I exhibited, and in this I said that the earth jargonia "is distinguished from zirconia and all other known substances by the following very remarkable properties. The natural silicate is almost, if not quite colourless, and yet it gives a spectrum which shows above a dozen narrow black lines, much more distinct than even those characteristic of salts of didymium. When melted with borax it gives a glassy bead, clear and colourless both hot and cold, and no trace of absorption-bands can be seen in the spectrum; but if the borax bead be saturated at a high temperature, and flamed, so that it may be filled with crystals of borate of jargonia, the spectrum shows four distinct absorption-bands, unlike those due to any other known substances" *.

I have since applied myself almost exclusively to this subject, hoping to have been able to communicate to the Royal Society a full account before the close of this session; but so much still remains to be done, that it is now impossible to give more than a brief outline of some of the more important facts. The delay has not been occasioned by any difficulty in proving it to be a new substance, but because its properties are so unique and have so much interest in connexion with physics that it appeared desirable to carefully examine all other known elements, in order to ascertain whether any exhibit analogous phenomena.

That jargonium is quite distinct from zirconium is proved not only by the spectra, but also by other facts. Both I and Mr. David Forbes have succeeded, by entirely different processes, in separating from jargons zirconia apparently quite free from jargonia, and jargonia nearly, if not quite, free from zirconia; and, even if the separation be not perfect, it is, at all events, more than sufficient to prove that they are distinct. They are certainly closely allied, and are deposited from borax blowpipe beads in microscopical crystals of the same general forms, quite unlike those characteristic of other known earths; but, beyond this, the difference is

* For the further history of this subject see Professor Church's papers, *Intellectual Observer*, 1866, vol. ix. p. 291, *Chemical News*, March 12 and 19, 1869, vol. xix. pp. 121 & 142, *Athenæum*, March 27; and also my own, *Chemical News*, vol. xix. p. 122, and *Athenæum*, April 3, 1869.

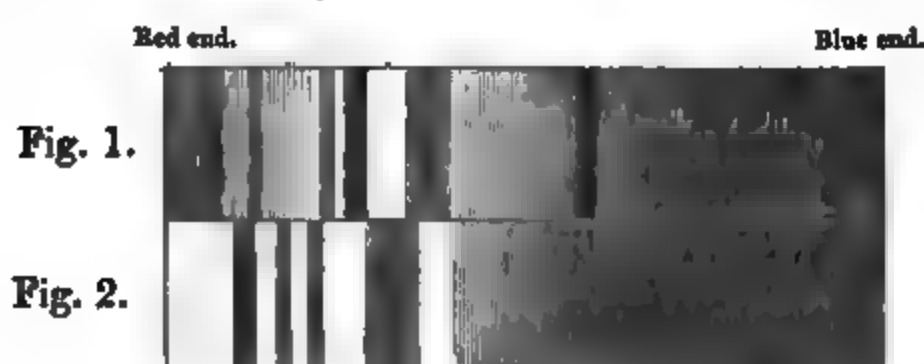
as great as that between any other two closely related elements. Judging from Mr. D. Forbes's analysis, kindly made at my request, and from a comparison of the spectra, the amount of jargonia in different jargons varies up to about 10 per cent. The entire or comparative absence from the zircons of Miask, Fredericksvärn, and various other localities, appears to explain some of the facts which led Svanberg* to conclude that zircons contain more than one earth. He was so far correct, but failed to establish the existence of any substance with special chemical or physical properties; and if, as is probable, the Norwegian zircons, which, according to his views, contain such a notable quantity of this supposed new earth as to have led him to give it the name *noria*, were from Fredericksvärn, and if the Siberian were from Miask, his *norium* cannot be looked upon as equivalent to my jargonium, which is almost or quite absent from those zircons.

The most remarkable peculiarity of jargonium is that its compounds may exist in no less than three different crystalline states, giving spectra which differ from one another as much as those of any three totally different elements which give the most striking and characteristic spectra. Several substances can be obtained in two physical states, giving different spectra: but usually only one of them is crystalline; the other is the vitreous or colloid condition. Crystalline minerals, coloured by oxide of chromium, do indeed show two types of spectra, but I am not aware that they ever both occur in the same mineral. In the case of jargonium, however, the three types of spectra are all met with in *crystalline* modifications of apparently the *same* compound.

The most characteristic test for jargonia is the spectrum of the borax blowpipe beads, seen with the spectrum-microscope, which enables us to detect it in zircons containing less than one per cent. As much of the earth or natural silicate as will completely dissolve should be melted in circular loops of platinum wire, about $\frac{1}{8}$ of an inch in diameter, with a mixture of borax and boric acid, and a very strong heat kept up till crystals begin to be deposited, owing to loss of the solvent by volatilization. On removing the beads from the flame they remain clear, and show a few acicular crystals, but give no absorption-bands in the spectrum. On reheating to a temperature just below very dull redness, they turn white, and so very opaque that no ordinary light will pass through them. When, however, a small and very bright image of the sun is formed in their centre, by means of an almost hemispherical condensing lens of $\frac{1}{8}$ inch diameter, and a cap placed over the object-glass, with a round hole less than the beads nearly in the focus, so as to prevent the passage of extraneous light, they are seen to be illuminated by transmitted light of about the same brilliancy as that of a bright cloud, so as to give an excellent spectrum, without being at all dazzling. In the case of beads containing jargonia, the spectrum differs completely according to the temperature at which the included crystals have

* Pogg. Ann. 1845, vol. lxx. p. 317.

been deposited. As already mentioned, a clear glassy bead gives no absorption-bands; and when the crystals are deposited at as low a temperature as possible, much below dull redness, and only just high enough to soften the borax, there may be scarcely any trace of bands; but, if a clear bead be quickly raised to a temperature very little below dull redness, it suddenly becomes opaque, and shows a spectrum with a number of narrow black absorption-bands (fig. 1). The most distinct is in the green, then one in the red, and one in the blue; and there are three fainter, one in the orange, and two in the green. On raising the temperature to bright redness all these bands vanish, and four others appear, none of which coincides with the former (fig. 2). Three are situated in the red and orange, and



one in the green, so as to give a spectrum of very different general character. In this state the bead is a pale straw-colour, and not, as before, almost white. In the case of nearly pure jargonium, the bead should not be more than $\frac{1}{8}$ of an inch thick, or else it would be too opaque. Pure zirconia treated in the same manner gives no bands whatever in any condition; the bead is quite white, and sufficiently transparent when two or three times as thick as just named.

It might be thought that the three different spectra thus briefly described were due to different compounds, if it were not that there is a similar series in the case of the natural crystalline silicate. Some of the jargons of Ceylon have a specific gravity very little inferior to that of pure zircons (4.70), and contain very little jargonium; but those of low gravity (4.20 or thereabouts) contain perhaps nearly 10 per cent., in a form which gives scarcely any trace of absorption-bands. On keeping such a specimen at a bright red heat for some time, the specific gravity increases from about 4.20 to 4.60. Judging from the imperfect data now known, this indicates that the volume of the silicate of jargonium is reduced to about one-half; the hardness becomes somewhat greater, and, when examined with the spectrum-microscope, the spectrum is found to be entirely changed. Instead of a mere trace of bands, a spectrum is seen with thirteen narrow black lines and a broader band, more remarkable than that of any clear transparent substance with which I am acquainted. No such changes occur in the case of zircons free from jargonium, like those from Miask, Siberia; there is no increase in the specific gravity, and no absorption-bands are developed, and, as a general rule, the increase varies

simply and directly as the amount of jargonia which passes from one state into the other. Zircons in their natural condition from various localities contain a very variable absolute and relative amount of these two modifications of jargonia, and there seems good reason to believe that this difference in physical state may materially assist us in determining the temperature at which certain rocks have been formed. I have also met with one example of the third form of spectrum. A brown-red zircon from Ceylon was so dark in one part as to be quite opaque, and therefore I do not know what the original spectrum might have been. On heating it to redness, the whole became a clear pale green; and, without examination with the spectroscope, no one would have suspected any difference between the different portions. That which was originally a pale brown-red then showed the same spectrum as that usually developed by heat, whilst that which was originally very dark showed an entirely different spectrum, corresponding exactly with that of the borate deposited in blowpipe beads at a medium temperature. It also corresponds in general character, but not in detail, with that of the blue spinels from Ceylon, which must, I think, contain a small quantity of jargonia. That part of the zircon which gave this spectrum appears to have had the same remarkably low specific gravity of about 4.0, both before and after ignition, as though the volume of the silicate of jargonia in this state were even greater than in that which gives no bands. All these spectra, due to jargonium, are of a very marked character, and quite unlike those due to any other element in similar conditions.

The alteration produced in jargons by heat is, to some slight extent, analogous to what occurs on heating carbonate of lime in the state of arragonite; but, instead of changing into an opaque mass of minute crystals of another form of the carbonate (calcite), which has a less specific gravity, is less hard, and does not give a different spectrum, they are still as simple and transparent crystals as at first; the specific gravity and hardness are increased, and the spectrum is entirely changed. Iodide of mercury is an excellent illustration of an alteration in the spectrum, due to a change in crystalline form produced by heat; but still the facts differ most materially from those described, and there are only two modifications—the yellow and the scarlet. The existence of three crystalline modifications is similar to what occurs in titanitic acid. Anatase, Brookite, and rutile have distinct crystalline forms, but they do not differ much in specific gravity, and their spectra present no characteristic differences. On the whole, the different states of carbon (charcoal, graphite, and diamond) are perhaps the best illustration of the existence of three different conditions in the same substance, since they differ materially in specific gravity and optical characters, one being black, the other having a metallic lustre, and the third being transparent and colourless; but these are variations of the element itself, and not, as in the case of jargonium, modifications of its compounds. So far as I am aware, there is indeed no substance which shows strictly comparable facts.

There cannot then, I think, be any doubt whatever that jargonium is not only a new elementary substance, but is also one likely to throw much light on several important physical questions. By the time that the Society resumes its meetings, I trust that I shall be able to send a complete account of the whole of my investigations, including such facts connected with other substances as may serve to illustrate the very peculiar properties of this hitherto unrecognized element.

POSTSCRIPT. Received June 18, 1869.

I here subjoin a brief account of the methods employed by Mr. David Forbes* and myself in separating zirconia and jargonia from one another. He separated apparently pure zirconia by means of strong hydrochloric acid, which dissolved the chloride of jargonium, but left chloride of zirconium undissolved; and obtained the approximately pure jargonia by adding to the solution excess of ammonia, and then considerable excess of tartaric acid, which left most of the tartrate of jargonia insoluble, but dissolved what may turn out to be a mixture of zirconia and jargonia with a third substance, not yet sufficiently studied—perhaps Svanberg's noria. My own analysis was only qualitative. I fused powdered jargon with several times its weight of borax, which gave a perfectly clear glass, completely soluble in dilute hydrochloric acid. After separating the silica in the usual manner, a slight excess of ammonia was added to the hydrochloric-acid solution of the earths, and then some oxalic and hydrochloric acids, which left undissolved apparently pure zirconia that had passed into an imperfectly soluble state. To the solution so much ammonia was added as to give a very copious precipitate, but yet to leave the solution with a very decided acid reaction. After removing the precipitate, which was chiefly oxalate of zirconia, almost or quite free from jargonia, excess of ammonia was added to the solution, and the washed precipitate digested in dilute hydrochloric acid, to remove peroxide of iron. The insoluble portion must have been approximately pure oxalate of jargonia, for it gave the characteristic spectra described below in remarkable perfection. Though this method succeeded far better than I anticipated, I do not yet understand the exact conditions requisite to ensure success, and have been prevented by absence from home from making further experiments.

IX. "Solar Radiation." By J. PARK HARRISON, M.A. Communicated by Prof. STOKES, Sec. R.S. Received June 12, 1869.

In a communication which the author had the honour of making to the

* Chemical News, June 11, 1869, vol. xix. p. 277.

Royal Society in 1867 *, it was shown, from observations of the black-bulb thermometer and Herschel's actinometer, that maximum effects of solar radiation occur at Greenwich, on the average, some weeks after the summer solstice, and about two hours after mid-day, when the atmosphere would appear to be charged with a considerable amount of vapour.

These results accord with the fact that the highest readings of the solar thermometer are met with in India in districts of great relative humidity †, the explanation of the phenomenon being, as the author ventured to suggest in the paper above alluded to, that an increase of insolation is produced by radiation from cloud and visible vapour.

During the two years which have elapsed since the spring of 1867, whenever the state of the sky and other circumstances permitted, special observations have been made for the purpose of ascertaining with greater certainty the nature of the relation between insolation and humidity.

Before proceeding to state results, it will afford additional proof that a connexion between the phenomena really exists, if a passage in the appendix to a work by the late Principal of St. Andrews, until very recently overlooked, is quoted in support of the fact. Mr. Forbes, writing some years ago, employs much the same words that were used in the paper above referred to:—"Cloudy weather, if the sun be not itself greatly obscured, apparently increases the effect of solar radiation" ‡.

The action, however, does not appear to be confined to days on which there is *visible* cloud; for even on cloudless days (so called) very high readings of solar radiation seem to be due to the presence of opalescent vapour, which can be easily detected if the hand or some other screen is held for a few minutes before the sun.

Thus, on May 2, 1868, at 1^h 30^m, solar radiation appearing to be relatively intense, on raising a screen white glare was observed around the sun, and the tint of the sky, which had previously appeared a fair blue, was found, more especially in the south, to be very pale.

But the most interesting result of this series of observations is the discovery that an apparent increase of solar radiation occurs as the sun enters a white cloud of sufficient tenuity to allow free passage for its rays.

In October 1867, at 2^h, whilst attentively watching a solar thermometer, a sudden rise was observed to take place, upon which, the sun being immediately screened, it was found that it had entered the bright border of a cumulus.

On May 11, 1868, at 22^h 40^m, as a very light cloud approached the sun, which was shining in blue sky, the mercury rose 4°, and in 30 seconds 3° more as it entered the white cloud.

On the same day, at 23^h, the reading of the solar thermometer was 101°F. when the sun was in the midst of cirri, but it fell in 3 minutes 9° when

* Proc. Roy. Soc., Feb. 1867.

† Proc. Roy. Soc., March 1865.

‡ Travels through the Alps of Savoy, App. III. p. 417.

well free again; then rose 6° as light cloud again crossed it. The air was perfectly still.

On May 15, 1868, the highest reading of the solar thermometer for the day occurred at 2^h 17^m, just as the sun entered the skirts of a cloud.

On July 21, 1868, at 2^h, the maximum of the day (128° F.) was reached when the sun was shining in a patch of pale sky surrounded with white cumuli, some of which were within one or two diameters of its disk.

To mention one more example amongst numerous others which might be cited; on Aug. 3, 1868, at 0^h 40^m, under an apparently clear sky, the solar thermometer registering 112° , and the temperature of shade 82° , in two minutes insolation increased to 125° , whilst the temperature of shade rose 0.3 only; on examining the sky in the neighbourhood of the sun, white cirri were detected crossing its disk.

Light cloud and opalescent vapour having been thus found, when in the direction of the sun, to intensify the effects of solar radiation, a series of experiments was commenced with circular screens of various sizes, to discover, if possible, *the distance* to which the effects of bright glare and light vapoury cloud extended round the sun.

The observations were made when the sun's altitude was between 30 and 50 degrees. All the screens were placed at a uniform distance of six inches from the bulb of a solar thermometer, $\frac{1}{4}$ in. in diameter, coated with China ink, and laid on a small piece of dark oak about two inches by ten inches on grass. The bulb of the thermometer was not covered with an exhausted globe. The mean results of the experiments were as follows* :—

1. A screen $\frac{1}{2}$ in. in diameter reduced the difference of the readings of the black-bulb thermometer and a thermometer in the shade, four yards distant, by one-third.

2. A screen $2\frac{1}{2}$ ins. in diameter reduced the difference by two-thirds.

On reversing the experiment, converse results were obtained, *e. g.*

The rays of the sun, after passing through a circular aperture $2\frac{1}{2}$ ins. in diameter in a 12-in. screen, were made to fall on the bulb of the solar thermometer, when the readings were found to equal in value those obtained when the instrument was entirely exposed†.

And no difference was noticed when the black-bulb thermometer was screened from the rest of the sky by a double cover of mill-board placed tent-wise over it.

* Similar results were obtained when the solar thermometer was laid upon short grass, in the afternoon, when the dew was off the ground.

With the instrument freely suspended 6 in. above the grass, the readings showed a proportionate fall.

† In the above experiments, it is evident that the whole of the results were not due to direct radiation or reflection. Account must be taken of the greater or less distance of the heated surface of the ground, and of the hot air in contact with it, from the bulb of the solar thermometer.

Results of an equally negative kind were obtained in the case of other experiments which were made with the object of detecting heat in the light reflected from sky and cloud not in the direction of the sun.

A black-bulb thermometer, after having been placed for some time in a dark room, was then exposed to the sky, near a large French window, facing S.E., the glass of which was clear, and had been carefully cleaned, without any rise being perceptible. The sun, at an altitude of about 40° , was shining brightly on white vapour and light cirro-cumuli*.

Thermometers were also placed in the open air on the north side of the house, on a still day, exposed to half the sky when covered with bright white clouds; but the mercury stood at the same height as in a dark passage on the same side of the building†.

The same apparent absence of any direct heating-power in the light reflected from the sky generally was shown in this, as in the previous series of experiments when the solar thermometer was screened, excepting in the direction of the sun.

As respects the momentary increase of insolation which occurs in connexion with bright vapour in the neighbourhood of the sun, further experiment is required for the purpose of ascertaining whether it is due to radiation or to reflection.

NOTE.—An opportunity occurred on the 7th of June of repeating the experiments with screens at altitudes of the sun exceeding 50° . The following results were obtained:—

h m					
At 0	0.	B. B. 110.	Temp. of shade	73.	{ Sky cloudless, but with a good deal of white vapour, more especially about the sun.
		(Exposed to the sun and sky.)			
0	4.	B. B. 90.	Temp. of shade	73.	" " "
		(Shaded from sun by a 2-in. screen.)			
0	30.	B. B. 104.	Temp. of shade	73.	Light air.
		(Exposed to sun and sky.)			
0	35.	B. B. 94.	Temp. of shade	73.	Light air.
		(Shaded from sun by a $\frac{1}{4}$ -in. screen.)			
1	0.	B. B. 108.	Temp. of shade	74.	Quite calm.
		(Exposed to sun and sky.)			
1	5.	B. B. 109.	Temp. of shade	74.	Quite calm.
		(Exposed to sun through a 2-in. circular aperture in a 12-in. screen.)			
1	15.	B. B. 108.	Temp. of shade	74.	Quite calm.
		(Exposed to sun and sky.)			
1	18.	B. B. 106.	Temp. of shade	74.	Quite calm.
		(Exposed to sun through a 2-in. circular aperture in a 12-in. screen.)			
1	20.	B. B. 106.	Temp. of shade	74.	Quite calm.
		(Exposed to sun but screened from sky.)			

* Experiments were also tried with a 7-inch lens, without result.

† The thermometer exposed to the sky would probably have stood *lower* than the one in the house if the sky had been perfectly clear.

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END OF THE SEVENTEENTH VOLUME.

OBITUARY NOTICES OF FELLOWS DECEASED.

Æt. 1 to 12 (1791 to 1804).

MICHAEL FARADAY* was born in the working class, of a very religious family. For two generations at least those who preceded him shared the extreme views in favour of toleration and disestablishment which caused, first, the deposition of the Rev. John Glas, and afterwards the secession of his son-in-law, R. Sandeman, from the Presbyterian Church of Scotland. That the revealed will of Christ should be the supreme and only law, not only in all church questions, but in every thought and word and deed, was the belief of those who were nearest to Faraday in his infancy ; and this he held throughout his life, as though it had been a special revelation to himself.

His father, James, was the third of ten children born at Clapham in Yorkshire. He was a blacksmith ; his eldest brother worked as slater, grocer, and millowner, another brother was a farmer, another a packer, another a shopkeeper, and the youngest a shoemaker. Another of the brothers died young, in the year Michael was born ; and a letter from the mother of the young man shows the strength of the religious feeling in mother and son.

When twenty-five, in 1786, James Faraday married Margaret Hastwell, daughter of a farmer near Kirkby Stephen. Soon after their marriage they came to Newington in Surrey, where Michael, their third child, was born, September 22, 1791, in a house probably long since pulled down. The father obtained work at Boyd's, in Welbeck Street ; and when Michael was about five years old, after living a short time in Gilbert Street, they removed to rooms over a coach-house in Jacob's Well Mews, Charles Street, Manchester Square. The home of Michael Faraday was in these mews for nearly ten years ; and his family remained there until 1809, when they moved to 18 Weymouth Street.

Faraday himself has pointed out where he played at marbles in Spanish Place, and where, years later, he took care of his little sister in Manchester Square. He says, " My education was of the most ordinary description, consisting of little more than the rudiments of reading, writing, and arithmetic at a common day-school. My hours out of school were passed at home and in the streets."

Only a few yards off was a bookseller's shop, No. 2 Blandford Street ; there, as a boy of thirteen, in 1804, he went on trial for a year to Mr. George Riebau. Once when walking with a niece they passed a little news-boy, when he said, " I always feel a tenderness for those boys, because I once carried newspapers myself."

* An account of " Faraday as a Discoverer " having been already given to the world by one eminently qualified for the task, it has been deemed advisable in this place to give a narrative of the chief events of his personal history, with such indications of his character and opinions as may be read in his written correspondence and private memorials. This service has been kindly rendered by Dr. Bence Jones, F.R.S., Secretary to the Royal Institution, the devoted friend of Faraday, in whose hands have been placed the letters and manuscripts from which the substance, and, for the most part, the words of the present notice have been taken.— W. S., Sec. R.S.

Æt. 13 to 19 (1805 to 1811).

On the 7th of October, 1805, when fourteen, Faraday was apprenticed; and, in consideration of his faithful service, no premium was given to Riebau.

Four years later his father wrote (in 1809), "Michael is bookbinder and stationer, and is very active at learning his business. He has been most part of four years of his time out of seven. He has a very good master, and mistress, and likes his place well: he had a hard time for some while at first going; but, as the old saying goes, he has rather got the head above water, as there is two other boys under him."

Faraday himself says, "Whilst an apprentice I loved to read the scientific books which were under my hands, and amongst them delighted in Marcet's 'Conversations on Chemistry,' and the electrical treatises in the 'Encyclopædia Britannica.' I made such simple experiments in chemistry as could be defrayed in their expense by a few pence per week, and also constructed an electrical machine, first with a glass phial, and afterwards with a real cylinder, as well as other electrical apparatus of a corresponding kind." He told a friend that Watts on the Mind first made him think, and that his attention was turned to science by the article "Electricity" in an encyclopædia he was employed to bind.

"My master," he says, "allowed me to go occasionally of an evening to hear the lectures delivered by Mr. Tatum in natural philosophy at his house, 53 Dorset Street, Fleet Street. I obtained a knowledge of these lectures by bills in the streets and shop-windows near his house. The hour was eight o'clock in the evening. The charge was 1s. per lecture, and my brother Robert [who was three years older and followed his father's business] made me a present of the money for several. I attended twelve or thirteen lectures between February 19, 1810, and September 26, 1811. It was at these lectures I first became acquainted with Magrath, Newton, Nicol, and others."

He learned perspective of a Mr. Masquerier, that he might illustrate these lectures. "Masquerier lent me Taylor's Perspective, a 4to volume, which I studied closely, copied all the drawings, and made some other very simple ones, as of cubes or pyramids, or columns in perspective, as exercises of the rules. I was always very fond of copying vignettes and small things in ink; but I fear they were mere copies of the lines, and that I had little or no sense of the general effect and of the power of the lines in producing it." How he was educating himself at this time and the subjects that interested him, may be seen in a manuscript volume (a shadow of the future) which he called "The Philosophical Miscellany, being a collection of notices, occurrences, events, &c. relating to the arts and sciences collected from the public papers, reviews, magazines, and other miscellaneous works. Intended," he says, "to promote both amusement and instruction, and also to corroborate or invalidate those theories which are continually starting into the world of science. Collected by M. Faraday, 1809-10."

In 1811 (æt. 19) he became acquainted, at Mr. Tatum's, with Mr.

Huxtable and Mr. Benjamin Abbott ; the first was a medical student, the other, who belonged to the Society of Friends, was employed in a house of business in the city.

Mr. Huxtable lent him Parkes's 'Chemistry,' which Faraday bound for him, and the third edition of Thompson's 'Chemistry.'

Æt. 20 (1812).

Among the few notes Faraday made of his own life are the following :—

"During my apprenticeship I had the good fortune, through the kindness of Mr. Dance, who was a customer of my master's shop and also a member of the Royal Institution, to hear four of the last lectures of Sir H. Davy in that locality [he always sat in the gallery over the clock]. The dates of these lectures were February 29, March 14, April 8 and 10, 1812. Of these I made notes, and then wrote out the lectures in a fuller form, interspersing them with such drawings as I could make. The desire to be engaged in scientific occupation, even though of the lowest kind, induced me, whilst an apprentice, to write, in my ignorance of the world and simplicity of my mind, to Sir Joseph Banks, then President of the Royal Society. Naturally enough, 'No answer,' was the reply left with the porter."

On Sunday, July 12, 1812, three months before his apprenticeship was over, he wrote the first of a series of letters to his friend Mr. Benjamin Abbott (who was a year and a half younger than himself), from which a full view can be gained of what he was by nature, and what his self-education at this time had made him.

"I have lately made a few simple galvanic experiments merely to illustrate to myself the first principles of the science. I was going to Knight's to obtain some nickel, and bethought me that they had malleable zinc. I inquired and bought some ; have you seen any yet ? The first portion I obtained was in the thinnest pieces possible,—observe, in a flattened state. It was, they informed me, thin enough for the electric smoke, or, as I before called it, De Luc's electric column. I obtained it for the purpose of forming disks, with which and copper, to make a little battery. The first I completed contained the immense number of seven pair of plates !!! and of the immense size of halfpence !!!!! I, sir, I, my own self, cut out seven disks of the size of halfpences each ! I, sir, covered them with seven halfpence, and I interposed between seven, or rather six, pieces of paper soaked in a solution of muriate of soda !!! But laugh no longer, dear A., rather wonder at the effects this trivial power produced ; it was sufficient to produce the decomposition of sulphate of magnesia, an effect which extremely surprised me." And then he describes how he built up a larger battery, and obtained greater and further effects, and reasons on the results, and urges his friend to think of these things, and "let me, if you please, sir, if you please let me know your opinion." On the Monday he adds a postscript : "I am just now involved in a fit of vexation. I have an excellent prospect before me,

and cannot take it up for want of ability. Had I perhaps known as much of mechanics, mathematics, mensuration, and drawing as I do perhaps of some other sciences, that is to say, had I happened to employ my mind with these instead of other sciences, I could have obtained a place, an easy place, too, and that in London, at 5', 6', 7', £800 per annum. Alas! alas! Inability. I must ask your advice on the subject, and intend, if I can, to see you next Sunday; one necessary branch of knowledge would be that of the steam-engine, and, indeed, anything where iron is concerned."

In his next letter he says, speaking of fresh experiments with his battery, "I must trust to your experiments more than my own; I have no time, and the subject requires several;" and in a letter written August 11, "Pyrotechny is a beautiful art, but I never made any practical progress in it, except in the forming a few bad squibs; so that you will gain little from me on that point."

In his next letter (August 19) he says, "I cannot see any subject except chlorine to write on. Be not surprised, my dear A., at the ardour with which I have embraced this new theory. I have seen Davy himself support it. I have seen him exhibit experiments (conclusive experiments) explanatory of it; and I have heard him apply these experiments to the theory, and explain and enforce them in (to me) an irresistible manner. Conviction, sir, struck me, and I was forced to believe him, and with that belief came admiration."

In a letter dated about a fortnight before his apprenticeship was out he writes, "Your commendations of the MS. lectures [of Davy] compel me to apologize most humbly for the numerous (very, very numerous) errors they contain. If I take you right, the negative words 'no flattery' may be substituted by the affirmative 'irony;' be it so, I bow to the superior scholastic erudition of Sir Ben. There are in them errors that will not bear to be jested with, since they concern not my own performance so much as the performance of Sir H., and those are errors in theory; there are, I am conscious, errors in theory, and those errors I would wish you to point out to me before you attribute them to Davy."

In the last letter before the great change came (October 1, 1812), he says, "I rejoice in your determination to pursue the subject of electricity, and have no doubt that I shall have some very interesting letters on the subject. I shall certainly wish to (and will if possible) be present at the performance of the experiments; but you know I shall shortly enter on the life of a journeyman, and then I suppose time will be more scarce than it is even now."

On the 8th of October he went as journeyman bookbinder to a Mr. De la Roche, then a French emigrant in London. His master was a very passionate man, and troubled his assistant much; so much, that he felt he could not remain in that place, though every inducement was held out to him. His master liked him; and, to tempt him to stay, said "I have no child, and if you will stay with me you shall have all I have when I am gone."

In his first letter to his friend Abbott, after his apprenticeship was ended,

October 11, he says, "As for the change which you suppose to have taken place with respect to my situation and affairs, I have to thank my late master, it is but little. Of liberty and of time I have, if possible, less than before, though I hope my circumspection has not at the same time decreased. I am well aware of the irreparable evils that an abuse of those blessings will give rise to. These were pointed out to me by common sense; nor do I see how anyone who considers his own station and his own free occupations, pleasures, actions, &c. can unwittingly engage himself in them. I thank that Cause to whom thanks are due that I am not in general a profuse waster of those blessings which are bestowed on me as a human being; I mean health, sensation, time, and temporal resources. Understand me here, for I wish not to be mistaken: I am well aware of my own nature; it is evil, and I feel its influence strongly. I know, too, that——; but I find that I am passing insensibly to a point of divinity; and as these matters are not to be treated lightly, I will refrain from pursuing it."

To his friend Huxtable he writes on the 18th: "Conceiving it would be better to delay my answer until my time was expired, I did so; that took place Oct. 7, and since then I have had by far less time and liberty than before. With respect to a certain place I was disappointed, and am now working at my old trade, the which I wish to leave at the first convenient opportunity. I am at present in very low spirits, and scarce know how to continue on in a strain that will be any way agreeable to you."

"Under the encouragement of Mr. Dance," he says, "I wrote to Sir Humphry Davy, sending, as a proof of my earnestness, the notes I had taken of his last four lectures; the reply was immediate, kind, and favourable. After this I continued to work as a bookbinder, with the exception of some days during which I was writing as an amanuensis for Sir H. Davy, at the time when the latter was wounded in the eye from an explosion of the chloride of nitrogen."

On the 24th of December, 1812, Sir Humphry Davy wrote to Faraday:—"Sir, I am far from displeased with the proof you have given me of your confidence, and which displays great zeal, power of memory, and attention. I am obliged to go out of town, and shall not be settled in town till the end of January; I will then see you at any time you wish. It would gratify me to be of any service to you; I wish it may be in my power. I am, Sir, Your obedient humble Servant."

Æt. 21 (1813).

He "went," he says, "to the City Philosophical Society, which was founded in 1808 at Mr. Tatum's house, and, I believe, by him. He introduced me as a member of the Society in 1813. Magrath was Secretary to the Society. It consisted of thirty or forty individuals, perhaps all in the humble or moderate rank of life. Those persons met every Wednesday evening for mutual instruction. Every other Wednesday the members were alone, and considered and discussed such questions as were brought forward by

each in turn. On the intervening Wednesday evenings friends also of the members were admitted, and a lecture was delivered, literary or philosophical, each member taking the duty, if possible, in turn (or in default paying a fine of half a guinea). This Society was very moderate in its pretensions, and most valuable to the members in its results." [I remember, too, says one of the members, we had a "class-book," in which, in rotation, we wrote essays, and passed it to each other's houses.]

Sir H. Davy, at his first interview, advised him to keep in business as a bookbinder, and he promised to give him the work of the Institution, as well as his own and that of as many of his friends as he could influence.

One night, in Weymouth Street, he was startled by a loud knock at the door, and on looking out he saw a carriage from which the footman had alighted and left a note for him. This was a request from Sir H. that he would call on him the next morning. Sir H. then referred to their former interview, and inquired whether he was still in the same mind, telling him that if so he would give him the place of assistant in the laboratory of the Royal Institution, from which he had on the previous day ejected its former occupant. The salary was to be 25s. a week, with two rooms at the top of the house.

In the minutes of the meeting of Managers on the 1st of March, 1813, is this entry :—"Sir Humphry Davy has the honour to inform the Managers that he has found a person who is desirous to occupy the situation in the Institution lately filled by William Payne. His name is Michael Faraday. He is a youth of twenty-two years of age. As far as Sir H. Davy has been able to observe or ascertain, he appears well fitted for the situation. His habits seem good, his disposition active and cheerful, and his manner intelligent. He is willing to engage himself on the same terms as given to Mr. Payne at the time of quitting the Institution.

"Resolved,—That Michael Faraday be engaged to fill the situation lately occupied by Mr. Payne, on the same terms."

As early as the 8th of March, Faraday dates his first letter from the Royal Institution to his friend Abbott.

"I have been employed," he says, "to-day in part in extracting the sugar from a portion of beetroot, and also in making a compound of sulphur and carbon—a combination which has lately occupied in a considerable degree the attention of chemists."

A month later he says :—"When writing to you I seize that opportunity of striving to describe a circumstance or an experiment clearly, so that you will see I am urged on, by selfish motives partly, to our mutual correspondence ; but though selfish yet not censurable.

"Agreeable to what I have said above, I shall at this time proceed to acquaint you with the results of some more experiments on the detonating compound of chlorine and azote ; and I am happy to say I do it at my ease, for I have escaped (not quite unhurt) from four different and strong explosions of the substance. Of these the most terrible was when I was

holding between my thumb and finger a small tube containing $7\frac{1}{2}$ grains of it. My face was within 12 inches of the tube, but I fortunately had on a glass mask. It exploded by the slight heat of a small piece of cement that touched the glass above half an inch from the substance, and on the outside. The explosion was so rapid as to blow my hand open, tear off a part of one nail, and has made my fingers so sore that I cannot yet use them easily. The pieces of tube were projected with such force as to cut the glass face of the mask I had on."

On the 1st of June he writes :—"The subject upon which I shall dwell more particularly at present has been in my head for a considerable time, and it now bursts forth in all its confusion. The opportunities that I have lately had of attending and obtaining instruction from various lecturers in their performance of the duty attached to that office, has enabled me to observe the various habits, peculiarities, excellencies, and defects of each of them, as they were evident to me during the delivery. I did not wholly let this part of the things occurrent escape my notice ; but, when I found myself pleased, endeavoured to ascertain the particular circumstance that had affected me ; also, when attending to Mr. Brande and Mr. Powell in their lectures, I observed how the audience were affected, and by what their pleasure and their censure was drawn forth.

"It may perhaps appear singular and improper that one who is entirely unfit for such an office himself, and who does not even pretend to any of the requisites for it, should take upon him to censure and to commend others, to express satisfaction at this, to be displeased with that, according as he is led by his judgment, when he allows that his judgment is unfit for it ; but I do not see, on consideration, that the impropriety is so great. If I am unfit for it, it is evident that I have yet to learn ; and how learn better than by the observation of others ? If we never judge at all we shall never judge right ; and it is far better to learn to use our mental powers (though it may take a whole life for the purpose) than to leave them buried in idleness, a mere void." And then for three letters he goes on with his ideas on lecture-rooms, lectures, apparatus, diagrams, experiments, audiences ; and when urged, two years later, to complete his remarks, he answers, Dec. 31, 1816 :—"With respect to my remarks on lectures, I perceive I am but a mere tyro in the art, and therefore you must be satisfied with what you have, or expect at some future time a recapitulation, or rather revision of them."

"During this spring Magrath and I established the mutual-improvement plan, and met at my rooms up in the attics of the Royal Institution, or at Wood Street at his warehouse. It consisted perhaps of half a dozen persons, chiefly from the City Philosophical Society, who met of an evening to read together, and to criticise, correct, and improve each other's pronunciation and construction of language. The discipline was very sturdy, the remarks very plain and open, and the results most valuable. This continued for several years." Saturday night was the time of meeting

at the Royal Institution, in the furthest and uppermost room in the house, then Faraday's place of residence.

He says :—" In the autumn Sir H. Davy proposed going abroad, and offered me the opportunity of going with him as his amanuensis, and the promise of resuming my situation in the Institution upon my return to England. Whereupon I accepted the offer, left the Institution on the 13th of October, and, after being with Sir H. Davy in France, Italy, Switzerland, the Tyrol, Geneva, &c. in that and the following year, returned to England and London the 23rd April 1815."

Whilst abroad he kept a daily journal, "not," he said, "to instruct or to inform, or to convey even an imperfect idea of what it speaks ; its sole use is to recall to my mind at some future time the things I see now, and the most effectual way to do that will be, I conceive, to write down, be they good or bad, my present impressions." From this journal, and from his letters to his mother and his friend Benjamin Abbott, only a few characteristic passages can be given here.

In his journal he wrote, Wednesday, 13th October :—" This morning formed a new epoch in my life. I have never before, within my recollection, left London [he had as an infant gone to Newcastle and Whitehaven, by sea chiefly] at a greater distance than twelve miles, and now I leave it perhaps for many years, to visit spots between which and home whole realms will intervene. 'T is indeed a strange venture at this time to trust ourselves in a foreign and hostile country, where also so little regard is had to protestations and honour, that the slightest suspicion would be sufficient to separate us for ever from England, and perhaps from life. But curiosity has frequently incurred dangers as great as these, and therefore why should I wonder at it in the present instance. If we return safe, the pleasures of recollection will be highly enhanced by the dangers encountered ; and a never-failing consolation is that, whatever be the fate of our party, variety, a great source of amusement, and pleasure must occur."

Some idea of the variety of his observations may be got from this note, 28th October, Dreux :—" I cannot help dashing a note of admiration to one thing found in this part of the country—the pigs ! At first I was positively doubtful of their nature ; for though they have pointed noses, long ears, rope-like tails, and cloven feet, yet who would have imagined that an animal with a long thin body, back and belly arched upwards, lank sides, long slender feet, and capable of outrunning our horses for a mile or two together, could be at all allied to the fat sow of England ! When I first saw one, which was at Morlaix, it started so suddenly, and became so active in its motions on being disturbed, and so dissimilar in its actions to our swine, that I looked out for a second creature of the same kind before I ventured to decide on its being a regular or an extraordinary production of nature ; but I find they are all alike, and that what at a distance I should judge to be a greyhound, I am obliged, on a near approach, to acknowledge a pig."

Æt. 22 (1814).

To his mother he writes, April 14, 1814, from Rome:—"When Sir H. Davy first had the goodness to ask me whether I would go with him, I mentally said, 'no, I have a mother, I have relations here,' and I almost wished that I had been insulated and alone in London; but now I am glad that I have left some behind me on whom I can think, and whose actions and occupations I can picture in my mind. Whenever a vacant hour occurs I employ it by thinking on those at home. In short, when sick, when cold, when tired, the thoughts of those at home are a calm and refreshing balm to my heart. Let those who think such thoughts are useless, vain, and paltry think so still. I envy them not their more refined and more estranged feelings. Let them look about the world unencumbered by such ties and heart-strings, and let them laugh at those who, guided more by nature, cherish such feelings. For me, I still cherish them, in opposition to the dictates of modern refinement, as the first and greatest sweetness in the life of man."

In a letter to his friend Abbott, dated September 6, 1814, he says:—"I fancy that when I set my foot in England I shall never take it out again; for I find the prospect so different from what it at first appeared to be, that I am certain, if I could have foreseen the things that have passed, I should never have left London. In the second place, enticing as travelling is (and I appreciate fully its advantages and pleasures), I have several times been more than half decided to return hastily home; but second thoughts have still induced me to try what the future may produce, and now I am only detained by the wish of improvement. I have learned just enough to perceive my ignorance, and, ashamed of my defects in everything, I wish to seize the opportunity of remedying them. The little knowledge I have gained in languages makes me wish to know more of them, and the little I have seen of men and manners is just enough to make me desirous of seeing more; added to which, the glorious opportunity I enjoy of improving in the knowledge of chemistry and the sciences continually, determines me to finish this voyage with Sir Humphry Davy; but if I wish to enjoy those advantages I have to sacrifice much; and though those sacrifices are such as an humble man would not feel, yet I cannot quietly make them. Travelling, too, I find, is almost inconsistent with religion (I mean modern travelling), and I am yet so old-fashioned as to remember strongly (I hope perfectly) my youthful education, and upon the whole, *malgré* the advantages of travelling, it is not impossible but that you may see me at your door when you expect a letter."

Æt. 23 (1815).

On the 25th January 1815, he writes:—"You tell me I am not happy, and you wish to share my difficulties. I have nothing important to tell you, or you should have known it long ago; but, since your friendship makes you feel for me, I will trouble you with my trifling affairs."

“It happened, a few days before we left England, that Sir H.’s valet declined going with him, and in the short space of time allowed by circumstances, another could not be got. Sir H. told me he was very sorry, but that if I would do such things as were absolutely necessary for him until he got to Paris, he should there get another. I murmured, but agreed. At Paris he could not get one; at Lyons he could not get one; at Montpellier he could not get one; nor at Genoa, nor at Florence, nor at Rome, nor in all Italy; and I believe at last he did not wish to get one; and we are just the same now as we were when we left England. This, of course, throws things into my duty which it was not my agreement, and is not my wish to perform, but which are, if I remain with Sir H., unavoidable. These, it is true, are very few; for having been accustomed in early years to do for himself, he continues to do so at present, and he leaves very little for a valet to perform; and as he knows that it is not pleasing to me, and that I do not consider myself as obliged to do it, he is always as careful as possible to keep those things from me which he knows would be disagreeable. But Lady Davy is of another humour. She likes to show her authority, and at first I found her extremely earnest in mortifying me. This occasioned quarrels between us, at each of which I gained ground and she lost it; for the frequency made me care nothing about them and weakened her authority, and after each she behaved in a milder manner. Sir H. has also taken care to get servants of the country, ycleped *lacquais de place*, to do everything she can want, and now I am somewhat comfortable; indeed at this moment I am perfectly at liberty, for Sir H. has gone to Naples to search for a house or lodging to which we may follow him, and I have nothing to do but see Rome, write my journal, and learn Italian.”

About the same time he writes to his friend Huxtable:—

“Since Sir H. has left England he has made a great addition to chemistry in his researches on the nature of iodine. He first showed that it was a simple body. He combined it with chlorine and hydrogen, and lately with oxygen, and thus has added three acids of a new species to the science. He combined it with the metals, and found a class of salts analogous to the hyperoxymuriates. He still further combined these substances, and investigated their curious and singular properties.

“The combination of iodine with oxygen is a late discovery, and the paper has not yet perhaps reached the Royal Society. It confirms all Sir H.’s former opinions and statements, and shows the inaccuracy of the labours of the French chemists on the same subjects.

“Sir Humphry also sent a long paper lately to the Royal Society, on the ancient Greek and Roman colours, which will be worth your reading when it is printed.”

A fortnight after his return to England he was engaged as assistant in the laboratory at a salary of 30*s.* a week, and apartments were given to him.

Æt. 24 (1816).

On the 17th of January, 1816, Faraday began a course of seventeen Lectures on Chemistry, at the City Philosophical Society, which extended over two years and a half. He called them "an account of the inherent Properties of Matter, of the forms in which matter exists, and of simple elementary substances." During the year he gave six or seven lectures on the general properties of matter, on the attraction of cohesion, on chemical affinity, on radiant matter, on oxygen, chlorine, iodine, and fluorine, on hydrogen, and on nitrogen. He wrote his first lectures at full length, whilst of the latter lectures he only made notes, putting the experiments very distinctly apart, and he kept very much to this plan during the rest of his life.

It was in this year also that Faraday published his first paper, an analysis of native caustic lime, in the *Quarterly Journal of Science*. In the volume of his 'Experimental Researches on Chemistry and Physics,' he has added a note:—"I reprint this paper at full length; it was the beginning of my communications to the public, and in its results very important to me. Sir Humphry Davy gave me the analysis to make as a first attempt in chemistry, at a time when my fear was greater than my confidence, and both far greater than my knowledge; at a time, also, when I had no thought of ever writing an original paper on science. The addition of his own comments, and the publication of the paper, encouraged me to go on making, from time to time, other slight communications, some of which appear in this volume. Their transference from the 'Quarterly' into other journals increased my boldness, and now that forty years have elapsed, and I can look back on what successive communications have led to, I still hope, much as their character has changed, that I have not either now or forty years ago been too bold."

Early in February he thus wrote to his friend Abbott:—"Be not offended that I turn to write you a letter, because I feel a disinclination to do anything else; but rather accept it as a proof that conversation with you has more power with me than any other relaxation from business,—business I say; and I believe it is the first time for many years that I have applied it to my own occupations. But at present they actually deserve the name; and you must not think me in a laughing mood, but in earnest. It is now 9 o'clock P.M., and I have just left the laboratory and the preparation for to-morrow's two lectures. Our double course makes me work enough; and to them add the attendance required by Sir H. in his researches, and then if you compare my time with what is to be done in it, you will excuse the slow progress of our correspondence on my side. Understand me, I am not complaining; the more I have to do the more I learn, but I wish to avoid all impression on your side that I am lazy—suspicions, by-the-by, which a moment's reflection convinces me can never exist."

In consideration of the additional labour caused to him by Mr. Brande's

lectures in the laboratory, his salary at the Institution was increased to £100 per annum.

This year Faraday began a common-place book, in which he continued to make entries on all subjects for fifteen years. Some of the earliest are on the production of oxygen, on the combustion of zinc and iron in condensed air, on a course of lectures on geology delivered at the Royal Institution by Mr. Brande, and an account of Zerah Colburn, thirteen years old, the American calculating boy. Sir H. Davy sent him with a note, saying "his father will explain to you the method the son uses, in confidence; I wish to ascertain if it can be practically used."

He wrote in this year:—"When Mr. Brande left London in August, he gave the Quarterly Journal in charge to me; it has very much of my time and care, and writing through it has been more abundant with me. It has, however, also been the means of giving me earlier information on some new objects of science."

Æt. 25 (1817).

In 1817 he gave five lectures at the City Philosophical Society on the atmosphere, on sulphur and phosphorus, on carbon, on combustion, and on the metals generally. He had a paper in the Quarterly Journal on the escape of gases through capillary tubes. The entries in his common-place book consist of geological notes of South Moulton Slate, Tiverton, Hulverston, Taunton, Somerton, and Castle Cary; a multitude of chemical queries or questions to be worked at, among which are the exciting effects of different vapours and gaseous mixtures; compounds of chlorine and carbon made out in the autumn of 1820; electricity, magnetism; a pyrometer; extracts from Shakspeare, Lalla Rookh, Rambler, &c.

At the end of the year he tells his friend Abbott that he can see less of him, "in consequence of an arrangement I have made with a gentleman recommended to me by Sir H. Davy; I am engaged to give him lessons in mineralogy and chemistry, three times a week, in the evenings, for a few months."

Æt. 26 (1818).

In 1818 five lectures were given by Faraday at the City Philosophical Society, on gold, silver, &c., on copper and iron, on tin, lead, and zinc, and on alkalies and earths. He had six papers in the Quarterly Journal, of which the most important was on sounds produced by flame in tubes.

In his common-place book there is a long course of lectures on oratory, by Mr. B. H. Smart; questions for Dorset Street; an experimental agitation of the question of electrical induction, "Bodies do not act where they are not—query, is not the reverse of this true? Do not all bodies act where they are not; and do any of them act where they are? Query, the nature of courage; is it a quality or a habit?" Chemical questions.

On July 1st he gave a lecture to the City Philosophical Society. It is entitled "Observations on the Inertia of the Mind." As this lecture is wholly

written out, it probably was one of the essays contained in the class-book of the Society.

Towards the end of the year Faraday wrote his first letter to M. G. De la Rive, the father of the present M. Auguste De la Rive. He says :—

“ Dear Sir,—Your kindness, when here, in requesting me to accept the honour of a communication with you on the topics which occur in the general progress of science, was such as almost to induce me to overstep the modesty due to my humble situation in the philosophical world, and to accept of the offer you made me. But I do not think I should have been emboldened thus to address you had not Mr. Newman since then informed me that you again expressed a wish to him that I should do so ; and fearful that you should misconceive my silence I put pen to paper, willing rather to run the risk of being thought too bold than of incurring the charge of neglect towards one who had been so kind to me in his expressions. My slight attempts to add to the general stock of chemical knowledge have been received with favourable expressions by those around me ; but I have, on reflection, perceived that this arose from kindness on their parts, and the wish to incite me on to better things. I have always, therefore, been fearful of advancing on what has been said, lest I should assume more than was intended ; and I hope that a feeling of this kind will explain to you the length of time which has elapsed between the time when you requested me to write and the present moment when I obey you.

“ I am not entitled, by any peculiar means of obtaining a knowledge of what is doing at the moment in science, to deserve your attention, and I have no claims in myself to it. I judge it probable that the news of the philosophical world will reach you much sooner through other more authentic and more dignified sources, and my only excuse even for this letter is obedience to your wishes, and not on account of anything interesting for its novelty.” He then describes a new process for the preparation of gas for illumination. He ends, “ I am afraid that, with all my reasons, I have not been able to justify this letter. If my fears are true I regret at least ; it was your kindness that drew it from me, and to your kindness I must look for an excuse.”

Æt. 27 (1819).

In 1819 he had no paper in the Quarterly Journal. He gave one lecture at the City Philosophical Society on the Forms of Matter. Matter he classifies into four states, which depend on differences in the essential properties, and cautiously says, “ thus a partial reconciliation is established to the belief that all the variety of this fair globe may be converted into three kinds of radiant matter.”

His common-place book contains scarcely any scientific notices.

On July the 10th he started by coach for a three weeks' walking tour in Wales, with his friend Magrath. He kept a journal, and his descriptions of the scenery, of the copper works of Swansea, the mines of Anglesea, and the slate-quarries of Bangor, are still of interest.

Æt. 28 (1820).

This year was one of the most important in the life of Faraday; he had his first paper read to the Royal Society on two new compounds of chlorine and carbon, and on a new compound of iodine, carbon, and hydrogen; and with Mr. Stodart, the surgical instrument maker, he published, in the *Quarterly Journal of Science*, experiments on the alloys of steel, made with a view to its improvement.

In his common-place book, among the chemical questions, we find chemical lessons, or a plan of lessons in chemistry, and processes for manipulation, the germ of his work on *Chemical Manipulation*. There is also a list headed "Lecture Subjects," including application of statics to chemistry, approximation of mechanical and chemical philosophy, application of mathematics to actual service and use in the arts, series of mechanical arts, as tanning.

On the 20th of April he writes to M. G. De la Rive:—"I never in my life felt such difficulty in answering a letter as I do at this moment your very kind one of last year. I was delighted on receiving it to find that you had honoured me with any of your thoughts, and that you would permit me to correspond with you by letter. Mr. Stodart and myself have lately been engaged in a long series of experiments and trials on steel, with the hope of improving it, and I think we shall in some degree succeed. We are still very much engaged in the subject; but if you will give me leave I will, when they are more complete, which I expect will be shortly, give you a few notes on them. I succeeded by accident a few weeks ago in making artificial plumbago, but not in useful masses. We have lately had some important trials for oil in this metropolis, in which I, with others, have been engaged. They have given occasion for many experiments in oil, and the discovery of some new and curious results; one of the trials only is finished, and there are four or five more to come. As soon as I can get time, it is my intention to trace more closely what takes place in oil by heat."

June 26 he sends a long abstract of the paper on Steel, and ends:—"Now I think I have noticed the most interesting points at which we have arrived. Pray pity us, that after two years' experiments we have got no further; but I am sure if you knew the labour of the experiments you would applaud us for our perseverance at least. We are still encouraged to go on, and I think the experience we have gained will shorten our future labours.

"If you should think any of our results worth notice in the 'Bibliothèque,' this letter is free to be used in any way you please. Pardon my vanity for supposing anything I can assist in doing can be worth attention; but you know we live in the good opinion of ourselves and of others, and therefore naturally think better of our own productions than they deserve."

Early the following month there is evidence that an entire change took

place in the state of his mind. Among his friends was Mr. Edward Barnard, one of a family living in Paternoster Row, with which he had long been intimate, and which agreed with his own family in its religious views. Faraday proposed to, and ultimately was accepted by, Mr. Barnard's sister, Sarah.

Æt. 29 (1821).

March 11, Sir H. Davy wrote :—" Dear Mr. Faraday, I have spoken to Lord Spencer, and I am in hopes that your wishes may be gratified; but do not mention the subject till I see you." This wish was probably to bring his wife to the Institution. In June he was appointed superintendent of the house and laboratory, in the absence of Mr. Brande.

All obstacles were removed, and the marriage took place on the 12th of June. Mr. Faraday, desiring that the day should be considered just like any other day, offended some of his near relations by not asking them to his wedding.

In a letter to his wife's sister, previous to the marriage, he says, " There will be no bustle, no noise, no hurry occasioned even in one day's proceeding. In externals, that day will pass like all others, for it is in the heart that we expect and look for pleasure."

A month later, at a meeting of the congregation, he was fully admitted as a member of the Sandemanian Church.

His common-place book shows that he read little. In a letter, May 19, to M. G. De la Rive, he says, " Mr. Stodart and myself are continuing our experiments on steel, which are very laborious."

On July 12, a paper was read to the Royal Society on a new Compound of Chlorine and Carbon, by Phillips and Faraday. This, as well as Faraday's previous paper on two Chlorides of Carbon, was printed in the *Philosophical Transactions*. In the *Quarterly Journal* he had a short paper on the Vapour of Mercury at common temperatures.

On the 12th of September he writes the following letter to M. G. De la Rive :—

" You partly reproach us here with not sufficiently esteeming Ampère's experiments on electro-magnetism. Allow me to extenuate your opinion a little on this point. With regard to the experiments, I hope and trust that due weight is allowed to them; but these you know are few, and theory makes up the great part of what M. Ampère has published, and theory in a great many points unsupported by experiments, when they ought to have been adduced. At the same time, M. Ampère's experiments are excellent, and his theory ingenious; and for myself, I had thought very little about it before your letter came, simply because, being naturally sceptical on philosophical theories, I thought there was a great want of experimental evidence. Since then, however, I have engaged on the subject, and have a paper in our Institution journal, which will appear in a week or two, and that will, as it contains experiment, be immediately applied by M. Ampère in sup-

port of his theory much more decidedly than it is by myself. I intend to enclose a copy of it to you, and only want the means of sending it.

“ I find all the usual attractions and repulsions of the magnetic needle by the conjunctive wire are deceptions, the motions being not attractions or repulsions, nor the result of any attractive or repulsive forces, but the result of a force in the wire, which, instead of bringing the pole of the needle nearer to or further from the wire, endeavours to make it move round it in a never-ending circle and motion whilst the battery remains in action. I have succeeded not only in showing the existence of this motion theoretically, but experimentally, and have been able to make the wire revolve round a magnetic pole, or a magnetic pole round the wire, at pleasure. The law of revolution, and to which all the other motions of the needle and wire are reducible, is simple and beautiful. Conceive a portion of connecting wire north and south, the north end being attached to the positive pole of a battery, the south to the negative ; a north magnetic pole would then pass round it continually in the apparent direction of the sun from east to west above, and from west to east below. Reverse the connexions with the battery, and the motion of the pole is reversed. Or if the south pole is made to revolve, the motions will be in the opposite directions, as with the north pole.

“ If the wire be made to revolve round the pole, the motions are according to those mentioned. For the apparatus I used there were but two plates, and the direction of the motions was of course the reverse of those with a battery of several pair of plates, and which are given above. Now I have been able experimentally to trace this motion into its various forms, as exhibited by Ampère’s helices, &c., and in all cases to show that dissimilar poles repel as well as attract, and that similar poles attract as well as repel, and to make, I think, the analogy between the helice and common bar-magnet far stronger than before ; but yet I am by no means decided that there are currents of electricity in the common magnet. I have no doubt that electricity puts the circles of the helice into the same state as those circles are in that may be conceived in the bar-magnet ; but I am not certain that this state is directly dependent on the electricity, or that it cannot be produced by other agencies, and therefore, until the presence of electrical currents be proved in the magnet by other than magnetical effects, I shall remain in doubts about Ampère’s theory.”

Oct. 8th he writes to J. Stodart, Esq. :—

“ I hear every day more and more of those sounds, which, though only whispers to me, are, I suspect, spoken aloud amongst scientific men, and which, as they in part affect my honour and honesty, I am anxious to do away with, or at least to prove erroneous in those parts which are dishonourable to me. You know perfectly well what distress the very unexpected reception of my paper on Magnetism in public has caused me, and you will not therefore be surprised at my anxiety to get out of it, though I give trouble to you and others of my friends in doing so. If I under-

stand aright, I am charged (1) with not acknowledging the information I received in assisting Sir H. Davy in his experiments on this subject ; (2) with concealing the theory and views of Dr. Wollaston ; (3) with taking the subject whilst Dr. Wollaston was at work on it ; and (4) with dishonourably taking Dr. Wollaston's thoughts, and pursuing them without acknowledgment to the results I have brought out.

“ There is something degrading about the whole of these charges ; and were the last of them true, I feel that I should not remain on the terms I now stand at with you or any scientific person. Nor can I indeed bear to remain suspected of such a thing. My love for scientific reputation is not yet so high as to induce me to obtain it at the expense of honour, and my anxiety to clear away this stigma is such, that I do not hesitate to trouble you, even beyond what you may be willing to do for me.”

He proceeds then to justify himself, and says, “ The cause of my making the experiments detailed in my paper, was the writing of the historical Sketch of Electromagnetism that has appeared in the last two Numbers of the ‘ Annals of Philosophy.’ ”

On the 30th of October he writes directly to Dr. Wollaston, saying :— “ I heard from two or three quarters that it was considered that I had not behaved honourably, and that the wrong I had done I had done to you ; I immediately wished and endeavoured to see you, but was prevented by the advice of my friends, and am only now at liberty to pursue the plan I intended to have taken at first.

“ If I have done any one wrong it was quite unintentional, and the charge of behaving dishonourably is not true. I am bold enough, sir, to beg the favour of a few minutes' conversation with you on this subject, simply for these reasons, that I can clear myself, that I owe obligations to you, that I respect you, that I am anxious to escape from unfounded impressions against me, and, if I have done any wrong, that I may apologise for it.”

The following day Dr. Wollaston writes :— “ You seem to me to labour under some misapprehension of the strength of my feelings upon the subject to which you allude. As to the opinions which others may have of your conduct, that is your concern, not mine ; and if you fully acquit yourself of making any incorrect use of the suggestions of others, it seems to me that you have no occasion to trouble yourself much about the matter. But if you are desirous of any conversation with me, and could with convenience call to-morrow morning between ten and half-past ten, you will be sure to find me.”

In a letter to M. G. De la Rive a fortnight later, he does not allude to the distress of mind he had gone through.

On Christmas Day he succeeded in making a wire through which a current of voltaic electricity was passing obey the magnetic poles of the earth in the way it does the poles of a bar-magnet.

Mr. George Barnard, who was with him in the laboratory at the time,

writes :— ‘ All at once he exclaimed, ‘ Do you see, do you see, do you see, George ! ’ as the small wire began to revolve. One end I recollect was in the cup of quicksilver, the other attached above to the centre. I shall never forget the enthusiasm expressed in his face, and the sparkling in his eyes ! ’ ”

Æt. 30 (1822).

In 1822, a paper on the Alloys of Steel by Stodart and Faraday was read to the Royal Society, and printed in the Transactions. In the Quarterly Journal of Science he had two papers on the Changing of Vegetable Colours as an alkaline property, and on some Bodies possessing it ; and on the Action of Salts on Turmeric Paper.

The results of the paper on steel were of no practical value, and this, one of his first and most laborious investigations, is strikingly distinguished from all his other works by ending in nothing.

This year he began a fresh manuscript volume, which he called “ Chemical Notes, Hints, Suggestions, and Objects of Pursuit.” To it he transferred many of the queries out of his common-place book, but he separated his subjects under different heads. He puts as a sort of preface, “ I already owe much to these notes, and think such a collection worth the making by every scientific man. I am sure none would think the trouble lost after a year’s experience.” When a query got answered, he drew a pen through it, and wrote the date of the answer across it. In this book are the first germs, in the fewest possible words, of his future work.

The last week in July he went with his friend Richard Phillips to Mr. Vivian’s, near Swansea, to introduce a new process into the copper-works, and for a trial at Hereford, which was put off. At the end of a fortnight he returned to London.

His letters to Mrs. Faraday, who went to Ramsgate, are full of affection, and the account of his “ escape from the large mansion and high company ” on the Sunday, and other passages, show how strongly religious feeling was at work in him.

Æt. 31 (1823).

Two papers this year were read to the Royal Society, and printed in the Transactions—one on Fluid Chlorine, the other on the Condensation of several Gases into Liquids ; and he had four papers in the Quarterly Journal of Science—one on Hydrate of Chlorine, one on the Change of Musket-balls in Shrapnell Shells, on the Action of Gunpowder on Lead, on the purple tint of Plate-glass affected by Light. In a letter to Prof. G. De la Rive, March 24, he says :—“ I have been at work lately, and obtained results which I hope you will approve of. I have been interrupted twice in the course of experiments by explosions, both in the course of eight days. One burnt my eyes, the other cut them, but I fortunately escaped with slight injury only in both cases, and am now nearly well. During the winter I took the opportunity of examining the hydrate of chlorine, and analyzing

it; the results, which are not very important, will appear in the next number of the Quarterly Journal (over which I have no influence). Sir H. Davy, on seeing my paper, suggested to me to work with it under pressure, and see what would happen by heat &c. Accordingly I enclosed it in a glass tube, hermetically sealed, heated it, obtained a change in the substance, and a separation into two different fluids; and upon further examination I found that the chlorine and water had separated from each other, and the chlorine gas, not being able to escape, had condensed into the liquid form. To prove that it contained no water, I dried some chlorine gas, introduced it into a long tube, condensed it, and then cooled the tube, and again obtained fluid chlorine. Hence what is called chlorine gas is the vapour of a fluid. I have written a paper, which has been read to the Royal Society, and to which the President did me the honour to attach a note, pointing out the general application and importance of this mode of producing pressure with regard to the liquefaction of gases. He immediately formed liquid muriatic acid by a similar means, and, pursuing the experiments at his request, I have since obtained sulphurous acid, carbonic acid, sulphuretted hydrogen, euchlorine, and nitrous oxide in the fluid state, quite free from water. Some of these require great pressure for this purpose, and I have had many explosions.

"I send you word of these results because I know your anxiety to hear of all that is new, but do not mention them publicly (or at least the latter ones, until you hear of them, either through the journals, or by another letter from me, or from other persons), because Sir Humphry Davy has promised the results in a paper to the Royal Society for me, and I know he wishes first to have them read there; after that they are at your service.

"I expect to be able to reduce many other gases to the liquid form, and promise myself the pleasure of writing you about them."

March 25, Monday, he writes to his friend Huxtable:—"I met with another explosion on Saturday evening, which has again laid up my eyes. It was from one of my tubes, and was so powerful as to drive the pieces of glass like pistol-shot through a window. However, I am getting better, and expect to see as well as ever in a few days. My eyes were filled with glass at first."

On May the 1st his certificate was read for the first time at the Royal Society:—

"Mr. Michael Faraday, a gentleman eminently conversant in chemical science, and author of several papers, which have been published in the Transactions of the Royal Society, being desirous of becoming a Fellow thereof, we, whose names are undersigned, do of our personal knowledge recommend him as highly deserving that honour, and likely to become a useful and valuable member."

Twenty-nine names follow; the first six were Wm. H. Wollaston, J. G. Children, Wm. Babington, Sir W. Herschel, J. South, Davies Gilbert. The

certificate had to be read at ten successive meetings before the ballot came on.

On the 30th of May he wrote to H. Warburton, Esq. :—" Sir, I have been anxiously waiting the opportunity you promised me of a conversation with you, and from late circumstances am now still more desirous of it than at the time when I saw you in the Committee. I am sure you will not regret the opportunity you will afford for an explanation ; for I do not believe there is anything you would ask *after you have communicated with me*, that I should not be glad to do. I am satisfied that many of the feelings you entertain on the subject in question would be materially altered by granting my request. At the same time, as I have more of your opinions by report than otherwise, I am perhaps not well aware of them. It was only lately that I knew you had any feeling at all on the subject. You would probably find yourself engaged in doing justice to one who cannot help but feel that he has been injured, though he trusts unintentionally. I feel satisfied you are not in possession of all the circumstances of the case, but I am also sure you will not wish willingly to remain ignorant of them. Excuse my earnestness and freedom on this subject, and consider for a moment how much I am interested in it."

At the foot of the copy of this letter Faraday made the following notes :—" In relation to Davy's opposition to my election at the R. S. : Sir H. Davy angry, May 30 ; Phillips's report through Mr. Children, June 5 ; Mr. Warburton called first time, June 5, evening ; I called on Dr. Wollaston, and he not in town, June 9 ; I called on Dr. Wollaston and saw him, June 14 ; I called at Sir H. Davy's, and he called on me, June 17."

Many years ago he gave a friend the following facts, which were written down at the time : Sir H. Davy told him that he must take down his certificate. Faraday replied that he had not put it up : that he could not take it down as it was put up by his proposers. Sir Humphry then said, he must get his proposers to take it down. Faraday answered that he knew they would not do so. Then, said Sir H., I, as President will take it down. Faraday replied, that he was sure Sir Humphry Davy would do what he thought was for the good of the Society.

One of Faraday's proposers told him that Sir H. had walked for an hour round the courtyard of Somerset House, trying to convince Faraday's informant that Faraday ought not to be elected. However, the storm passed away, but not without leaving its effects ; and on the 29th of June Sir H. Davy ends a note—" I am, dear Faraday, very sincerely, your well-wisher and friend."

July 8, Mr. Warburton wrote :—" I have read the article in the Royal Institution Journal, vol. xv. p. 288, on Electromagnetic Rotation, and without meaning to convey to you that I approve of it unreservedly, I beg to say that upon the whole it satisfies me, as I think it will Dr. Wollaston's other friends. Having everywhere admitted and maintained that, on the score of scientific merit, you were entitled to a place in the Royal Society, I

never cared to prevent your election, nor should I have taken any pains to form a party in private to oppose you. What I should have done would have been to take the opportunity, which the proposing to ballot for you would have afforded me, to make remarks in public on that part of your conduct to which I objected. Of this I made no secret, having intimated my intention to some of those from whom I knew you would hear of it, and to the President himself. When I meet with any of those in whose presence such conversation may have passed, I shall state that my objections to you as a Fellow are and ought to be withdrawn, and that I now wish to forward your election."

Aug. 29, Faraday writes to Mr. Warburton :—

"I thank you sincerely for your kindness in letting me know your opinion of the statement; though your approbation of it is not unreserved, yet it very far surpasses what I expected; and I rejoice that you do not now think me destitute of those moral feelings which you remarked to me were necessary in a Fellow of the Royal Society.

"Conscious of my own feelings and the rectitude of my intentions, I never hesitated in asserting my claims, or in pursuing that line of conduct which appeared to me to be right. I wrote the statement under this influence without any regard to the probable result; and I am glad that a step which I supposed would rather tend to aggravate feelings against me has, on the contrary, been the means of satisfying the minds of many, and of making them my friends. Two months ago I had made up my mind to be rejected by the Royal Society as a Fellow, notwithstanding the knowledge I had that many would do me justice; and in the then state of my mind rejection or reception would have been equally indifferent to me. Now that I have experienced so fully the kindness and liberality of Dr. Wollaston, which has been constant throughout the whole of this affair, and that I find an expression of goodwill strong and general towards me, I am delighted by the hope I have of being honoured by Fellowship with the Society; and I thank you sincerely for your promise of support in my election, because I know you would not give it unless you sincerely thought me a fit person to be admitted."

Faraday was the original Secretary of the Athenæum Club; but finding the occupation incompatible with his pursuits, resigned in May 1824. The original prospectus and early list of members have his name attached to them.

This year he was elected Corresponding Member of the Academy of Sciences, Paris, of the Accademia dei Georgofili di Firenze, Honorary Member of the Cambridge Philosophical Society and the British Institution.

Æt. 32 (1824).

Faraday was elected Fellow of the Royal Society, January 8th. This year he published only a historical statement in the Quarterly Journal of Science on the liquefaction of gases, showing that carbonic acid, ammonia,

arseniuretted hydrogen, chlorine, sulphurous acid had been liquefied before his own experiments in 1823. He joined Mr. Brande in the delivery of the morning course of chemical lectures at the Institution. In July he went to the Isle of Wight with Mrs. Faraday, and returned again in August to bring her home. He was elected an Honorary Member of the Cambrian Society of Swansea, and a Fellow of the Geological Society. This year the President and Council of the Royal Society appointed a committee for the improvement of glass for optical purposes, consisting of Fellows of the Royal Society and members of the then Board of Longitude.

Æt. 33 (1825).

Faraday was made Director of the Laboratory of the Royal Institution, and therein he had three or four evening meetings of the members of the Institution, from which came the Friday evening meetings of the members. He was elected a Member of the Royal Institution, and a Corresponding Member of the Society of Medical Chemists, Paris. He had a paper on new compounds of carbon and hydrogen, and on certain other products obtained during the decomposition of oil by heat, read to the Royal Society, and printed in the Transactions; one of these substances was benzol. He had a paper in the Quarterly Journal on some cases of the formation of ammonia, and on the means of testing the presence of minute portions of nitrogen in certain states.

In May a subcommittee, consisting of Mr. Herschel, Mr. Dollond, and Mr. Faraday, was appointed to have the direct superintendence and performance of experiments on the manufacture of optical glass. "It was my business to investigate particularly the chemical part of the inquiry. Mr. Dollond was to work and try the glass, and ascertain practically its good or bad qualities, whilst Mr. Herschel was to examine its physical properties, reason respecting their influence and utility, and make his competent mind bear upon every part of the inquiry. In March 1829 the committee was reduced to two by the retirement of Mr. Herschel, who about that period went to the Continent."

In July he left London by steamboat for Scotland. After visiting the damask works, he went to Leith to see the glass works. He minutely describes the geology of Salisbury Craig, Arthur's Seat, and Craighleith quarries, and then went to Rubislaw (Bleaching Liquor Works), Aberdeen. Here he made many experiments for the proprietors, with whom he stayed.

Æt. 34 (1826).

He had a paper on the Mutual Action of Sulphuric Acid and Naphthaline printed in the Philosophical Transactions, and another on the existence of a limit to Vaporization, and in the Quarterly Journal of Science four papers—on Pure Caoutchouc and the Substances by which it is accompanied in the state of Sap or Juice, on the Fluidity of Sulphur at common temperatures, on a peculiar perspective appearance of aerial light and shade, and on the confinement of Dry Gases over Mercury.

There were seventeen meetings of the members of the Royal Institution held on Friday evenings during this season, and at these Faraday gave seven discourses—on Pure Caoutchouc; on Brunel's Condensed Gas-engine; on Lithography; on the existence of a limit to Vaporization; on Sulphovinic and Sulphonaphthalic Acid; on Drummond's Light; on Brunel's Tunnel at Rotherhithe.

This year he was relieved from the duty of chemical assistant at the lectures given at the Institution, because of his occupation in research, and he was made an honorary member of the Westminster Medical Society.

In his chemical notes there is an analysis of "committee glass" and Saxony gunpowder, and remarks on calico printing and soap making, and soda from common salt.

In July he again was in the Isle of Wight.

Æt. 35 (1827).

Faraday gave his first course of lectures in the theatre of the Institution in April on Chemical Philosophy.

He writes:—"The President and Council of the Royal Society applied to the President and Managers of the Royal Institution for leave to erect on their premises an experimental room with a furnace, for the purpose of continuing the investigation on the manufacture of optical glass. They were guided in this by the desire which the Royal Institution has always evinced to assist in the advancement of science; and the readiness with which the application was granted showed that no mistaken notion had been formed in this respect. As a member of both bodies, I felt much anxiety that the investigation should be successful. A room and furnaces were built at the Royal Institution in September 1827, and an assistant was engaged, Sergeant Anderson of the Royal Artillery. He came on the 3rd of December."

He had four papers in the Quarterly Journal of Science:—1, on the Fluidity of Sulphur and Phosphorus at common temperatures. "In this," he says, "I published some time ago [the year previous] a short account of an instance of the existence of fluid sulphur at common temperatures; and though I thought the fact curious, I did not esteem it of such importance as to put more than my initials to the account. I have just learned through the 'Bulletin Universel' for September, p. 78, that Signor Bellani had observed the same fact in 1813, and published it in the 'Giornale di Fisica.' M. Bellani complains of the manner in which facts and theories which have been published by him are afterwards given by others as new discoveries; and though I find myself classed with Gay-Lussac, Sir H. Davy, Daniell, and Bostock, in having thus erred, I shall not rest satisfied without making restitution, for M. Bellani in this instance certainly deserves it at my hand." 2, on the probable decomposition of certain gaseous compounds of carbon and hydrogen during sudden expansion; 3, on transference of Heat by change of Capacity in Gas; and 4, Experiments on the Nature of Labarraque's

Disinfecting Soda Liquid. There were nineteen Friday evening meetings at the Royal Institution. Faraday gave an account of the magnetic phenomena developed by metals in motion, on the chemical action of chlorine and its compounds as disinfectants, and on the progress of the Thames tunnel. In this year he published his "Chemical Manipulations," in one volume, 8vo. A second edition appeared in 1830, and a third in 1842.

He was made a Correspondent of the Société Philomathique, Paris.

Æt. 36 (1828).

He had a few words in the Quarterly Journal on anhydrous crystals of sulphate of soda. He gave four of the Friday evening lectures: Illustrations of the new Phenomena produced by a current of Air or Vapour recently observed by M. Clement; on the reciprocation of Sound; and also a discourse on the Nature of Musical Sound. The matter belonged to Mr. Wheatstone, but was delivered by Mr. Faraday. The last evening was on the recent and present state of the Thames tunnel.

He was made a Fellow of the Natural Society of Science of Heidelberg.

He was invited to attend the meetings of the Board of Managers of the Institution; and he received his first (gold) medal, one of a series of ten given to Members of the Royal Institution (as a reward for chemical discoveries) by Mr. John Fuller, a Member.

Æt. 37 (1829).

He gave the Bakerian lecture at the Royal Society on the Manufacture of Glass for Optical purposes.

This most laborious investigation led to no good in the direction that was originally expected, but the use of the glass manufactured, as described afterwards, became of the utmost importance in his diamagnetic and magneto-optical researches, and it led to the permanent engagement, in 1832, of Mr. Charles Anderson as Faraday's assistant in all his researches, "to whose rare steadiness, exactitude, and faithfulness in the performance of all that was committed to his charge Faraday was much indebted."

He gave Friday evening discourses on Mr. Robert Brown's discovery of Active Molecules in bodies, either organic or inorganic; on Brard's test of the action of weather on building stones; on Wheatstone's further investigations on the resonances or reciprocal vibrations of volumes of air; on Brunel's block machinery at Portsmouth; on the phonical or nodal figures of elastic laminæ; on the manufacture of glass for optical purposes.

He was made a member of the Scientific Advising Committee of the Admiralty, Patron of the Library of the Institution, Honorary Member of the Society of Arts, Scotland.

At the end of June he writes to Colonel Drummond, Lieutenant-Governor of the Royal Academy, Woolwich:—"I should be happy to undertake the duty of lecturing on chemistry to the gentlemen cadets of Woolwich, provided that the time I should have to take for the purpose from professional

business at home were remunerated by the salary. . . . For these reasons [which he gives] I wish you would originate the terms rather than I. . . . I consider the offer a high honour, and beg you to feel assured of my sense of it. I should have been glad to have accepted or declined it, independent of pecuniary motives; but my time is my only estate, and that which would be occupied in the duty of the situation must be taken from what otherwise would be given to professional business."

At Christmas he for the first time gave the Juvenile Lectures.

Æt. 38 (1830).

This year he had a paper in the *Institution Journal* supplementary to his former paper in 1826 on the limits of vaporization.

His Friday evening discourses were on Aldini's proposed method of preserving men exposed to flame; on the Transmission of Musical sounds through solid conductors and their subsequent reciprocation; on the Flowing of Sand under Pressure; on the application of a New Principle in the Construction of Musical Instruments; on the laws of Coexisting Vibrations in strings and rods, illustrated by the kaleidophone.

The following recollections from about 1823 to 1830 are by Mrs. Faraday's youngest brother, Mr. George Barnard, the artist:—

"All the years I was with Harding I dined at the Royal Institution. After dinner we nearly always had our games just like boys—sometimes at ball, or with horse chestnuts instead of marbles, Faraday appearing to enjoy them as much as I did, and generally excelling us all. Sometimes we rode round the theatre on a velocipede (and tradition remains that in the earliest part of a summer morning Faraday has been seen going up Hampstead Hill on his velocipede).

"At this time we had very pleasant conversaziones of artists, actors, and musicians at Hullmandel's, sometimes going up the river in his eight-oar cutter, cooking our own dinner, enjoying the singing of Garcia and his wife and daughter (afterwards Malibran), indeed of all the best Italian singers, and the society of most of the Royal Academicians, such as Stanfield, Turner, Westall, Landseer, &c.

"After Hullmandel's excellent suppers, served on a dozen or two small tables in his large rooms, we had charades, Faraday and many of us taking parts with Garcia, Malibran, and the rest.

"My first and many following sketching trips were made with Faraday and his wife. Storms excited his admiration at all times, and he was never tired of looking into the heavens. He said to me once, 'I wonder you artists don't study the light and colour in the sky more, and try more for effect.' I think this quality in Turner's drawings made him admire them so much. He made Turner's acquaintance at Hullmandel's, and afterwards often had applications from him for chemical information about pigments. Faraday always impressed upon Turner and other artists the great necessity there was to experiment for themselves, putting washes and tints of all their pig-

ments in the bright sunlight, covering up one half, and noticing the effect of light and gases on the other.

“On one of our sea-side excursions we were bathing together, when Faraday, who was a fair swimmer, on coming in was overtaken by a tremendous wave which overtopped his head, and dashed him with violence on the beach, bruising him much. He impressed on me never to think any one could stand against such a breaker; that one should turn round and dive through it, throwing one's self off the ground. Faraday did not fish at all during these country trips, but just rambled about geologizing or botanizing.”

If Faraday's scientific life had ended here it might well have been called a noble success. He had made two leading discoveries, the one on electro-magnetic motions, the other on the condensation of several gases into liquids. He had carried out two important and most laborious investigations on the alloys of steel and on the manufacture of optical glass. He had made many communications to the Royal Society, and many more to the Quarterly Journal of Science. From assistant in the laboratory he had become its director. He was constantly lecturing in the great theatre, and he had probably prolonged the existence of the Royal Institution by taking the most active part in the establishment of the Friday evening meetings.

But when we turn to the eight volumes of manuscripts of his ‘Experimental Researches,’ which he bequeathed to the Royal Institution, we find that he was just going to begin to work. The first of these large folio volumes begins in 1831 with paragraph 1, and continues in the seventh to paragraph 15,389 in 1856. The results of this work he has collected himself in four volumes octavo. The three volumes on electricity were published in 1839, in 1844, and in 1855; the last volume, on chemistry and physics, he published in 1859. Whenever he was about to investigate a subject, he wrote out, on separate slips of paper, different queries regarding it which his genius made him think were “naturally possible” to be answered by experiment. He slightly fixed them one beneath another, in the order in which he intended to experiment. As a slip was answered it was removed, and others were added in the course of the investigation, and these in their turn were worked out and removed. If no answer was obtained, the slip remained to be returned to at another time. Out of the answers the manuscript volumes were formed, and from these the papers were written for the Royal Society, where they were always read before the popular account of them was given to the Royal Institution at a Friday evening meeting.

When nearly fifty years of age, he became so seriously troubled with want of memory and giddiness that he thought he should be unable to do any more, and in his most exact way he drew up the following table of the work he had given up temporarily during the first ten years that his experimental investigations in electricity had lasted:—

1841	1840	1839	1838	1837	1836	1835	1834	1833		
										May give up Easter lectures and all other business at Royal Institution.
										Gave up Friday evenings.
										Gave up juvenile lectures.
										Gave up Mr. Brande's twelve morning lectures.
										Closed three days in the week.
										Declined reprinting 'Chemical Manipulation.'
										Gave up many morning lectures.
										Gave up the rest of professional business.
										Gave up excuse business.
										Declined all dining-out invitations.
										Gave up professional business in courts.
										Declined Council business at Royal Society.

Æt. 39 (1831).

In this year the first series of 'Experimental Researches in Electricity' was read to the Royal Society. It contained experiments (1) on the Induction of Electric Currents, (2) on the Evolution of Electricity from Magnetism, (3) on a new Electrical Condition of Matter, and on Arago's Magnetic Phenomena. He had also in the Transactions a paper on a peculiar class of acoustical figures, and on certain forms assumed by groups of particles upon vibrating elastic surfaces. In the Quarterly Journal of Science he had a paper on a peculiar class of optical deceptions, which gave rise to the chromatrope.

He gave five Friday discourses on a peculiar class of Optical Deceptions; on Oxalamide, discovered by M. Dumas; on Light and Phosphorescence (being an account of experiments recently made in the Royal Institution by Mr. Pearshall, Chemical Assistant); on Trevelyan's recent Experiments on the production of Sound during the conduction of Heat; and on the Arrangements assumed by Particles upon Vibrating Elastic Surfaces.

He was elected an Honorary Member of the Imperial Academy of Sciences, Petersburg.

In a letter to his friend, Richard Phillips, he first complains of his memory. "My memory gets worse and worse daily, I will not therefore say I have not received your Pharmacopœia." Three months later he thanks him for the last edition of the Pharmacopœia, and says, "I am busy just now again on electro-magnetism, and think I have got hold of a good thing, but can't say. It may be a weed instead of a fish that, after all my labour, I may at last pull up. I think I know why metals are magnetic when in motion, though not (generally) when at rest."

Nov. 29.—Two months later he again writes, and this time from

Brighton:—"We are here to refresh. I have been working and writing a paper that always knocks me up in health, but now I feel well again and able to pursue my subject, and now I will tell you what it is about. The title will be, I think, 'Experimental Researches in Electricity':—I. On the Induction of Electric Currents; II. On the Evolution of Electricity from Magnetism; III. On a new Electrical Condition of Matter; IV. On Arago's Magnetic Phenomena. There is a bill of fare for you, and, what is more, I hope it will not disappoint you. Now the pith of all this I must give you very briefly, the demonstrations you shall have in the paper when printed.

"I. When an electric current is passed through one of two parallel wires, it causes at first a current in the same direction through the other, but this induced current does not last a moment, notwithstanding the inducing current (from the voltaic battery) is continued; all seems unchanged, except that the principal current continues its course. But when the current is stopped, then a return current occurs in the wire under induction, of about the same intensity and momentary duration, but in the opposite direction to that first formed. Electricity in currents therefore exerts an inductive action like ordinary electricity, but subject to peculiar laws. The effects are a current in the same direction when the induction is established, a reverse current when the induction ceases, and a *peculiar state* in the interim. Common electricity probably does the same thing; but as it is at present impossible to separate the beginning and the end of a spark or discharge from each other, all the effects are simultaneous and neutralize each other.

"II. Then I found that magnets would induce just like voltaic currents, and by bringing helices and wires and jackets up to the poles of magnets, electrical currents, were produced in them, these currents being able to deflect the galvanometer, or to make, by means of the helix, magnetic needles, or in one case even to give a spark. Hence the evolution of *electricity from magnetism*. The currents were not permanent; they ceased the moment the wires ceased to approach the magnet, because the new and apparently quiescent state was assumed just as in the case of the induction of current; but when the magnet was removed, and its induction therefore ceased, the return currents appeared as before. These two kinds of induction I have distinguished by the terms *volta-electric* and *magneto-electric* induction. Their identity of action and results is, I think, a very powerful proof of M. Ampère's theory of magnetism.

"III. The new electrical condition which intervenes by induction between the beginning and end of the inducing current gives rise to some very curious results. It explains why chemical action or other results of electricity have never been as yet obtained in trials with the magnet. In fact the currents have no sensible duration. I believe it will explain perfectly the *transference of elements* between the poles of the pile in decomposition; but this part of the subject I have reserved until the present experiments are com-

pleted ; and it is so analogous, in some of its effects, to those of Ritter's secondary piles, De la Rive and Van Beck's peculiar properties of the poles of a voltaic pile, that I should not wonder if they all proved ultimately to depend on this state. The condition of matter I have dignified by the term *Electrotonic*, THE ELECTROTONIC STATE. What do you think of that? Am I not a bold man, ignorant as I am, to coin words? but I have consulted the scholars, and now for IV.

"IV. The new state has enabled me to make out and explain all Arago's phenomena of the rotating magnet or copper plate. I believe, perfectly ; but as great names are concerned (Arago, Babbage, Herschel, &c.), and as I have to differ from them, I have spoken with that modesty which you so well know you and I and John Frost * have in common, and for which the world so justly commends us. I am even half afraid to tell you what it is. You will think I am hoaxing you, or else in your compassion you may conclude I am deceiving myself. However, you need do neither, but had better laugh, as I did most heartily, when I found that it was neither attraction nor repulsion, but just one of my *old rotations* in a new form. I cannot explain to you all the actions, which are very curious ; but in consequence of the electrotonic state being assumed and lost as the parts of the plate whirl under the pole, and in consequence of magneto-electric induction, currents of electricity are formed in the direction of the radii,—continuing, for simple reasons, as long as the motion continues, but ceasing when that ceases. Hence the wonder is explained that the metal has powers on the magnet when moving, but not when at rest. Hence is also explained the effect which Arago observed, and which made him contradict Babbage and Herschel, and say the power was repulsive ; but, as a whole, it is really tangential. It is quite comfortable to me to find that experiment need not quail before mathematics, but is quite competent to rival it in discovery ; and I am amazed to find that what the high mathematicians have announced as the *essential condition* to the rotation, namely, that *time is required*, has so little foundation, that if the time could by possibility be anticipated instead of being required, *i. e.* if the currents could be formed *before* the magnet came over the place instead of *after*, the effect would equally ensue. Adieu, dear Phillips. Excuse this egotistical letter from yours, very faithfully."

Æt. 40 (1832).

The second series of Experimental Researches in Electricity was this year the Bakerian lecture on Terrestrial Magneto-electric Induction, and on the Force and Direction of Magneto-electric Induction generally.

His Friday discourses were, (1) on Dr. Johnson's Researches on the Reproductive Power of Planariæ ; (2) recent experimental Investigation of Volta-electric and Magneto-electric Induction ; (3) Magneto-electric In-

* A pushing acquaintance, who, without claim of any kind, got himself presented at Court.

duction, and the explanation it affords of Arago's Phenomena of Magnetism exhibited by moving Metals ; (4) Evolution of Electricity, naturally and artificially, by the inductive action of the Earth's Magnetism ; (5) on the Crispation of Fluids lying on vibrating Surfaces ; and on Morden's Machinery for manufacturing Bramah's locks.

He was made Hon. Member of Philadelphia College of Pharmacy, and of Chemical and Physical Society, Paris ; Fellow of the American Academy of Arts and Sciences, Boston ; Member of the Royal Society of Science, Copenhagen ; D.C.L. of Oxford University ; and he received the Copley medal.

He collected the different papers, notes, notices, &c. published in octavo up to this year, and he added this preface to the volume :—" Papers of mine published in octavo in the Quarterly Journal of Science and elsewhere, since the time that Sir H. Davy encouraged me to write the 'Analysis of Caustic Lime.' Some I think (at this date) are good, others moderate, and some bad ; but I have put *all* into the volume, because of the utility they have been to me, and none more than the bad in pointing out to me in future, or rather after times, the faults it became me to watch and avoid. As I never looked over one of my papers a year after it was written without believing, both in philosophy and manner, it would have been much better done, I still hope this collection may be of great use to me."

In December, the Royal Institution being in trouble, a committee reported on all the salaries. "The Committee are certainly of opinion that no reduction can be made in Mr. Faraday's salary, £100 per annum, house, coals, and candles, and beg to express their regret that the circumstances of the Institution are not such as to justify their proposing such an increase of it as the variety of duties which Mr. Faraday has to perform, and the zeal and ability with which he performs them, appear to merit."

Æt. 41 (1833).

The third series of Experimental Researches contained the Identity of Electricities derived from different sources, and the relation by measure of common and voltaic electricity. The fourth series consisted of a new law of Electric Conduction, and on Conducting-power generally. The fifth series was on Electro-chemical Decomposition, new conditions of Electro-chemical Decomposition, influence of Water in Electro-chemical Decomposition, and Theory of Electro-chemical Decomposition. The sixth series was on the Power of Metals and other Solids to induce the combination of gaseous bodies.

He sent a short note to the editors of the Philosophical Magazine on a means of preparing the Organs of Respiration so as considerably to extend the time of holding the breath, with remarks on its application in cases in which it is required to enter an irrespirable atmosphere, and on the precautions necessary to be observed in such cases.

His Friday discourses were on the Identity of Electricity derived from different sources ; on the practical prevention of Dry Rot in Timber ; on the investigation of the Velocity and Nature of the Electric Spark and Light by Wheatstone ; on Mr. Brunel's new mode of constructing Arches for Bridges ; on the mutual relations of Lime, Carbonic Acid, and Water ; on a new law of Electric Conduction ; and on the power of Platina and other solid substances to determine the combination of gaseous bodies.

In the early part of the year Mr. Fuller had founded a professorship of chemistry at the Royal Institution, with a salary of about £100 a year. Mr. Faraday was appointed for his life, with the privilege of giving no lectures. He was made Corresponding Member of the Royal Academy of Sciences of Berlin, and Hon. Member of the Hull Philosophical Society.

Æt. 42 (1834).

The seventh series of Experimental Researches was on Electro-chemical Decomposition (continued) : on some general conditions of Electro-decomposition ; on a new measure of Volta Electricity ; on the Primary and Secondary character of bodies evolved in Electro-decomposition ; on the definite nature and extent of Electro-chemical Decomposition ; on the absolute quantity of Electricity associated with the Particles or Atoms of Matter.

The eighth series was on the Electricity of the Voltaic Pile, its source, quantity, and general characters ; on simple Voltaic Circles ; on the Intensity necessary for Electrolyzation ; on associated Voltaic Circles on the Voltaic Battery ; on the resistance of an Electrolyte to Electrolytic Action ; general remarks on the active Voltaic Battery. The ninth series was on the influence by induction of an Electric Current on itself, and on the inductive action of Electric Currents generally.

He gave four Friday discourses, the first on the principle and action of Ericsson's Caloric engine. The other lectures were on Electro-chemical Decomposition ; on the definite action of Electricity ; and on new applications of the products of Caoutchouc.

He was made Foreign Corresponding Member of the Academy of Sciences and Literature of Palermo.

Æt. 43 (1835).

The tenth series of Experimental Researches was on an improved form of the Voltaic Battery, some practical results respecting the Construction and Use of the Voltaic Battery.

He gave Friday discourses on Melloni's recent discoveries on Radiant Heat ; on the Induction of Electric Currents ; on the Manufacture of Pens from Quills and Steel, illustrated by Morden's machinery ; on the Condition and Use of the Tympanum of the Ear.

In July he went with Mrs. Faraday from Brighton to Dieppe, spending a week in Paris, and some days at Geneva ; he stayed two days at Chamouni. He writes to his friend Magrath :—" We are almost surfeited with

magnificent scenery ; and for myself I would rather not see it than see it with an exhausted appetite. The weather has been most delightful, and everything in our favour, so that the scenery has been in the most beautiful condition. Mont Blanc, above all, is wonderful, and I could not but feel, what I have often felt before, that painting is very far beneath poetry in cases of high expression, of which this is one. No artist should try to paint Mont Blanc, it is utterly out of his reach. He cannot convey an idea of it, and a formal map, or a common-place model, conveys more intelligence, even with respect to the sublimity of the mountain, than his highest efforts can do ; in fact he must be able to dip his brush in light and darkness before he can paint Mont Blanc. Yet the moment one sees it Lord Byron's expressions come to mind, and they seem to apply. The poetry and the subject dignify each other."

On the 20th of April Sir James South wrote to him to say that he would have a letter from Sir Robert Peel acquainting him with the fact that, had Sir R. Peel remained in office, a pension would have been given him. On the 23rd he wrote a letter to Sir James South, which, however, his father-in-law prevented him from sending. He said, "I hope you will not think that I am unconscious of the good you meant me, or undervalue your great exertions for me, when I say that I cannot accept a pension whilst I am able to work for my living. Do not from this draw any sudden conclusion that my opinions are such and such. I think that Government is right in rewarding and sustaining science. I am willing to think, since such approbation has been intended me, that my humble exertions have been worthy, and I think that scientific men are not wrong in accepting the pensions ; but still I may not take a pay which is not for services performed whilst I am able to live by my labours."

In the 'Times' of Saturday, 28th Oct. 1835, under the head of Tory and Whig Patronage to Science and Literature, is the following conversation, copied from Fraser's Magazine :—

"*Mr. F.* I am here, my Lord, by your desire ; am I to understand that it is on the business which I have partially discussed with Mr. Young? (Lord M.'s Secretary.) *Lord Melbourne.* You mean the pension, don't you? *Mr. F.* Yes, my Lord. *Lord M.* Yes, you mean the pension, and I mean the pension too. I hate the name of the pension. I look upon the whole system of giving pensions to literary and scientific persons as a piece of gross humbug ; it was not done for any good purpose, and never ought to have been done. It is a gross humbug from beginning to end. *Mr. F.* (rising, and making a bow). After all this, my Lord, I perceive that my business with your Lordship is ended. I wish you a good morning." Faraday said that the report of this conversation was full of error ; however he wrote :—

"*To the Right Hon. Lord Viscount Melbourne, First Lord of the Treasury.*

"October 26.

"My Lord,—The conversation with which your Lordship honoured me

this afternoon, including, as it did, your Lordship's opinion of the general character of the pensions given of late to scientific persons, induces me respectfully to decline the favour which I believe your Lordship intends for me; for I feel that I could not, with satisfaction to myself, accept at your Lordship's hands that which, though it has the form of approbation, is of the character which your Lordship so pithily applied to it."

This note, Mr. F. says, "was left by myself, with my card, at Lord Melbourne's office on the same evening, *i. e.* of the day of our conversation."

On the 6th of November Faraday wrote to Sir James South:—

"And now, my dear Sir, pray let me drop I know you have serious troubles of your own. Do not let me be one any longer either to you or to others. You have my most grateful feelings for all the kindness you have shown to him who is ever truly yours."

The intervention of Miss Fox and Lady Mary Fox, caused Lord Melbourne to write the following letter:—

"November 24.

"Sir,—It was with much concern that I received your letter declining the offer which I considered myself to have made in the interview which I had with you in Downing Street, and it was with still greater pain that I collected from that letter that your determination was founded upon the certainly imperfect, and perhaps too blunt and inconsiderate manner in which I had expressed myself in our conversation. I am not unwilling to admit that anything in the nature of censure upon any party ought to have been abstained from upon such an occasion; but I can assure you that my observations were intended only to guard myself against the imputation of having any political advantage in view, and not in any respect to apply to the conduct of those who had or hereafter might avail themselves of a similar offer. I intended to convey that, although I did not entirely approve of the motives which appeared to me to have dictated some recent grants, yet that your scientific character was so eminent and unquestionable as entirely to do away any objection which I might otherwise have felt, and to render it impossible that a distinction so bestowed could be ascribed to any other motive than a desire to reward acknowledged desert and to advance the interest of philosophy.

"I cannot help entertaining a hope that this explanation may be sufficient to remove any unpleasant or unfavourable impression which may have been left upon your mind, and that I shall have the satisfaction of receiving your consent to my advising His Majesty to grant to you a pension equal in amount to that which has been conferred upon Professor Airy and other persons of distinction in science and literature."

The same day Faraday wrote:—"My Lord, your Lordship's letter, which I have just had the honour to receive, has occasioned me both pain and pleasure—pain, because I should have been the cause of your Lordship's writing such a one, and pleasure, because it assures me that I am not unworthy of your Lordship's regard.

“As, then, your Lordship feels that, by conferring on me the mark of approbation hinted at in your letter, you will be at once discharging your duty as First Minister of the Crown, and performing an act consonant with your own kind feelings, I hesitate not to say I shall receive your Lordship’s offer both with pleasure and with pride.”

The pension was granted December 24, but in the interval he was much troubled by some, who thought that a contradiction to the injurious statement in the ‘Times’ against Lord Melbourne ought to be made.

To one Faraday writes :—“The pension is a matter of indifference to me, but other results, some of which have already come to pass, are not so. The continued renewal of this affair, to my mind, tempts me at times to what might be thought very ungenerous under the circumstances, namely, even at this late hour a determined refusal of the whole.”

On the 8th of December he, however, published a letter in the ‘Times,’ in which he says, “I beg leave thus publicly to state that neither directly nor indirectly did I communicate to the Editor of Fraser’s Magazine the information on which that article (an extract of which was published in the ‘Times’ of the 28th) was founded, or further, either directly or indirectly, any information to or for any publication whatsoever.”

This year he was made Corresponding Member of the Royal Academy of Medicine, Paris; Hon. Member of the Royal Society of Edinburgh, Institution of British Architects, and Physical Society of Frankfort; Hon. Fellow of the Medico-Chirurgical Society of London; and he was awarded one of the Royal Medals by the Royal Society.

Æt. 44 (1836).

This year the whole course of Faraday’s scientific work was changed by his appointment as Adviser to the Trinity House. He published one paper in the Philosophical Magazine on the general Magnetic Relations and Characters of the Metals, which he begins by saying, “general views have long since led me to an opinion, which is probably also entertained by others, though I do not remember to have met with it, that *all* the metals are magnetic in the same manner as iron.”

He gave four Friday discourses on Silicified Plants and Fossils; on Magnetism of Metals as a general character; on Plumbago, and on Pencils, Morden’s Machinery; and considerations respecting the nature of Chemical Elements.

The 3rd of February he wrote to Capt. Pelly, Deputy Master of the Trinity House:—

“I consider your letter to me as a great compliment, and should view the appointment at the Trinity House, which you propose, in the same light; but I may not accept even honours without due consideration.

“In the first place, my time is of great value to me, and if the appointment you speak of involved anything like periodical routine attendances, I do not think I could accept it. But if it meant that in consultation, in the

examination of proposed plans and experiments, in trials, &c. made as my convenience would allow, and with an honest sense of a duty to be performed, then I think it would consist with my present engagements. You have left the title and the sum in pencil. These I look at mainly as regards the character of the appointment; you will believe me to be sincere in this, when you remember my indifference to your proposition as a matter of interest, though *not as a matter of kindness*.

“In consequence of the goodwill and confidence of all around me I can at any moment convert my time into money, but I do not require more of the latter than is sufficient for necessary purposes. The sum therefore of £200 is quite enough in itself, but not if it is to be the indicator of the character of the appointment; but I think you do not view it so, and that you and I understand each other in that respect; and your letter confirms me in that opinion. The position which I presume you would wish me to hold is analogous to that of a standing counsel.

“As to the title, it might be what you pleased almost. Chemical adviser is too narrow; for you would find me venturing into parts of the philosophy of light not chemical. Scientific adviser you may think too broad (or in me too presumptuous); and so it would be, if by it was understood all science. It was the character I held with two other persons at the Admiralty Board in its former constitution.

“The thought occurs to me whether, after all, you want such a person as myself. This you must judge of; but I always entertain a fear of taking an office in which I may be of no use to those who engage me. Your applications are, however, so practical, and often so chemical, that I have no great doubt in the matter.”

On the 4th he was made Scientific Adviser in experiments on lights to the Corporation.

For thirty years nearly he held this post. What he did may be seen in the portfolios, full of manuscripts, which Mrs. Faraday has given to the Trinity House, in which, by the marvellous order and method of his notes and indices, each particle of his work can be found and consulted immediately.

His first work was to make a photometer. Throughout the whole year he was busy on the subject, making three photometers, and ascertaining the capability and accuracy of the instruments. He also experimented on the preparation of oxygen for the Bude light, drawing up the most exact tables for the record of the manufacture; for example, the 10th of November he says, “hence oxygen costs very nearly twopence per cubical foot; exactly 1·909 pence.”

He was made Senator of the University of London; Hon. Member of the Society of Pharmacy of Lisbon and of the Sussex Royal Institution; Foreign Member of the Society of Sciences of Modena, and the Natural-History Society of Basle.

Æt. 45 (1837).

This year the ‘Eleventh Series of Experimental Researches in Electricity’

was communicated to the Royal Society. It was on Induction : Induction an action of contiguous particles ; absolute charge of Matter ; Electrometer and Inductive Apparatus employed ; Induction in Curved Lines ; Specific Inductive Capacity ; general results as to Induction.

His work for the Trinity House consisted in examining the Trinity lamp, the French lamp, and the Bude lamp, as to intensity of light and price : “ pressed Mr. Gurney, by letter, to give us his best lamp at once and not to lose time.” Two of his four Friday discourses were on the views of Professor Mossotti as to one general law accounting for the different Forces in Matter ; on Dr. Marshall Hall’s views of the Nervous System.

He was elected Honorary Member of the Literary and Scientific Institution, Liverpool.

Æt. 46 (1838).

The twelfth series of Researches was published this year.—On Induction (continued) : Conduction or Conductive Discharge ; Electrolytic Discharge ; Disruptive Discharge, Insulation, Spark, Brush, Difference of Discharge at the positive and negative surfaces of conductors. The thirteenth series was also on Induction (continued) : Disruptive Discharge (continued). Peculiarities of positive and negative discharge either as spark or brush ; Glow Discharge ; Dark Discharge. Convection or Carrying Discharge. Relation of a vacuum to Electrical Phenomena. Nature of the Electrical Current. The fourteenth series was on the nature of the Electric Force or Forces. Relation of the Electric and Magnetic Forces, and notes on Electrical Excitation. The fifteenth series was a notice of the character and direction of the Electric Force of the Gymnotus.

For the Trinity House he a second time reported on the new Gurney lamp, comparing it in light and cost with the French lamp.

He gave four Friday discourses this year.

He was made Honorary Member of the Institution of Civil Engineers ; Foreign Member of the Royal Academy of Sciences, Stockholm ; and he received the Copley Medal.

Æt. 47 (1839).

At the end of July he was four days at Orfordness for the Trinity House, measuring and comparing at sea and on land the Argand lamp, the French lamp, and the Bude lamp.

He gave four Friday discourses, two of which were on the Electric powers of the Gymnotus and Silurus. An account of Gurney’s oxv-oil-lamp.

During thirteen years, Miss Reid, a niece of Mrs. Faraday s, had lived at the Institution, and she has thus given her recollections of Mr. Faraday during these and the following six years :—

“ There could be very few regular lessons at the Institution ; there were so many breaks and interruptions. Sometimes my uncle would give me a few sums to do, and he always tried to make me understand the why and wherefore of everything I did. Then occasionally he gave me a reading-lesson. How patient he was, and how often he went over and over the

same passage when I was unusually dense. He had himself taken lessons from Smart, and he used to practise reading with exaggerated emphasis occasionally.

“In the earlier days of the juvenile lectures he used to encourage me to tell him everything that struck me, and where my difficulties lay when I did not understand him fully. In the next lecture he would enlarge on those especial points, and he would tell me my remarks had helped him to make things clear to the young ones. He never mortified me by wondering at my ignorance, never seemed to think how stupid I was. I might begin at the very beginning again and again; his patience and kindness were unfailing.

“A visit to the laboratory used to be a treat when the busy time of the day was over.

“We often found him hard at work on experiments connected with his researches, his apron full of holes. If very busy he would merely give a nod, and aunt would sit down quietly with me in the distance, till presently he would make a note on his slate and turn round to us for a talk, or perhaps he would agree to come upstairs to finish the evening with a game at bagatelle, stipulating for half an hour's quiet work first to finish his experiment. He was fond of all ingenious games, and he always excelled in them. For a time he took up the Chinese puzzle, and, after making all the figures in the book, he set to work and produced a new set of figures of his own, neatly drawn, and perfectly accurate in their proportions, which those in the book were not. Another time, when he had been unwell, he amused himself with *Papyro-plastics*, and with his dexterous fingers made a chest of drawers and pigeon-house, &c.

“When dull and dispirited, as sometimes he was to an extreme degree, my aunt used to carry him off to Brighton, or somewhere, for a few days, and they generally came back refreshed and invigorated. Once they had very wet weather in some out of the way place, and there was a want of amusement, so he ruled a sheet of paper and made a neat draught-board, on which they played games with pink and white lozenges for draughts. But my aunt used to give up almost all the games in turn, as he soon became the better player, and, as she said, there was no fun in being always beaten. At bagatelle, however, she kept the supremacy, and it was long a favourite, on account of its being a cheerful game requiring a little moving about.

“Often of an evening they would go to the Zoological Gardens and find interest in all the animals, especially the new arrivals, though he was always much diverted by the tricks of the monkeys. We have seen him laugh till the tears ran down his cheeks as he watched them. He never missed seeing the wonderful sights of the day—acrobats and tumblers, giants and dwarfs; even Punch and Judy was an unfailing source of delight, whether he looked at the performance or at the admiring gaping crowd.

“He was very sensitive to smells; he thoroughly enjoyed a cabbage

rose, and his friends knew that one was sure to be a welcome gift. Pure Eau de Cologne he liked very much; it was one of the few luxuries of the kind that he indulged in; musk was his abhorrence, and the use of that scent by his acquaintance annoyed him even more than the smell of tobacco, which was sufficiently disagreeable to him. The fumes from a candle or oil-lamp going out would make him very angry. On returning home one evening, he found his rooms full of the odious smell from an expiring lamp; he rushed to the window, flung it up hastily, and brought down a whole row of hyacinth-bulbs and flowers and glasses.

“Mr. Magrath used to come regularly to the morning lectures, for the sole purpose of noting down for Mr. F. any faults of delivery or defective pronunciation he could detect. The list was always received with thanks; although his corrections were not uniformly adopted, he was encouraged to continue his remarks with perfect freedom. In early days he always lectured with a card before him with *Slow* written upon it in distinct characters. Sometimes he would overlook it and become too rapid; in this case Anderson had orders to place the card before him. Sometimes he had the word ‘Time’ on a card brought forward when the hour was nearly expired.”

Æt. 48 (1840).

Early in this year the sixteenth series of Experimental Researches appeared. It was on the Source of Power in the Voltaic Pile:—1. Exciting electrolytes, &c., being conductors of thermo and feeble currents; 2. Inactive Conducting Circles containing an electrolytic fluid; 3. Active Circles excited by solution of Sulphuret of Potassium. The seventeenth series came a few days after. Also on the Source of Power in the Voltaic Pile (continued): 4. The exciting Chemical Force by temperature; 5. The exciting Chemical Force affected by dilution; 6. Differences in the Order of the Metallic Elements of Voltaic Circles; 7. Active Voltaic Circles and Batteries without metallic contact; 8. Considerations of the sufficiency of chemical action; 9. Thermoelectric evidence; 10. Improbable nature of the assumed Contact Force.

He gave three Friday discourses.

The previous year, Dr. Hare, Professor of Chemistry in the University of Pennsylvania, wrote his objections to Faraday’s theoretical opinions on Static Induction. At the end of Faraday’s reply, he says:—“The paragraphs which remain unanswered refer, I think, only to differences of opinion, or else not even to differences, but opinions regarding which I have not ventured to judge. These opinions I esteem of the utmost importance; but that is a reason which makes me the rather desirous to decline entering upon their consideration, inasmuch as on many of their connected points I have formed no decided notion, but am constrained by ignorance and the contrast of facts to hold my judgment as yet in suspense. It is indeed to me an annoying matter to find how many subjects there are in electrical science on which, if I were asked for an opinion, I should have to say I

cannot tell—I do not know ; but, on the other hand, it is encouraging to think that these are they which, if pursued industriously, experimentally, and thoughtfully, will lead to new discoveries. Such a subject, for instance, occurs in the currents produced by dynamic induction, which you say it will be admitted do not require for their production intervening ponderable atoms. For my own part, I more than half incline to think they do require these intervening particles. But on this question, as on many others, I have not yet made up my mind.”

On the 1st of January the following year, Dr. Hare sent a reply. In Faraday’s answer to this, he says:—“ You must excuse me, however, for several reasons, from answering it at any length. The first is my distaste for controversy, which is so great that I would on no account our correspondence should acquire that character. I have often seen it do great harm, and yet remember few cases in natural knowledge where it has helped much either to pull down error or advance truth. Criticism, on the other hand, is of much value ; and when criticism such as yours has done its duty, then it is for other minds than those either of the author or critic to decide upon and acknowledge the right.”

This year he reported to the Trinity House on the necessity and method of examining lighthouse dioptric arrangements, and he had to examine the apparatus intended for Gibraltar. Between Purfleet and Blackwall he made a long comparison between English and French reflecting lamps and between English and French refracting prisms.

To Professor Auguste De la Rive, the son of his early friend, he wrote :—“ Though a miserable correspondent I take up my pen to write to you, the moving feeling being a desire to congratulate you on your discernment, perseverance, faithfulness, and success in the cause of *Chemical Excitement of the current in the Voltaic Battery*. You will think it is rather late to do so ; but not under the circumstances. For a long time I had not made up my mind ; then the facts of definite electrochemical action made me take part with the supporters of the chemical theory, and since then Marianini’s paper with reference to myself has made me read and experiment more generally on the point in question. In the reading, I was struck to see how soon, clearly, and constantly you had and have supported that theory, and think your proofs and reasons most excellent and convincing. The constancy of Marianini and of many others on the opposite side made me, however, think it not unnecessary to accumulate and record evidence of the truth, and I have therefore written two papers, which I shall send you when printed, in which I enter under your banners as regards the origin of electricity or of the current in the pile. My object in experimenting was, as I am sure yours has always been, not so much to support a given theory as to learn the natural truth ; and having gone to the question unbiassed by any prejudices, I cannot imagine how any one whose mind is not preoccupied by a theory, or a strong bearing to a theory, can take part with that of contact against that of chemical action. How-

ever, I am perhaps wrong saying so much, for, as no one is infallible, and as the experience of past times may teach us to doubt a theory which seems to be most unchangeably established, so we cannot say what the future may bring forth in regard to these views."

He was made Member of the American Philosophical Society, Philadelphia, and Honorary Member of the Hunterian Medical Society, Edinburgh.

He was in the autumn of this year ordained Elder in the Sandemanian Church, and he held the office three years and a half.

Æt. 49 (1841).

On the 2nd of September Faraday went down to St. Catherine's lighthouse in the Isle of Wight, to remedy the condensation of moisture on the glass in the inside. On the 6th he returned home, "quite satisfied with the chimney, and have no doubt we shall have a lantern quite clear from sweat, and also much cleaner, both as to the mirrors and roof, from soot and blackness, than heretofore."

The 30th of June he left London for three months, with Mrs. Faraday and Mr. and Mrs. George Barnard, for Ostend and Switzerland. The journal which he kept contains many most beautiful descriptions. That of Brienz Lake and the Giessbach is perhaps one of the most striking:—"George and I crossed the lake in a boat to the Giessbach, he to draw and I to saunter. The day was fine, but the wind against the boat; and these boats are so cumbrous, and at the same time expose so much surface to the air, that we were about two hours doing the two miles, with two men and occasionally our own assistance at the oar. We broke the oar-band; we were blown back and sideways. We were drawn against the vertical rock in a place where the lake is nearly 1000 feet deep; and I might tell a true tale, which would sound very serious, yet after all there was nothing of any consequence but delay. But such is the fallacy of description. We reached the fall and found it in its grandeur; for, as much rain fell last night, there was perhaps half as much more water than yesterday. This most beautiful fall consists of a fine river, which passes by successive steps down a very deep precipice into the lake. In some of these steps there is a clear leap of water 100 feet or more; in others, most beautiful combinations of leap, cataract, and rapid—the finest rocks occurring at the sides and bed of the torrent. In one part a bridge passes over it; in another a cavern and path occur under it. To-day every fall was foaming from the abundance of water, and the current of wind brought down by it was in some parts almost too strong to stand against. The sun shone brightly, and the rainbows seen from various parts were very beautiful. One at the bottom of a fine but furious fall was very pleasant; there it remained motionless, whilst the gusts and clouds of spray swept furiously across its place and were dashed against the rock. It looked like a spirit strong in faith and steadfast in the midst of the storm of passions sweeping across it; and

though it might fade and revive, still it held on to the rock as in hope and giving hope, and the very drops which, in the whirlwind of their fury, seemed as if they would carry all away, were made to revive it and give it greater beauty. How often are the things we fear and esteem as troubles made to become blessings to those who are led to receive them with humility and patience! In one part of the fall the effect of the current of air was very curious. The great mass of water fell into a foaming basin, but some diverted portions struck the rock opposite the observer, and, collecting, left it at the various projecting parts; but, instead of descending, these hundred little streams rushed upwards into the air, as if urged by a force the reverse of gravity; and as there was little other spray in this part, it did not at first occur to the mind that this must be the effect of a powerful current of air, which, having been brought down by the water, was returning up that face of the rock."

Into the pages of this journal he has fixed, with the most extreme neatness, the different mountain-flowers that he gathered in his walks.

Mrs. Faraday wrote for him part of a letter to Mr. Magrath:—"I think Mr. Young would be quite satisfied with the way my husband employs his time. He certainly enjoys the country exceedingly; and though at first he lamented our absence from home and friends very much, he seems now to be reconciled to it as a means of improving his general health. His strength is, however, very good. He thinks nothing of walking thirty miles in a day, and one day he walked forty-five, which I protested against his doing again, though he was very little the worse for it. I think that is too much. What would Mr. Young say to that; but the grand thing is rest and relaxation of mind, which he is really taking." He finishes the letter himself:—"Though my wife's letter will tell you pretty well all about us, yet a few lines from an old friend (though somewhat worn out) will not be unpleasant to one who, like that friend, is a little the worse for time and hard wear. However, if you jog on as well as we do, you will have no cause for grumbling, by which I mean to say that I certainly have not; for the comforts that are given me, and, above all, the continued kindness, affection, and forbearance of friends towards me, are, I think, such as few experience. Remember me most kindly to Mr. Young. I will give no opinion at present as to the effect of his advice on my health and memory; but I can have only one feeling as to his kindness, and, whatever I may forget, I think I shall not forget that. Now, as to the main point of this trip, *i. e.* the mental idleness, you can scarcely imagine how well I take to it, and what a luxury it is. The only fear I have is that when I return friends will begin to think that I shall overshoot the mark; for feeling that any such exertion is a strain upon that faculty, which I cannot hide from myself is getting weaker, namely, memory, and feeling that the less exertion I make to use that the better I am in health and head, so my desire is to remain indolent, mentally speaking, and to retreat from a position which should only be held by one who has the *power* as

well as the will to be active. All this, however, may be left to clear itself up as the time proceeds."

Æt. 50 (1842).

He resumed the Friday evening lectures, and gave one on the Conduction of Electricity in Lightning-rods, and one on the Principles and Practice of Hullmandel's Lithotint. This year he made four reports to the Trinity House :—1, on comparison of the amount of Light cut off by French glass and by Newcastle glass ; 2, on a new Mode of suspending the Mirrors ; 3, its application to the Lundy Lighthouse, so as to save light ; 4, a Report on the Ventilation of the Tynemouth Light ; and he went to see the operation of the grinding-apparatus for lenses at Newcastle.

To Dr. T. M. Browne, who had asserted the isomerism of carbon and silicon, and who asked Faraday to witness his experiments and give him a written testimonial if they were satisfactory, he writes :—" That which made me inaccessible to you makes me so in a very great degree to all my friends—*ill health connected with my head* ; and I have been obliged, and I am still, to lay by nearly all my own pursuits, and to deny myself the pleasure of society, either in seeing myself in my friends' houses or them here. This alone would prevent me from acceding to your request. I should, if I assented, do it against the strict advice of my friends, medical and social.

" The matter of your request makes me add a word or two, which I hope you will excuse. Any one who does what you ask of me, *i. e.* certify if the experiment is successful, is bound, without escape, to certify and publish also *if it fail* ; and I think you may consider that very few persons would be willing to do this. I certainly would not put myself in such a most unpleasant condition."

This year he was made Chevalier of the Prussian Order of Merit (one of thirty), and Foreign Associate of the Royal Academy of Sciences, Berlin.

Æt. 51 (1843).

Early this year he sent the eighteenth series of his ' Researches ' to the Royal Society. It was on the electricity evolved by the friction of water and steam against other bodies. This had been first observed by Sir W. Armstrong, and was attributed to evaporation, and was thought to be related to atmospheric electricity. He concluded, " the cause being, I believe, friction, has no effect in producing, and is not connected with, the general electricity of the atmosphere."

He read a paper at the Institution of Civil Engineers on the ventilation of lighthouse lamps, the points necessary to be observed, and the manner in which these have been, or may be, attained.

He gave three Friday discourses on some Phenomena of Electric Induction ; on the Ventilation of Lamp-burners, and on the Electricity of Steam.

For the Trinity House he went to the South Foreland lighthouses re-

garding their ventilation. He inspected the dioptric light of the first order, which had just been constructed in France and put up by French workmen, and compared its consumption of oil with the 15 Argand burners which were previously in use.

He sent to the Philosophical Magazine a paper on Static Electrical Inductive Action. Among his notes the following occurs:—"Propose to send to the Phil. Mag. for consideration the subject of a bar, or circular, or spherical magnet—first, in the strong magnetic field; then charged by it; and, finally, taken away and placed in space. Inquire the disposition of the dual force, the open or the related powers of the poles externally, and if they can exist unrelated. The difference between the state of the power, when related and when not, consistent with the conservation of force. Avoid any particular language. Should not pledge myself to answer any particular observations, or to any one, against open consideration of the subject. Want to direct the thoughts of all upon the subject, and to tie it there; and especially to gather for myself thought on the point of relation or non-relation of the antithetical force or polarities."

He was made Honorary Member of the Literary and Philosophical Society of Manchester, and Useful Knowledge Society, Aix la Chapelle.

Æt. 52 (1844).

He communicated to the Royal Society a paper on the Liquefaction and Solidification of Bodies generally existing as Gases. His object was to subject the gases to considerable pressure, with considerable depression of temperature. Though he did not condense oxygen, hydrogen, or nitrogen, the original objects of his pursuit, he added six substances, usually gaseous, to the list of those that could previously be shown in the liquid state, and he reduced seven, including ammonia, nitrous oxide, and sulphuretted hydrogen, into the solid form.

He sent to the Philosophical Magazine a speculation touching electric conduction and the nature of matter. Elsewhere he calls this "a speculation respecting that view of the nature of matter which considers its ultimate atoms as centres of force, and not as so many little bodies surrounded by forces, the bodies being considered in the abstract as independent of the forces, and capable of existing without them. In the latter view these little particles have a definite form and a certain limited size. In the former view such is not the case; for that which represents size may be considered as extending to any distance to which the lines of force of the particle extend. The particle, indeed, is supposed to exist only by these forces, and where they are it is."

This was the subject of his first Friday discourse. He also gave the last discourse on recent improvements in the Manufacture and Silvering of Mirrors.

For the Trinity House he only examined different cottons for the lamps.

In October he was sent by Sir James Graham with Mr. Lyell to attend the inquest on those who had died by the explosion in the Haswell colliery.

The following account is by Sir Charles :—

“Faraday undertook the charge with much reluctance, but no sooner had he accepted it than he seemed to be quite at home in his new vocation. He was seated near the coroner, and cross-examined the witnesses with as much talent, skill, and self-possession as if he had been an old practitioner at the bar. We spent eight hours, not without danger, in exploring the galleries where the chief loss of life had been incurred. Among other questions, Faraday asked in what way they measured the rate at which the current of air flowed in the mine. An inspector took a small pinch of gunpowder out of a box, as he might have taken a pinch of snuff, and allowed it to fall gradually through the flame of a candle which he held in the other hand. His companion, with a watch, marked the time the smoke took going a certain distance. Faraday admitted that this plan was sufficiently accurate for their purpose ; but, observing the somewhat careless manner in which they handled their powder, he asked where they kept it. They said they kept it in a bag, the neck of which was tied up tight. But where, said he, do you keep the bag? you are sitting on it was the reply ; for they had given this soft and yielding seat, as the most comfortable one at hand, to the Commissioner. He sprang up on his feet, and, in a most animated and expressive style, expostulated with them for their carelessness, which, as he said, was especially discreditable to those who should be setting an example of vigilance and caution to others who were hourly exposed to the danger of explosions. Hearing that a subscription had been opened for the widows and orphans of the men who had perished by the explosion, I found, on inquiry, that Faraday had already contributed largely. On speaking to him on the subject, he apologized for having done so without mentioning it to me, saying that he did not wish me to feel myself called upon to subscribe because he had done so.”

To a lady of the highest talent, who proposed to become his disciple, to go through with him all his own experiments, he wrote :—“That I should rejoice to aid you in your purpose you cannot doubt, but nature is against you. You have all the confidence of unbalked health and youth, both in body and mind. I am a labourer of many years’ standing, made daily to feel my wearing out. You, with increasing acquisition of knowledge, enlarge your views and intentions. I, though I may gain from day to day some little maturity of thought, feel the decay of powers, and am constrained to a continual process of lessening my intentions and contracting my pursuits. Many a fair discovery stands before me in thought which I once intended, and even now desire, to work out ; but I lose all hope respecting them when I turn my thoughts to that one which is in hand, and see how slowly, for want of time and physical power, it advances, and how likely it is to be not only a barrier between me and the many beyond in intellectual

view, but even the last upon the list of those practically wrought out. Understand me in this ; I am not saying that my mind is wearing out, but those physico-mental faculties by which the mind and body are kept in conjunction and work together, and especially the memory, fail me, and hence a limitation of all I was once able to perform with a much smaller extent than heretofore. It is this which has had a great effect in moulding portions of my later life, has tended to withdraw me from the communion and pursuits of men of science my cotemporaries, has lessened the number of points of investigation (that might at some time have become discoveries) which I now pursue, and which, in conjunction with its effects, makes me say most unwillingly that I dare not undertake what you propose—to go with you through even my own experiments. You do not know, and should not now but that I have no concealment on this point from you, how often I have to go to my medical friend to speak of giddiness and aching of the head, and how often he has to bid me cease from restless thoughts and mental occupation and retire to the seaside to inaction. You speak of religion, and here you will be sadly disappointed in me. You will perhaps remember that I guessed, and not very far aside, your tendency in this respect. Your confidence in me claims in return mine to you, which, indeed, I have no hesitation to give on fitting occasions ; but these I think are very few, for in my mind religious conversation is generally in vain. There is no philosophy in my religion. I am of a very small and despised sect of Christians, known, if known at all, as *Sandemanians*, and our hope is founded on the faith that is in Christ. But though the natural works of God can never by any possibility come in contradiction with the higher things which belong to our future existence, and must with everything concerning him ever glorify him, still I do not think it at all necessary to tie the study of the natural sciences and religion together ; and in my intercourse with my fellow creatures that which is religious and that which is philosophical have ever been two distinct things.”

In answer to Mr. Magrath, who sent him, from the ‘*Journal des Débats*,’ notice of his election as one of the eight foreign associates of the Academy of Sciences, Paris, he said :—“I received by this morning’s post notice of the event in a letter from Dumas, who wrote from the Academy at the moment of the deciding the ballot, and, to make it more pleasant, Arago directed it on the outside.”

He was also made Honorary Member of the Sheffield Scientific Society.

Æt. 53 (1845).

This year produced the nineteenth series of Researches on the Magnetization of Light and the Illumination of Magnetic Lines of Force:—1. Action of Magnets on Light ; 2. Action of Electric Currents on Light ; 3. General considerations. Also the twentieth series, on new Magnetic Actions, and on the Magnetic Conditions of all Matter :—1. Apparatus required ; 2. Action of magnets on heavy glass ; 3. Action of Magnets on other substances act-

ing magnetically on light ; 4. Action of Magnets on the Metals generally. And the twenty-first series, on new Magnetic Actions, and on the Magnetic Condition of all Matter (continued) : 5. Action of Magnets on the Magnetic Metals and their compounds ; 6. Action of Magnets on Air and Gases ; 7. General considerations.

For the Trinity House he made a long and exact comparison of the consumption and light of sperm and rape-oil. He gave a Friday discourse on the Condition and Ventilation of the Coal-mine Goaf, and another on the liquefaction and solidification of bodies usually gaseous ; another on anastatic painting, and on the Artesian well in Trafalgar Square.

Early in the year he thus wrote to Prof. Auguste De la Rive :—" I have waited and waited for a result, intending to write off to you on the instant, and hoping by that to give a little value to my letter, until now, when the time being gone and the result not having arrived, I am in a worse condition than ever ; and the only value my letter can have will be in the kindness with which you will receive it. The result I hoped for was the condensation of oxygen ; but though I have squeezed him with a pressure of 60 atmospheres at the temperature of 140° F. below 0° , he would not settle down into the liquid or solid state ; and now, being tired and ill and obliged to prepare for lectures, I must put the subject aside for a little while.

" Nitrogen is certainly a strange body. It encourages every sort of guess about its nature and will satisfy none. I have been trying to look at it in the condensed state, but as yet it escapes me.

" I thank you most truly, not only for the invitation (to the scientific meeting) you have sent me, but for all the favour you would willingly show me. Do you remember one hot day (I cannot tell how many years ago) when I was hot and thirsty in Geneva, and you took me to your house in the town and gave me a glass of water and raspberry vinegar ? That glass of drink is refreshing to me still."

Late in the year he writes to M. De la Rive :—" I have had your last letter by me for several weeks intending to answer it, but absolutely I have not been able ; for of late I have shut myself up in my laboratory and wrought to the exclusion of everything else. I am still so involved in discovery that I have hardly time for my meals, and am here at Brighton both to refresh and work my head at once ; and I feel that unless I had been here and been careful I could not have continued my labours. The consequence has been that last Monday I announced to our members at the Royal Institution another discovery, of which I will give you the pith.

" Many years ago I worked upon optical glass, and made a vitreous compound of silica, boracic acid, and lead, which I will now call heavy glass. It was this substance that enabled me first to act upon light by magnetic and electric forces. Now, if a square bar of this substance, about half an inch thick and two inches long, be very freely suspended between the poles of a powerful horseshoe electromagnet, immediately that the magnetic force is developed, the bar points, but it does not point from pole to pole, but equa-

torially or across the magnetic lines of force, *i. e.* east and west in respect of the north and south poles. If it be moved from this position it returns to it, and this continues as long as the magnetic force is in action. This effect is the result of a still simpler action of the magnet on the bar than what appears by the experiment, and which may be obtained at a single magnetic pole. For if a cubical or rounded piece of the glass be suspended by a fine thread 6 or 8 feet long, and allowed to hang very near a strong magneto-electric pole (not as yet made active), then, on rendering the pole magnetic, the glass will be repelled until the magnetism ceases. This effect and power I have worked out through a great number of its forms and strange consequences, and they will occupy two series of the 'Experimental Researches.' It belongs *to all matter* (not magnetic as iron) without exception; so that every substance belongs to one or the other class of magnetic or diamagnetic bodies. The law of action in its simplest form is that such matter tends to go from strong to weak points of magnetic force, and in doing this the substance will go in either direction along the magnetic curves, or in either direction across them. It is curious that amongst the metals are found bodies possessing this property in as high a degree as perhaps any other substance; in fact I do not know at present whether heavy glass, or bismuth, or phosphorus is the most striking in this respect."

In July he went with Mrs. Faraday and Mr. G. Barnard to France for three weeks, partly to inspect the lighthouses at Fecamp, Havre, Harfleur, and Cap de la Haye. His chief object was to be received into the Academy. At the same time he gained all the information he could regarding French lighthouses from M. H. Le Ponte and M. Fresnel. M. Dumas was his most constant companion in his visits to Chevreul, Milne-Edwards, Biot, Arago, the Well of Grenelle, and the water-works at Chaillot. On the 30th of July he went to the Institute. "Many of the members were gone out of town, but all that were there received me very kindly. I was glad to see Thenard, Dupuis, Flourens, Biot, Dumas of course, and Arago, Elie de Beaumont, Poinot, Babinet, and a great many others whose names and faces sadly embarrassed my poor head and memory. Chatting together, Arago told me he was my senior, being born in 1786, and consequently 59 years of age."

He finishes his journal thus:—"We left George at the London Bridge Station; thanks be to him for all his kind care and attention on the journey, which is better worth remembering than anything else of all that which occurred in it."

He was made Corresponding Member of the National Institute, Washington, and of the Société d'Encouragement, Paris.

Æt. 54 (1846).

Early in the year he gave a Friday discourse on the relation of Magnetism and Light, and another on the Magnetic Condition of Matter, and, later in

the season, another on Wheatstone's Electro-magnetic Chronoscope, at the end of which he said he was induced to utter a speculation long on his mind, and constantly gaining strength, viz. that perhaps those vibrations by which radiant agencies, such as light, heat, actinic influence, &c., convey this force through space, are not vibrations of an ether, but of the lines of force which, in his view, equally connect the most distant masses together and make the smallest atoms or particles by their properties influential on each other and perceptible to us. A little later he sends these views to the Philosophical Magazine as thoughts on ray vibrations; "but, from first to last, understand that I merely throw out, as matter for speculation, the vague impressions of my mind; for I give nothing as the result of sufficient consideration or as the settled conviction, or even probable conclusion, at which I had arrived." His last Friday discourse was on the Cohesive Force of Water.

He reported to the Trinity House on drinking-water of the Smalls Lighthouse, and on a ventilation apparatus for rape-oil lamps.

To the Secretary of the Institution, who consulted him regarding evening lectures, he said, "I see no objection to evening lectures if you can find a fit man to give them. As to popular lectures (which at the same time are to be *respectable* and *sound*), none are more difficult to find. Lectures which *really teach* will never be popular; lectures which are popular will never *really teach*. They know little of the matter who think science is more easily to be taught or learned than A B C; and yet who ever learned his A B C without pain and trouble? Still lectures can (generally) inform the mind and show forth to the attentive man what he really has to learn, and in their way are very useful, especially to the public. I think they might be useful to us now, even if they only gave an answer to those who, judging by their own earnest desire to learn, think much of them. As to agricultural chemistry, it is no doubt an excellent and a popular subject; but I rather suspect that those who know least of it think that most is known about it."

He received both the Rumford and a Royal Medal, and was made Honorary Member of the Society of Sciences, Vaud.

Æt. 55 (1847).

He gave Friday discourses on the Combustion of Gunpowder; on Mr. Barry's mode of ventilating the New House of Lords; and on the Steam-jet chiefly as a means of procuring ventilation.

He reported to the Trinity House on the ventilation of the South Foreland lights, and on a proposal to light buoys by platinum wire ignited by electricity.

He writes to the First Lord of the Admiralty from Edinburgh:—"For years past my health has been more and more affected; and the place affected is my head. My medical advisers say it is from mental occupation. The result is loss of memory, confusion, and giddiness; the sole

remedy, cessation from such occupation and head rest. I have in consequence given up, for the last ten years or more, all professional occupation, and voluntarily resigned a large income that I might pursue in some degree my own objects of research. But in doing this I have always, as a good subject, held myself ready to assist the Government if still in my power—*not for pay*, for, except in one instance (and then only for the sake of the person joined with me), I refused to take it. I have had the honour and pleasure of applications, and that very recently, from the Admiralty, the Ordnance, the Home Office, the Woods and Forests, and other departments, all of which I have replied to, and will reply to as long as strength is left me; and now it is to the condition under which I am obliged to do this that I am anxious to call your Lordship's attention in the present case. I shall be most happy to give my advice and opinion in any case as may be at the time within my knowledge or power, but I may not undertake to enter into investigations or experiments. If I were in London I would wait upon your Lordship, and say all I could upon the subject of the disinfecting fluids, but I would not undertake the experimental investigation; and in saying this I am sure that I shall have your sympathy and approbation when I state that it is now more than three weeks since I left London to obtain the benefit of change of air, and yet my giddiness is so little alleviated that I don't feel in any degree confident that I shall ever be able to return to my recent occupations and duties."

To Professor Schönbein he writes, three months later:—"I shame to say that I have not yet repeated the experiments (on ozone), but my head has been so giddy that my doctors have absolutely forbidden me the privilege and pleasure of working or thinking for a while; and so I am constrained to go out of town, be a hermit, and take absolute rest. In thinking of my own case it makes me rejoice to know of your health and strength, and look on whilst you labour with a constancy so unremitting and so successful."

He was made Member of the Academy of Sciences, Bologna, Foreign Associate of the Royal Academy of Sciences, Belgium, Fellow of the Royal Bavarian Academy of Sciences, Munich, and Correspondent of the Academy of Natural Sciences, Philadelphia.

Æt. 56 (1848).

He this year communicated his twenty-second series of 'Researches' as the Bakerian lecture. It was on the Crystalline Polarity of Bismuth (and other bodies), and on its relation to the Magnetic form of Force. 1. Crystalline Polarity of Bismuth; 2. Crystalline Polarity of Antimony; 3. Crystalline Polarity of Arsenic. The second part of this series on the same subject was (4) on the Crystalline Condition of various bodies, and (5) Nature of the Magnecrystallic Force, and general observations.

"I cannot conclude this series of Researches," he says, "without remarking how rapidly the knowledge of molecular forces grows upon us, and

how strikingly every investigation tends to develop more and more their importance and their extreme attraction as an object of study. A few years ago magnetism was to us an occult power affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relations with electricity, heat, chemical action, light, crystallization, and, through it, with the forces concerned in cohesion; and we may, in the present state of things, well feel urged to continue in our labours, encouraged by the hope of bringing it into a bond of union with gravity itself."

He gave three Friday discourses on the Diamagnetic Condition of Flame and Gases; on two recent inventions of Artificial Stone; and on the Conversion of Diamond into Coke by the Electric Flame.

He was made Foreign Honorary Member (one of eight) of the Imperial Academy of Sciences, Vienna, and Doctor of Liberal Arts and Philosophy in the University of Prague.

Æt. 57 (1849).

He gave two Friday discourses, one on Plücker's repulsion of the Optic Axes of Crystals by the Magnetic Poles; and the other on De la Rue's Envelope Machinery.

He reported to the Trinity House on the ventilation of Flambro' Head, Dungeness, Needles, and Portland Lighthouses.

He was made Honorary Member, First Class, Institute Royale des Pays-Bas, and Foreign Correspondent of the Institute, Madrid.

Æt. 58 (1850).

The twenty-third series of Researches in Electricity appeared, on the Polar or other Condition of Diamagnetic Bodies. The twenty-fourth series was the Bakerian lecture, on the possible relation of Gravity to Electricity. He finishes this paper, saying, "Here end my trials for the present. The results are negative; they do not shake my strong feeling of the existence of a relation between gravity and electricity, though they give no proof that such a relation exists." The twenty-fifth series was on the Magnetic and Diamagnetic Condition of Bodies:—1. Non-expansion of Gaseous Bodies by Magnetic Force. 2. Differential Magnetic Action. 3. Magnetic characters of Oxygen, Nitrogen, and Space. The twenty-sixth series was on Magnetic Conducting-power:—1. Magnetic Conduction. 2. Conduction Polarity. 3. Magnecrystallic Conduction. Atmospheric Magnetism:—1. General principles. The twenty-seventh series was on Atmospheric Magnetism (continued):—2. Experimental inquiry into the Laws of Atmospheric Magnetic Action, and their application to particular cases.

He gave a Friday discourse on the Electricity of the Air, and another on certain conditions of Freezing Water.

He reported on the adulteration of whitelead for the Trinity House.

To Prof. Schönbein he writes:—"By-the-by, I have been working with the oxygen of the air also. You remember that three years ago I dis-

tinguished it as a magnetic gas in my paper on the diamagnetism of flame and gases, founded on Bancalari's experiment. Now I find in it the cause of all the annual and diurnal and many of the irregular variations of the terrestrial magnetism. The observations made at Hobarton, Toronto, Greenwich, St. Petersburg, Washington, St. Helena, the Cape of Good Hope, and Singapore, all appear to me to accord with and support my hypothesis. I will not pretend to give you an account of it here, for it would require some detail, and I really am weary of the subject." Later he writes:—"I think I told you in my last how that oxygen in the atmosphere, which I pointed out three years ago in my paper on flame and gases as so very magnetic compared with other gases, is now to me the source of all the periodical variations of terrestrial magnetism, and so I rejoice to think and talk at the same time of your results, which deal also with that same atmospheric oxygen. What a wonderful body it is!"

Miss Martineau had said, on the authority of the Annual Register, that he countenanced the *Acarus Crossii*. Faraday corrects her:—"I hope you will forgive me for writing to you about this matter. I feel it a great honour to be borne on your remembrance, but I would not willingly be there in an erroneous point of view."

In the summer he was asked by a friend to stay in the country. He writes, August 24, from Upper Norwood:—"I have kept your picture to look at for a day or two before I acknowledge your kindness in sending it. It gives the idea of a tempting place; but what can you say to such persons as we are who eschew all the ordinary temptations of society? There is one thing, however, society has which we do not eschew; perhaps it is not very ordinary, though I have found a great deal of it, and that is kindness, and we both join most heartily in thanking you for it, even when we do not accept that which it offers. I must tell you how we are situated. We have taken a little house here on the hill-top, where I have a small room to myself, and have, ever since we came here, been deeply immersed in magnetic cogitations. I write and write and write until nearly three papers for the Royal Society are nearly completed, and I hope that two of them will be good if they justify my hopes, for I have to criticize them again and again before I let them loose. You shall hear of them at some of the Friday evenings; at present I must not say more. After writing I walk out in the evening, hand-in-hand, with my dear wife to enjoy the sunset; for to me, who love scenery, of all that I have seen or can see, there is none surpasses that of Heaven: a glorious sunset brings with it a thousand thoughts that delight me."

Earlier the same friend asked him, for the first time, to dinner. He writes from Brighton:—"Your note is a very kind one, and very gratefully received; I wish on some accounts that nature had given me habits more fitted to thank you properly for it by acceptance than those which really belong to me. In the present case, however, you will perceive that our being here supplies an answer (something like a lawyer's objection)

without referring to the greater point of principle. I should have been very sorry in return for your kindness to say *no* to you on the other ground, and yet I fear I should have been constrained to do so."

At the end of the year he had another invitation from the Honourable Col. Grey. "If you could make it convenient to come down to Windsor any afternoon in the course of next week, it would give His Royal Highness great satisfaction to have the opportunity of having some conversation with you on this interesting subject (the magnetic properties of oxygen)."

He was made Corresponding Associate of the *Accademia Pontificia*, Rome, and Foreign Associate of the Academy of Sciences, Haarlem.

Æt. 59 (1851).

The twenty-eighth series of Researches were sent to the Royal Society on Lines of Magnetic Force, their definite character, and their distribution within a Magnet and through Space; also the twenty-ninth series, on the employment of the Induced Magneto-electric Current as a test and measure of Magnetic Forces.

He gave three Friday discourses on the Magnetic Characters and Relations of Oxygen and Nitrogen; on Atmospheric Magnetism; and on Schönbein's Ozone.

No work is recorded for the Trinity House.

He was made Member of the Royal Academy of Sciences at the Hague, Corresponding Member of the Batavian Society of Experimental Philosophy, Rotterdam; Fellow of the Royal Society of Sciences, Upsala; a Juror of the Great Exhibition.

This year closed the series of 'Experimental Researches in Electricity.' It began in 1831 with the induction of electric currents, and his greatest discovery, the evolution of electricity from magnetism; then it continued to terrestrial magneto-electric induction; then to the identities of electricity from different sources; then to conducting-power generally. Then came electro-chemical decomposition; then the electricity of the voltaic pile; then the induction of a current on itself; then static induction. Then the nature of the electric force or forces, and the character of the electric force in the *Gymnotus*. Then the source of power in the voltaic pile; then the electricity evolved by friction of steam; then the magnetization of light and the illumination of magnetic lines of force; then new magnetic actions, and the magnetic condition of all matter; then the crystalline polarity of bismuth, and its relation to the magnetic form of force; then the possible relation of gravity to electricity; then the magnetic and diamagnetic condition of bodies, including oxygen and nitrogen; then atmospheric magnetism; then the lines of magnetic force, and the employment of induced magneto-electric currents as their test and measure.

The record of this work, which he has left in his manuscripts and republished in his three volumes from the papers in the *Philosophical Transactions*, will ever remain Faraday's noblest monument—full of genius in the

conception, full of finished and most accurate work in execution ; n quantity so vast that it seems impossible one man could have done so much ; and this will appear still more when it is remembered that Anderson's help may be summed up in two words, blind obedience.

The use of magneto-electricity in induction machines, in electrotyping, and in lighthouses are the most important practical applications of the 'Experimental Researches in Electricity ;' but who can attempt to measure or imagine the stimulus and the assistance which these researches have given, and will give, to other investigators ?

Lastly, if we look at the circumstances under which this work was done, we shall see that during the greater part of these twenty years the Royal Institution was kept alive by the innumerable Friday lectures which he gave at it. "We were living," as he once said to the managers, "on the parings of our own skin." He had no grant from the Royal Society, and during the whole of this time the fixed income which the Institution could afford to give him was £100 a year, to which the Fullerian professorship added nearly £100 more.

By the 'Experimental Researches in Electricity,' Faraday's scientific life may be divided into three parts. The first lasted to 1830, when he was thirty-eight ; the second, or "research period," lasted to 1851 ; and the third and final period began in 1852, and continued to his last report to the Trinity House (in 1865) on the foci and descent of a beam of light 336 feet at St. Bees Lighthouse.

Æt. 60 (1852).

The first and last Friday discourses of the season were on Lines of Magnetic Force. In the Philosophical Magazine there was a long paper on the Physical Character of the Lines of Magnetic Force. He begins with a note :—"The following paper contains so much of a speculative and hypothetical nature that I have thought it more fitted for the pages of the Philosophical Magazine than for those of the Philosophical Transactions. . . ." "The paper, as is evident, follows series xxviii. and xxix., and depends much for its experimental support on the more strict results and conclusions contained in them."

He made many reports to the Trinity House, among others :—on adulterated white-lead ; on oil in iron tanks ; on impure olive-oils ; on the Caskets lighthouse. And the question of the use of Watson's electric light was first moved by a letter of Dr. Watson to the Trinity House.

In October he wrote a long letter to M. De la Rive. ". . . Do not for a moment suppose I am unhappy. I am occasionally dull in spirits, but not unhappy. There is a hope which is an abundantly sufficient remedy for that ; and as that hope does not depend on ourselves, I am bold enough to rejoice in that I may have it.

"I do not talk to you about philosophy, for I forget it all too fast to make it easy to talk about. When I have a thought worth sending you, it is in

the shape of a paper before it is worth speaking of; and after that it is astonishing how fast I forget it again; so that I have to read up again and again my own recent communications, and may well fear that, as regards others, I do not do them justice. However, I try to avoid such subjects as other philosophers are working at, and for that reason have nothing important in hand just now. I have been working hard, but nothing of value has come of it."

Two months later he writes to Professor Schönbein from Brighton :—

"I am here sleeping, eating, and lying fallow, that I may have sufficient energy to give half a dozen juvenile Christmas lectures. The fact is, I have been working very hard for a long time to no satisfactory end. All the answers I have obtained from nature have been in the negative; and though they show the truth of nature as much as affirmative answers, yet they are not so encouraging; and so for the present I am quite worn out. I wish I possessed some of your points of character; I will not say which, for I do not know where the list might end, and you might think me simply absurd, and, besides that, ungrateful to providence."

Æt. 61 (1853).

Early in the year he gave a Friday discourse on observations on the Magnetic Force, and he gave the last lecture of the season on MM. Boussingault, Fremy, and Becquerel's experiments on oxygen.

He gave five reports to the Trinity House—on a comparison of the French lens and Chance's lens; on the lightning-rods at Eddystone and Bishop's Lighthouses; on the ventilation of St. Catherine and the Needles Lighthouses, and that at Cromer; and on fog-signals. A Company was formed to carry out Watson's electric light, but no trial of it took place.

In June he sent to the Athenæum an experimental investigation of table-moving. At the end he says, "I must bring this long description to a close. I am a little ashamed of it, for I think in the present age and in this part of the world it ought not to have been required. Nevertheless I hope it may be useful. There are many whom I do not expect to convince, but I may be allowed to say that I cannot undertake to answer such objections as may be made. I state my own convictions as an experimental philosopher, and find it no more necessary to enter into controversy on this point than on any other in science (as the nature of matter, or inertia, or the magnetization of light) on which I may differ from others. The world will decide sooner or later in all such cases, and I have no doubt very soon and correctly in the present instance."

A month later he writes to Professor Schönbein :—

"I have not been at work except in turning the tables upon the table-turners. Nor should I have done that, but that so many inquiries poured in upon me that I thought it better to stop the inpouring flood by letting all know at once what my views and thoughts were. What a weak, credulous, incredulous, unbelieving, superstitious, bold, frightened, what a

ridiculous world ours is as far as concerns the mind of man! How full of inconsistencies, contradictions, and absurdities it is! I declare that, taking the average of many minds that have recently come before me (and apart from that spirit which God has placed in each), and accepting for a moment that average as a standard, I should far prefer the obedience, affections, and instinct of a dog before it. Do not whisper this, however, to others. There is One above who worketh in all things, and who governs even in the midst of that misrule to which the tendencies and powers of men are so easily perverted."

After this year, as Director of the Laboratory and Superintendent of the House, he received £300 from the Royal Institution.

He was made Foreign Associate of the Royal Academy of Sciences, Turin, and Honorary Member of the Royal Society of Arts and Sciences, Mauritius.

Æt. 62 (1854).

At the end of this year he sent a long paper to the *Philosophical Magazine* on some points of magnetic philosophy. He begins saying:—"Within the last three years I have been bold enough, though only as an experimentalist, to put forth new views of magnetic action in papers having for titles, 'On Lines of Magnetic Force,' *Phil. Trans.* 1852; and 'On Physical Lines of Magnetic Force,' *Phil. Mag.* 1862. I propose to call the attention of experimenters in a somewhat desultory manner to the subject again, both as respects the deficiency of the present physical views and the possible existence of lines of physical force."

A course of lectures on education was given by different eminent men at the Royal Institution. Prince Albert came to Faraday's "Observations of Mental Education" on the 6th of May. In reprinting them, he said, "They are so immediately connected in their nature and origin with my own experimental life, considered either as cause or consequence, that I have thought the close of this volume (of *Researches on Chemistry and Physics*) not an unfit place for their reproduction." He ends his lecture by saying, "My thoughts would flow back amongst the events and reflections of my past life, until I found nothing present itself but an open declaration—almost a confession—as a means of performing the duty due to the subject and to you."

He gave two Friday discourses on Electric Induction, associated cases of Current and Static Effects; and on Magnetic Hypotheses.

The Parliamentary Committee of the British Association applied to him through Lord Wrottesley for his opinion whether any and what measures could be adopted by the Government or the Legislature to improve the position of science or of the cultivators of science in this country. He answers:—"I feel unfit to give a deliberate opinion. My course of life and the circumstances which make it a happy one for me are not those of persons who conform to the usages and habits of society. Through the kindness of all, from my Sovereign downwards, I have that which supplies all

my need; and in respect of honours, I have as a scientific man received from foreign countries and sovereigns those which, belonging to very limited and select classes, surpass in my opinion anything that it is in the power of my own to bestow.

“I cannot say that I have not valued such distinctions; on the contrary, I esteem them very highly, but I do not think I have ever worked for or sought them. Even were such to be now created here, the time is passed when these would possess any attraction for me, and you will see therefore how unfit I am, upon the strength of any personal motive or feeling, to judge of what might be influential upon the minds of others. Nevertheless I will make one or two remarks which have often occurred to my mind.

. . . . A Government should, *for its own sake*, honour the men who do honour and service to the country. The aristocracy of the class should have distinctions which should be unattainable except to that of science.

. . . . But, besides, the Government should, in the very many cases which come before it having a relation to scientific knowledge, employ men who pursue science, provided they are also men of business. This is perhaps now done to some extent, but to nothing like the degree which is practicable with advantage to all parties. The right means cannot have occurred to a Government which has not yet learned to approach and distinguish the class as a whole.”

He sent five reports to the Trinity House, one of which, in two parts, was on Dr. Watson's electric light (voltaic), and on Prof. Holmes's electric light (magneto-electric). The conclusion was that he could not recommend the electric light, that it had better be tried for other than lighthouse uses first. To Dr. Watson he wrote that he “could not put up in a lighthouse what has not been perfectly established beforehand, and is only experimental.”

He was made Corresponding Associate of the Royal Academy of Sciences, Naples.

Æt. 63 (1855).

His first Friday discourse was on some Points of Magnetic Philosophy and on Gravity. Later he gave a discourse on Electric Conduction; and another on Ruhmkorff's Induction-apparatus.

For the Trinity House he only went to Birmingham to examine some apparatus of Chance's.

This year, on the application of his friend M. Dumas, he was made Commander of the Legion of Honour, and received the Grand Medal of Honour of the French Exhibition for his discoveries.

He was made Honorary Member of the Imperial Society of Naturalists, Moscow, and Corresponding Associate of the Imperial Institute of Sciences of Lombardy.

Æt. 64 (1856).

This year he sent to the Royal Society his last paper, Experimental Relations of Gold (and other metals) to Light. It was read as the Bakerian lecture early the following year.

“At one time I had hoped that I had altered one coloured ray into another by means of gold, which would have been equivalent to a change in the number of undulations ; and though I have not confirmed that result as yet, still those I have obtained seem to me to present a useful experimental entrance into certain physical investigations respecting the nature and action of a ray of light. I do not pretend that they are of great value in their present state, but they are very suggestive, and they may save much trouble to any experimentalist inclined to pursue and extend this line of investigation.”

He gave two Friday discourses, the first on certain magnetic actions and affections ; and the second on M. Petitjean's process for silvering glass, and some observations on divided gold.

He gave five reports to the Trinity House, and he entered into an engagement regarding the Board of Trade Lighthouses, and made four reports, two on Cape Race Lighthouse, and one on Dr. Normandy's distilled water-apparatus.

He was made Corresponding Member of the Netherland Society of Sciences, Batavia, and Member of the Imperial Royal Institute of Padua.

Æt. 65 (1857).

Two Friday discourses were given, the first on the Conservation of Force, and the second on the relations of Gold to Light.

“Various circumstances,” he begins, “induce me at the present moment to put forth a consideration regarding the conservation of force. . . . There is no question which lies closer to the root of all physical knowledge than that which inquires whether force can be destroyed or not. . . . Agreeing with those who admit the conservation of force to be a principle in physics as large and sure as that of the indestructibility of matter, or the invariability of gravity, I think that no particular idea of force has a right to unlimited and unqualified acceptance that does not include assent to it. . . . Supposing the truth of the principle is assented to, I come to its uses. No hypothesis should be admitted nor any assertion of a fact credited that denies the principle. . . . The received idea of gravity appears to me to ignore entirely the principle of the conservation of force, and by the terms of its definition, if taken in an absolute sense, ‘*varying* inversely as the square of the distance,’ to be in direct opposition to it.”

To Mr. Barlow he writes :—

“I am in town, and at work more or less every day. My memory wearies me greatly in working ; for I cannot remember from day to day the conclusions I come to, and all has to be thought out many times over. To write it down gives no assistance, for what is written down, is itself forgotten. It is only by very slow degrees that this state of mental muddiness can be wrought either through or under ; nevertheless I know that to work somewhat, is far better than to stand still, even if nothing comes of it.

It is better for the mind itself—not being quite sure whether I shall ever end the research, and yet being sure that, if in my former state of memory, I could work it out in a week or two to a successful result.”

He gave six reports to the Trinity House. The most important was on Holmes’s magneto-electric light, which was put up at Blackwall, and observed from Woolwich, and compared with a Fresnel lamp in the centre of Bishop’s lens, and also in the focus of a parabolic reflector. He critically examined the cost of the apparatus, the price of the light, the suppositions regarding its intensity and advantages, and the proposition to put one up in a lighthouse. He agreed to its being tried at the South Foreland.

He was made Member of the Institute of Breslau, Corresponding Associate of Institute of Sciences, Venice, and Member of the Imperial Academy, Breslau.

Æt. 66 (1858).

He wrote a short paper on Regelation, which he sent with a letter to Dr. Tyndall on Ice of irregular fusibility. These were printed in Dr. Tyndall’s paper on some Physical Properties of Ice in the Philosophical Transactions for this year.

He gave two Friday discourses. The first was remarks on Static Induction; and the other on Wheatstone’s Electric Telegraph in relation to Science (being an argument in favour of the full recognition of science as a branch of education).

This year Prince Albert offered him a house on Hampton Court Green. It required repair, and he doubted whether he could afford to do it up.

He writes to a niece:—

“The case is settled. The Queen has desired me to dismiss all thoughts of the repairs, as the house is to be put into thorough repair both inside and out. The letter from Sir C. Phipps is most kind.”

To Sir C. Phipps he writes:—

“I find it difficult to write my thanks or express my sense of the gratitude I owe to Her Majesty; first, for the extreme kindness which is offered to me in the use of the house at Hampton Court, but far more for that condescension and consideration which, in respect of personal rest and health, was the moving cause of the offer. I feared that I might not be able properly to accept Her Majesty’s most gracious favour. I would not bring myself to decline so honourable an offer, and yet I was constrained carefully to consider whether its acceptance was consistent with my own particular and peculiar circumstances. The enlargement of Her Majesty’s favour has removed all difficulty. I accept with deep gratitude, and I hope that you will help me to express fitly to Her Majesty my thanks and feelings on this occasion.”

To M. De la Rive he thus writes on the death of Mrs. Marcet:—

“Your subject interested me deeply every way, for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at the age of 13 in the year

1804, remained there eight years, and during the chief part of the time bound books. Now it was in those books in the hours after work that I found the beginning of my philosophy. There were two that especially helped me, the 'Encyclopælia Britannica,' from which I gained my first notions of electricity, and Mrs. Marcet's 'Conversations on Chemistry,' which gave me my foundation in that science.

"Do not suppose that I was a very deep thinker, or was marked as a precocious person. I was a very lively, imaginative person, and could believe in the Arabian Nights as easily as in the Encyclopædia; but facts were important to me and saved me. I could trust a fact, and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge, and clung fast to it. Thence my deep veneration for Mrs. Marcet: first, as one who had conferred great personal good and pleasure on me, and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things to the young, untaught, and inquiring mind.

"You may imagine my delight when I came to know Mrs. Marcet personally; how often I cast my thoughts backwards, delighting to connect the past and the present; how often, when sending a paper to her as a thank-offering, I thought of my first instructress; and such like thoughts will remain with me.

"I have some such thoughts even as regards your own father, who was, I may say, the first who personally, at Geneva, and afterwards by correspondence, encouraged, and by that sustained me."

He made twelve reports to the Trinity House. The most important was on the electric light at the South Foreland. He went there, with a Committee of the Trinity House, to see it from sea and land. The light was in the centre of the Fresnel apparatus, in the upper light, as a fixed light, and so comparable with the lower fixed light, which consisted of oil-lamps in reflectors. They went to the Varne light-ship. The upper was generally inferior to the lower light. Next morning they went to the lighthouse, and examined it by day and also at night.

He was made Corresponding Member of the Hungarian Academy of Sciences, Pesth.

Æt. 67 (1859).

He gave two Friday discourses on Schönbein's Ozone and Antozone; and on Phosphorescence, Fluorescence, &c. He sent eleven reports to the Trinity House, and one to the Board of Trade. On the 28th of March, the magneto-electric light was again exhibited at the South Foreland. On the 20th of April he went to sea to examine it. "The upper light," he says, "is far superior to the lower light; the electric light very fine." He visited the lighthouse; he found new lamps by Duboscq, and silvered reflectors behind. He writes:—"As a light unexceptionable; as electric light won-

derful." He had before drawn up instructions to lighthouse keepers and pilot cutters; and on the 29th of April he reports the sufficiency of the light as established.

He reported this year on Way's mercurial electric light; the one advantage it had was that the place of the light was unchangeable.

He was one of a Commission appointed to consider the subject of lighting public galleries by gas; and he reported favourably on the experimental attempt at the Sheepshanks Gallery.

To Mr. Barlow he writes from Hampton Court:—"As I have been out here with only runs into town, I really know very little of what is going on there, and what I learn I forget. The Senate of the University accepted and approved of the report of the Committee for Scientific Degrees; so that that will go forward (if the Government approve), and will come into work next year. It seems to give much satisfaction to all who have seen it, though the subject is beset with difficulties; for when the depth and breadth of science came to be considered, and an estimate was made of how much a man ought to know to obtain a right to a degree in it, the amount in words seemed to be so enormous as to make me hesitate in demanding it from the student; and though in the D.S. one could divide the matter and claim eminence in one branch of science rather than good general knowledge in all, still in the B.S., which is a progressive degree, a more extended, though a more superficial acquaintance seemed to be required. In fact the matter is so new, and there is so little that can serve as previous experience in the founding and arranging these degrees, that one must leave the whole endeavour to shape itself as the practice and experience accumulates."

Æt. 68 (1860).

He gave two Friday discourses on Lighthouse Illumination by the Electric Light; and on the Electric Silk-loom.

He gave eleven reports to the Trinity House, and he examined three Red-Sea lighthouses for the Board of Trade. On the 13th of February he went to Dover, but was prevented by snow from reaching the lighthouse; on the 17th he tried again, and on the 28th he gave his final report on the practicability and utility of the magneto-electric light. He says, "Hope it will be applied." On the 14th of March the magneto-electric light was proposed for Dungeness. On the 21st he gives his reply, and says there is no difficulty.

He was appointed with Sir Roderick Murchison to report upon the means of preserving the stonework of the new Palace at Westminster.

At Christmas he gave his last course of juvenile lectures on the chemical history of a candle.

He was made Foreign Associate of the Academy of Sciences, Pesth, and Honorary Member of the Philosophical Society of Glasgow.

He resumed the office of Elder in his Church in the autumn, and in little more than three years and a half he finally resigned it.

Æt. 69 (1861).

He gave Friday discourses on Platinum, and on Warren De La Rue's Photographic Eclipse results.

He gave ten reports to the Trinity House. The most important work was a visit on 31st of October to Dungeness, to see the new magneto-electric lamps, the machines, and the steam-engines. He drew up forms of observations to be made at Dungeness, at other lighthouses, and by the pilot cutters.

To Prof. Schönbein he writes :—" You really startle me with your independent antozone. . . . Surely you must hold it in your hand like a little struggler ; for, if I understand you rightly, it must be a far more abundant body than cæsium. For the hold you have already obtained over it I congratulate you, as I would do if you had obtained a crown, and more than for a new metal. But surely these wonderful conditions of existence cannot be confined to oxygen alone. I am waiting to hear that you have discovered like parallel states with iodine, or bromine, or hydrogen, and nitrogen—what of nitrogen ? is not its apparent quiet simplicity of action all a sham ? not a sham, indeed ; but still not the only state in which it can exist. If the compounds which a body can form show something of the state and powers it may have when isolated, then what should nitrogen be in its separate state ? You see I do not work ; I cannot ; but I fancy, and stuff my letters with such fancies (not a fit return) to you."

In another letter he says, " I am still dull, stupefied, and forgetful. I wish a discovery would turn up with me, that I might answer you in a decent, respectable way ; but it will not."

Still later he says :—" I look forward to your new results with great interest ; but I am becoming more and more timid when I strive to collate hypotheses relating to the chemical constitution of matter. I cannot help thinking sometimes whether there is not some state or condition of which our present notions give us very little idea, and which yet would reveal to us a flood, a world of real knowledge,—a world of facts available both by practical application and their illustrations of first principles ; and yet I cannot shape the idea into a definite form, or reach it by any trial facts that I can devise ; and that being the case, I drop the attempt and imagine that all the preceding thought has just been a dreaminess and no more ; and so there is an end of it."

In October he wrote to the Managers of the Institution :—" It is with the deepest feeling that I address you. I entered the Royal Institution in March 1813, nearly forty-nine years ago, and, with exception of a comparatively short period, during which I was abroad on the continent with Sir H. Davy, have been with you ever since. During that time I have been most happy in your kindness, and in the fostering care which

the Royal Institution has bestowed upon me. Thank God, first, for all his gifts. I have next to thank you and your predecessors for the unswerving encouragement and support which you have given me during that period. My life has been a happy one, and all I desired. During its progress I have tried to make a fitting return for it to the Royal Institution, and through it to science. But the progress of years (now amounting in number to threescore and ten) having brought forth first the period of development, and then that of maturity, have ultimately produced for me that of gentle decay. This has taken place in such a manner as to make the evening of life a blessing; for whilst increasing physical weakness occurs, a full share of health free from pain is granted with it, and whilst memory and certain other faculties of the mind diminish, my good spirits and cheerfulness do not diminish with them.

"Still I am not able to do as I have done. I am not competent to perform as I wish the delightful duty of teaching in the Theatre of the Royal Institution, and I now ask you (in consideration for me) to accept my resignation of the juvenile lectures. Being unwilling to give up what has always been so kindly received and so pleasant to myself, I have tried the faculties essential for their delivery, and I know that I ought to retreat; for the attempt to realize (in those trials) the necessary points brings with it weariness, giddiness, fear of failure, and the full conviction that it is time to retire; I desire therefore to lay down this duty. I may truly say that such has been the pleasure of the occupation to me, that my regret must be greater than yours need or can be.

"And this reminds me that I ought to place in your hands the whole of my occupation. It is no doubt true that the juvenile lectures, not being included in my engagement as professor, were when delivered by me undertaken as an extra duty, and remunerated by an extra payment. The duty of research, superintendence of the house, and of other services still remains; but I may well believe that the natural change which incapacitates me from lecturing, may also make me unfit for some of these. In such respects, however, I will leave you to judge, and to say whether it is your wish that I should still remain as part of the Royal Institution. I am, gentlemen, with all my heart, your faithful and devoted servant."

Shortly afterwards he wrote to the Secretary:—"You know my feelings, in regard to the exceedingly kind manner in which the Board of Managers received my letter, and you therefore can best convey to them my deep thanks on this occasion. Please do this for me. Nothing would make me happier in the things of this life than to make some scientific discovery or development, and by that to justify the Board in their desire to retain me in my position here."

Sir Emerson Tennant wished Mr. Faraday to witness the phenomena produced by Mr. Home. Mr. Faraday says, in his reply, "You will see that I consent to all this with much reserve and only for your sake." Three days afterwards Sir E. Tennant says, "As Mr. Home's wife is dying, the

probability is that the meeting, at which I wished you to be present, on the 24th may not take place. From the same cause I am unable to see Mr. Home previously, or to make the inquiries of himself necessary to satisfy the queries in your letter."

He was made Honorary Member of the Medical Society of Edinburgh.

Æt. 70 (1862).

On the 20th of June he gave his last Friday discourse, on Gas furnaces.

He gave seventeen reports to the Trinity House, and two to the Board of Trade. The most important of the Trinity House reports were still on the magneto-electric light. On the 12th of February he went to Dungeness, examined the engine-room, the machines, the lanthorn, the lamps, and the photometric effects. The keepers he examined, and found them not intelligent enough. At night he went to sea, testing at five miles off the effects of oil-lamp reflectors and the electric light, Prof. Holmes himself being in charge of the lamps for the trials. Then he went to the Varne floating-light, and compared Dungeness, Grisnez, and the South Foreland lights. In the morning he went to Dover to examine the upper South Foreland new hydrostatic lamp; and, in the course of the year, the different observations made at South Foreland, Varne, Dungeness, and the pilot-cutters had to be considered and reported on. The House of Commons this year called for copies of his reports on the magneto-electric light to be printed. At the International Exhibition he saw Berlio's magneto-electric machine and light, and he reported on the construction of it.

This year he was examined at great length by the Public School Commissioners. His most important answers were these:—"that the natural knowledge which had been given to the world in such abundance during the last fifty years, I may say, should remain untouched, and that no sufficient attempt should be made to convey it to the young mind, growing up and obtaining its first views of these things, is to me a matter so strange that I find it difficult to understand; though I think I see the opposition breaking away, it is yet a very hard one to be overcome. That it ought to be overcome I have not the least doubt in the world." In answer to the question at what age it might be serviceable to introduce the physical sciences, he says, "I think one can hardly tell that until after experience for some few years. All I can say is this, that at my Juvenile Lectures, at Christmas time, I have never found a child too young to understand intelligently what I told him: they came to me afterwards with questions which proved their capability."

Again he says, "I do think that the study of natural science is so glorious a school for the mind, that with the laws impressed on all created things by the Creator, and the wonderful unity and stability of matter and the forces of matter, there cannot be a better school for the education of the mind."

In September he wrote his last letter to Prof. Schönbein; he says,

“Again and again I tear up my letters, for I write nonsense. I cannot spell or write a line continuously. Whether I shall recover this confusion, do not know. I will not write any more. My love to you.”

The Duke of Devonshire at his installation would have the University of Cambridge confer the degree of LL.D. on Faraday. He was also made Knight Commander of the Order of St. Maurice and Lazarus, Italy.

Æt. 71 (1863).

He made twelve reports to the Trinity House. In February he was again at Dungeness examining a new optic apparatus, and comparing the reflectors with the electric light, and new and old apparatus. He reported on the observations regarding the magneto-electric light, and on a French application to the Board of Trade about the magneto-electric light.

To the Registrar of the London University he wrote :—“Many of your recent summonses have brought so vividly to my mind the progress of time in taking from me the power of obeying their call, that I have at last resolved to ask you to lay before the Senate my desire to relinquish my station and render up that trust of duty which I can no longer perform with satisfaction either to myself or to others.

“The position of a Senator is one that should not be held by an inactive man to the exclusion of an active one. It has rejoiced my heart to see the progress of the University and of education under its influence and power ; and that delight I hope to have so long as life shall be spared to me.”

He was made Foreign Associate of the Imperial Academy of Medicine, Paris.

Æt. 72 (1864).

Twelve reports were made between January and October to the Trinity House. One was on a new magneto-electric machine ; another on drawings, proposals, and estimates for the magneto-electric light at Portland. He made seven examinations of white and red leads, and two examinations of waters from Orfordness and the Fog-gun station, Lundy Island ; and he reported on two 4th-order lights for the River Gambia.

He replied to an invitation of the Messrs. Davenport :—“I am obliged by your courteous invitation ; but really I have been so disappointed by the manifestations to which my notice has at different times been called, that I am not encouraged to give any more attention to them, and therefore I leave these to which you refer in the hands of the Professors of Legerdemain. If spirit communications, not utterly worthless, should happen to start into activity, I will leave the spirits to find out for themselves how they can move my attention. I am tired of them.”

A few weeks later he replied to another different invitation :—

“Whenever the spirits can counteract gravity or originate motion, or supply an action due to natural physical force, counteract any such action,—whenever they can pinch or prick me, or affect my sense of feeling or any other sense, or in any other way act on me without my waiting on them, or,

working in the light, can show me a hand, either writing or not, or in any way make themselves visibly manifest to me—whenever these things are done, or anything which a conjuror cannot do better, or, rising to higher proofs, whenever the spirits describe their own nature, and, like honest spirits, say what they can do, or pretending, as their supporters do, that they can act on ordinary matter whenever they initiate action, and so make *themselves* manifest,—whenever by such-like signs they come to me and ask my attention to them, I will give it. But until some of these things be done, I have no more time to spare for them or their believers, or for correspondence about them.”

At the end of the year he was asked by Mr. Cole to be a Vice-President of the Albert Hall. He replied :—“ I have just returned from Brighton, to which place my doctor had sent me under nursing care. Hence the delay in answering your letter, for I was unaware of it until my return. Now, as to my acceptance of the honour you propose to me. With my rapidly failing faculties, ought I to accept it? You shall decide. Remember that I was obliged to decline lecturing before Her Majesty and the Royal Family at Osborne; that I have declined and am declining the Presidency of the Royal Society, the Royal Institution, and other bodies; declaring myself unfit to undertake any responsibility or duty even in the smallest degree. Would it not therefore be inconsistent to allow my name to appear amongst those of the effectual men who delight, as I should have done under other circumstances, to honour in every way the memory of our most gracious and regretted leader? These are my difficulties. It is only the name and the remembrance of His Royal Highness which would have moved me from a long-taken resolution.”

Mr. Cole decided, “without a moment’s doubt,” that he was to be a Vice-President.

To a friend he writes :—“ I find myself less and less fit for communication with society, even in a meeting of family—brothers and sisters. I cannot keep pace in recollection with the conversation, and so have to sit silent and taciturn. Feeling this condition of things, I keep myself out of the way of making an exposure of myself.”

He was made Foreign Associate of the Royal Academy of Sciences, Naples.

Æt. 73 (1865).

He made his last report for the Trinity House in May this year on St. Bees Light.

He wrote to the Deputy Master :—“ I write to put myself plainly before you in respect of the matter about which I called two days ago. At the request of the then Deputy Master I joined the Trinity House in February 1836, now near upon thirty years since. I find that time has had its usual effect upon me, and that I have lost the power of remembering and also of other sorts, and I desire to relieve my mind. Can this

be done without my retiring altogether, and can you help me in this matter?"

In looking back to his work for the Trinity House, going down to analyses of cottons, oils, paints, and waters, and recalling his words "that £200 a year is quite enough in itself, but not if it is to be the indicator of the character of the appointment," one is rejoiced to find that he received the highest reward which the scientific man can obtain. After himself testing the results by the most complete and searching trials, he was able to recommend that his own grandest discovery should be applied to "the great object of guiding the mariner across the dark and dreary waste of waters."

To the Managers of the Royal Institution he wrote, March 1 :—

"Unless it be that as I get older I become more infirm in mind, and consequently more timid and unsteady, and so less confident in your warm expressions, I might, I think, trust more surely in your resolution of the 2nd of December, 1861, and in the reiterated verbal assurances of your kind Secretary than I do ; but I become from year to year more shaken in mind, and feel less able to take any responsibility on me. I wish, therefore, to retire from the position of Superintendent of the house and laboratories. That which has in times past been my chiefest pleasure has now become a very great anxiety ; and I feel a growing inability to advise on the policy of the Institution, or to be the one referred to on questions both great and small as to the management of the house.

"In a former letter, when laying down the juvenile lectures, I mentioned 'that other duties, such as research, superintendence of the house, and other services still remain ;' but I then feared that I might be found unfit for them ; I am now persuaded that this is the case. If under these circumstances you may think that with the resignation of the positions I have thus far filled the rooms I occupy should be at liberty, I trust that you will feel no difficulty in letting me leave them ; for the good of the Institution is my chief desire in the whole of this action. Permit me to sign myself personally, your dear, indebted, and grateful friend."

"Resolved unanimously—

"That the Managers thank Professor Faraday for the scrupulous anxiety which he has now and ever shown to act in every respect for the good of the Royal Institution. They are most unwilling that he should feel that the cares of the laboratories and the house weigh upon him. They beg that he will undertake only so much of the care of the house as may be agreeable to himself, and that whilst relinquishing the duties of 'Director of the laboratory,' he will retain his home at the Royal Institution."

Sir David Brewster sent him a pamphlet on the Invention and Introduction of the Dioptric Lights, and asked him to give his opinion on the value and importance of these lights. He replied :—" . . . I would rather not enter as an arbitrator or judge into the matter, for I have of late been

resigning all my functions as one incompetent to take up such matters, and the Royal Institution as well as the Trinity House have so far accepted them as to set me free from all anxiety of thought in respect to them. In fact my memory is *gone*, and I am obliged to refrain from reading argumentative matter or from judging of it. I am very thankful for their tenderness in the matter; and if it please Providence to continue me a year or two in this life, I hope to bear the decree patiently. My time for contending for temporal honours is at an end, whether it be for myself or others."

In the fine summer at Hampton Court he sat in his window delighting in the clouds and the holiday-people on the green. A friend from London asked how he was. "Just waiting," he replied. This he more fully said in a note. "I bow before him who is Lord of all, and hope to keep waiting patiently for His time and mode of releasing me according to His divine word, and the great and precious promises whereby His people are made partakers of the divine nature."

To Sir James South, who wished to have some account of Anderson's services, Faraday wrote:—"Whilst endeavouring to fulfil your wishes in relation to my old companion, Mr. Anderson, I think I cannot do better than accompany some notes which he has himself drawn up and had printed, by some remarks of mine, which will show how and how long he has been engaged here.

"He came to assist in the glass house for the service of science in September 1827, where he remained working until about 1830. Then for a while he was retained by myself. In 1832 he was in the service of the Royal Institution, and paid by it. From that time to the present he has remained with that body, and has obtained their constant approbation. In January 1842 they raised his pay to £100 per annum with praise. In 1847 they raised it in like manner to £110. For the same reason in 1853 they raised it to £120; and in 1860, in a minute, of which I think Mr. Anderson has no copy, they say that, in consideration of his now lengthened services and the diligence exhibited by him, they are of opinion that his salary should be raised to £130.

"Mr. Anderson still remains with us, and is in character what he has ever been. He and I are companions in years and in work and in the Royal Institution. Mr. Brande's testimony when he left the Institution is to the same purport as the others. Mr. Anderson was 75 years of age on the 12th of last month (January). He is a widower, but has a daughter keeping his house for him. We wish him not to come to the Royal Institution, save when he is well enough to make it a pleasure; but he seems to be happy being so employed."

Æt. 74 (1866).

Early in January Anderson died. Sir James South wished some monument] to be put up to him, and wrote to Faraday. He replied:—

"My dear old friend, I would fain write to you, but, indeed, write to no one, and have now a burn on the fingers of my right hand which adds to my trouble ; so that I still use my dear J.'s hand as one better than my own, and fear I give her great work by so doing. She has, I understand, written to you this morning, and told you how averse I am to meddling with sepulchral honours in *any* case. I shall mention your good will to Anderson" [here Faraday took the pen, because his niece made some objection to the words "mention the good will to Anderson," who was dead] ; "but I tell them what are my feelings. I have told several what may be my own desire ; to have a plain simple funeral, attended by none but my own relatives, followed by a gravestone of the most ordinary kind, in the simplest earthly place.

"As death draws nigh to old men or people, this world disappears, or should become of little importance. It is so with me ; but I cannot say it simply to others [here he stopped his writing, and his niece finished the note], for I cannot write it as I would. Yours, dear old friend, whilst permitted."

The Society of Arts this summer gave him a medal for his scientific discoveries.

During the winter he became very feeble in all muscular power. Almost the last interest he showed in scientific things was in a Holtz electric machine.

In the spring, for a short time, with decreasing power, there was at times wandering of mind. One day he fancied he had made some discovery somehow related to Pasteur's dextro- and lævo-racemic acid. He desired the traces of it to be carefully preserved, for "it might be a glorious discovery."

His loss of power became more and more plain during the summer and autumn and winter : all the actions of the body were carried on with difficulty ; he was scarcely able to move ; but his mind continually overflowed with the consciousness of the affectionate care of those dearest to him.

Æt. 75 (1867).

At times he could hardly speak a word, and with difficulty swallow a mouthful.

In the spring he went to Hampton Court. Gradually he became more and more torpid, and on the 25th of August he died there.

He said of himself, "In early life I was a very lively imaginative person, who could believe in the Arabian Nights as easily as in the Encyclopædia. But facts were important to me and saved me. I could trust a fact." And so afterwards this blacksmith's son from Jacob's Well Mews, full of inborn religion, and gentleness, genius, and energy, searched for and trusted to facts in his experimental researches, and thus left to science a monument of himself that may be compared even to that of Newton.

H. B. J.

On the 11th of December, 1781, at Jedburgh, was born **DAVID BREWSTER**, who, having made a telescope when only 10 years of age, and having entered on his university course at 12, devoted one of the longest of lives to discoveries in optics, and at last, laden with academic and scientific honours, sank peacefully to rest on the 10th of February, 1868.

He was one of four brothers, all educated for the Church of Scotland, and he advanced to the position of a licentiate; but a certain nervousness in speaking and delicacy of health, combined with an overpowering love for scientific pursuits, led him to decline a good presentation, and to abandon the clerical profession for that of an expounder of natural philosophy. Thus he entered on a career of investigation and literary work which for magnitude, as well as importance, has rarely been rivalled.

As an editor, he commenced in 1808 a work so large that it occupied him for twenty-two years—the *Edinburgh Encyclopædia*; and in the mean time he began with Professor Jameson the *Edinburgh Philosophical Journal*, and subsequently the *Edinburgh Journal of Science*; and from 1832 he was one of the editors of the *Philosophical Magazine*. Throughout his connexion with these periodicals he was a frequent contributor of original articles to their pages, and he continued to the last to write for the *North British* and other *Reviews* in a style so polished and so vigorous, that multitudes learnt from him the actual state of scientific questions who would never have read a merely learned dissertation.

But his fame rests not so much on this literary work as on his original researches, which were so numerous that the ‘*Catalogue of Scientific Papers*’ now being published by the Royal Society contains the titles of 299 papers by him, besides five in which his name is conjoined with those of other investigators. And these researches, though principally connected with the phenomena of light, spread over many other departments of human knowledge.

Nor were Brewster’s labours for the advancement of science confined to the laboratory and the desk. In 1821 he founded the Scottish Society of Arts, and in 1831 he was one of the small party of friends who instituted the British Association, in the meetings of which he usually took a prominent part.

During this time honours steadily flowed in upon him. He was made an honorary M.A. of Edinburgh in 1800, and seven years afterwards an honorary LL.D. of Aberdeen. From 1838 to 1859 he was Principal of the United Colleges of St. Salvador and St. Leonard’s at the University of St. Andrews; and for the last eight years of his life he held the same important office in the leading University of Scotland.

Having been chosen a Fellow of the Royal Society of Edinburgh in 1808, Sir David acted for a long time as its Secretary, and he was President at the time of his death. In 1815 he obtained both the Copley Medal and

the Fellowship of our Society ; and this was followed three years afterwards by the Rumford Medal, and subsequently by one of the Royal Medals ; and, singularly enough, in each case for discoveries concerning the Polarization of Light. In 1816 the French Institute awarded him a pecuniary prize, and nine years afterwards he became a Corresponding Member of that body ; while in 1849 there was conferred upon him the distinguished honour of being chosen one of the eight Foreign Associates of the Academy of Sciences.

It would be tedious to enumerate his other honours from learned bodies at home and abroad ; suffice it to add that he was made a Chevalier of the Prussian Order of Merit, and was knighted by his sovereign in 1832.

Sir David was twice married : first to the daughter of James Macpherson, M.P., of Belleville, the translator of Ossian, and afterwards to Jane Kirk, second daughter of the late Thomas Purnell, Esq., of Scarborough.

To give any adequate idea of the discoveries made known in those scientific papers which Sir David Brewster published every two or three months for sixty years, would be a task of gigantic magnitude. There seem to be thirty papers by him in our Transactions, principally in the earlier part of his career, and, with two exceptions, they are all on optical subjects. In 1813 he commenced with a communication "On some Properties of Light," and in the two succeeding years our Society published for him no less than nine papers—on the polarization of light by oblique transmission, by its passage through unannealed glass, by simple pressure, or by reflection, and on the optical properties of mother-o'-pearl, on calcareous spar. The phenomena of double refraction were indeed treated of in several subsequent papers ; but there is a gap between 1819 and 1829, when he wrote on the periodical colours produced by grooved surfaces, investigated elliptic polarization by metals, and reverted to the optical nature of the crystalline lens. Two papers, one on the Diamond and the other on the Colours of Thin Plates, terminate this series in 1841 ; and the only paper he afterwards sent to our Transactions was one in conjunction with Dr. Gladstone on the Lines of the Solar Spectrum. But there seems never to have been any long intermission in his researches on light ; for he was constantly sending communications on this subject to the Royal Society of Edinburgh or some other learned body, or to the various scientific serials with which he was connected. Thus in the first Number of the Edinburgh Philosophical Journal we find two papers from his pen, the first on new optical and mineralogical structure exhibited in certain specimens of Apophyllite and other minerals, the second on the Phosphorescence of Minerals.

It was as a laborious observer and ingenious experimenter that he excelled ; he cared rather to collect a multitude of facts than to deduce from them general laws. Wonderful proofs of perseverance are his Tables of refractive indices, of dispersive powers, and of the polarizing angles of various reflecting bodies ; and he seems to have submitted to optical exami-

nation every mineral that came in his way. Frequently one of these substances would form the subject of a monograph, as diamond, or amber, the double cyanide of platinum and magnesium, the felspar of Labrador with its changeable tints, or Glauberite with its one axis of double refraction for the violet, and two axes for the red ray. The prismatic spectrum arrested his attention, and in 1834 he announced the absorption of certain rays by the earth's atmosphere, and by nitrous gas; while eight years afterwards he pointed out the existence of luminous lines in certain flames corresponding to those defective in the light of the sun; but he missed the beautiful explanation of Kirchhoff. He also investigated the phenomena of diffraction and dichroism, and of late years exhibited to the British Association the tints of a soap-bubble, or of decomposing glass rendered still more lively by being viewed through a microscope. Indeed his last legacy to science was a paper on Film forms.

The best monument to his fame is perhaps his investigation of polarized light. Malus had first set foot on this domain, but his premature death left it open to the entrance of Brewster, and what wonderful regions did he explore! It not unfrequently happened that some other philosopher, with perhaps a profounder knowledge of mathematics, stepped in and deduced important laws; but sometimes he himself arrived at the higher generalizations; as, for instance, may be cited that of the refractive index of a substance being the tangent of its polarizing angle. But he was not always fortunate in his theories; thus his ingenious view of solar light, as composed of three primary colours (red, yellow, and blue) forming coincident spectra of equal length, has been shown to be completely fallacious. Yet he never abandoned his theory; a fact which we are disposed to attribute, not to a want of conscientious truthfulness, but rather to an inability to appreciate the real bearing of an argument, and to an over confidence in his own memory and the testimony of his senses.

During his optical investigations Sir David often turned from the phenomena seen to the organ of sight, and experimented on that wonderful eye which saw bands in the red rays less refrangible than Fraunhofer's A. Of late years especially he examined the functions of the retina, the *foramen centrale*, and the choroid coat of the eye of animals; he wrote several papers on the *muscæ volitantes*, and explained many peculiarities of single and binocular vision, and not a few optical illusions.

While pursuing these researches on light, he made frequent excursions into other regions of science; he discovered fluids in the cavities of some of the minerals he was examining, and these must be investigated; he wrote much on the mean temperature of the globe; his attention was attracted at one time to fossil bones from Ava, at another to the varnish-trees of India; while systems of double stars, and the pyro-electricity of minerals shared the notice of his comprehensive mind.

As an inventor of new apparatus Brewster also acquired no little renown. His first paper on this subject appears to have been "Some remarks on

Achromatic Eyepieces" in Nicholson's Journal for 1806; and seven years afterwards he published a separate "Treatise on new Philosophical Instruments for various purposes in the Arts and Sciences." In 1816, while repeating some experiments of Biot with a glass trough, he noticed that peculiar method of reflection which is the principle of the Kaleidoscope; and no sooner was this pretty instrument before the public than it became marvellously popular, and that not only as a toy for old and young, but large expectations were raised of its usefulness to the artist and designer of patterns. We are also indebted to him for many other ingenious contrivances for micrometers, burning-glasses, &c., and his writings frequently contained the germs of future inventions. Hence it is not easy to determine his precise share of merit in such appliances as the lenticular stereoscope, or the polyzonal lenses used in lighthouses. In regard to the latter, however, it may be safely maintained that while the chief credit of elaborating the dioptric system of illumination must be given to Fresnel, the persistent advocacy of Brewster materially contributed to its adoption on the shores of our own island.

In addition to the treatises already mentioned he wrote several distinct works of a biographical character:—the *Memoirs of Sir Isaac Newton*, *Euler's Letters and Life*, and the *Martyrs of Science*, viz. Galileo, Tycho Brahe, and Kepler. Nor must he omitted his letters on *Natural Magic*, and his 'More Worlds than One, the Creed of the Philosopher, and the Hope of the Christian.'

Sir David's anonymous writings were nearly as numerous as those to which his name was attached, and they spread over a wider range of subjects. The elaborate treatises on Optics in the *Edinburgh Encyclopædia* and in the recent editions of the *Encyclopædia Britannica* are both from his pen, and to each he contributed the articles on Hydrodynamics and Electricity. In the older work he also wrote on Astronomy, Mechanics, Microscopy, and Burning instruments, while in the later work he turned his attention among other subjects to that of photography.

To the *Edinburgh Review* he contributed twenty-eight articles, which are comprised between the Nos. LVII. and LXXXI. They include biographical notices of such men as Davy and Watt; reviews of such philosophical works as Whewell's 'History and Philosophy of the Inductive Sciences,' Mrs. Somerville's 'Connexion of the Physical Sciences,' Lord Brougham's 'Discourse on the Study of Natural Philosophy,' and even Compté's 'Philosophie Positive:' they pass from Buckland's Geology or Daguerre's photogenic drawings to the lighter subjects of deer-stalking or salmon-fishing; they follow Sir James Ross or Sir George Back in their arctic researches, and describe the British lighthouse system or the phenomena of thunder-storms.

To the *Quarterly Review* he seems to have contributed five articles, and in them he gives his estimate of works by Babbage, Herschel, and Abercrombie; while the subjects he treats are as wide apart as the production

of sound, and the analysis of the intellectual powers, the supposed decline of science in England, and the philosophy of apparitions.

'Meliora' and the Foreign Review each contain two articles from his pen ; one in the latter being a notice of Dutrochet's 'Observations sur Endosmose et Exosmose.'

But it was in the North British Review that the longest series of articles appeared. We have a list before us of seventy-six in the first thirty-nine parts of that quarterly serial, and we doubt whether the enumeration is complete. This shows that, on an average, Sir David wrote two of these literary productions for each part, and suggests the idea that he must have reviewed every book of note that he read. The first Number of the North British commences with an article by him, on Flourens's 'Eloge Historique de Cuvier ;' and further on in the same part he discusses the 'Lettres Provinciales' and other writings of Blaise Pascal. In the second Number he describes the Earl of Rosse's great reflecting telescope ; and shortly we find him engaged with such serious works as Humboldt's 'Cosmos' or Murchison's 'Siluria:' the rival claimants for the honour of having discovered Neptune divide his attention with Macaulay's 'History of England,' or the 'Vestiges of the Natural History of Creation.' With Layard he takes his readers to Nineveh, with Lyell he visits North America, and with Richardson he searches the Polar seas. The Exhibition of 1851, the Peace Congress, and the British Association, come in turn under his descriptive notice ; or turning from large assemblies to individual philosophers, he sketches Arago, Young, or Dalton. In one Number we have "The Weather and its Prognostics," and "The Microscope and its Revelations : " elsewhere he describes the Atlantic telegraph, whilst in a single article he groups together "the life-boat, the lightning-conductor, and the lighthouse." He reviews in turn Mary Somerville's 'Physical Geography,' and Keith Johnston's 'Physical Atlas ;' the History of Photography engages him at one time, and at another Weld's History of our Society. Under the guidance of Sir Henry Holland he investigates the curious mental phenomena of mesmerism and electro-biology, and under that of George Wilson he inquires into colour-blindness. He criticises Goethe's scientific works, expounds De la Rive's 'Treatise on Electricity,' and Arago's on Comets ; or turning from these severer studies, he allows Humboldt to exhibit the 'Aspects of Nature' in different lands to the multifarious readers of the Review.

In addition to all this Sir David issued some pamphlets of a personal nature—controversial writings which some objected to as unnecessarily persistent, though it should be recorded to his honour that he was ready to profit by friendly remonstrance.

Few of his living companions will remember this Nestor in science otherwise than as a venerable form full of vivacity and intelligence, keenly alive not to physical questions alone, but to the various social, political, and ecclesiastical interests of his time, and giving frequent indications of that humble

cal, and ecclesiastical interests of his time, and giving frequent indications of that humble faith in God which was the foundation of his character, and which brightened his declining years and the closing scenes of his earthly life. His many personal friends will retain his memory in their warm affection. Posterity will know him mainly for having opened up new regions in our knowledge of optical phenomena, and for having given a mighty impulse to science during two-thirds of the nineteenth century.—J. H. G.

CHARLES GILES BRIDLE DAUBENY* was born February 11, 1795, at Stratton in Gloucestershire, third son of the Rev. James Daubeny, entered Winchester School in 1808, and was elected to a demyship in Magdalen College, Oxford, in 1810. In 1814, at the age of 19, he took the degree of B.A. in the second class, according to the old style of the Oxford Examinations. In 1815 he won the Chancellor's Prize for the Latin Essay, the prize for the English Essay in the same year being gained by Arnold.

Destined for the profession of medicine, he proceeded to London and Edinburgh as a medical student (1815–18). The lectures of Professor Jameson in Edinburgh on Geology and Mineralogy attracted his earnest attention, and strengthened the desire to cultivate natural science which had been awakened by the teaching of Dr. Kidd at Oxford. In Dr. Kidd's class-room the future historian of volcanoes had frequently met Buckland and the Conybeares, Whateley and the Duncans—men of vigorous minds and various knowledge. The change from thoughtful Oxford to active Edinburgh was the crisis in Daubeny's career. The fight was then raging in the modern Athens between Plutonists and Neptunists, Huttonians and Wernerians, and the possession of Arthur's Seat and Salisbury Craig was sternly debated by the rival worshippers of fire and water. Daubeny entered keenly into this discussion, and, after quitting the University of Edinburgh, proceeded, in 1819, on a leisurely tour through France, everywhere collecting evidence on the geological and chemical history of the globe, and sent to Professor Jameson from Auvergne the earliest notices which had appeared in England of that remarkable volcanic region†. Some of the views afterwards advanced by the young physicist touching the elevation of the hills and the geological age of the valleys of Auvergne‡ have been reexamined and discussed by later eminent writers, such as Scrope, Murchison, Lyell—not always in agreement with him, or, indeed, with one another; while the prehistoric antiquity of the volcanoes them-

* Extracted from a more extended Obituary Notice of Dr. Daubeny, read to the Ashmolean Society of Oxford, by Professor John Phillips, F.R.S., February 17, 1868.

† Letters on the Volcanoes of Auvergne, in Jameson's Edinburgh Journal, 1820–21.

‡ Transactions of the Royal Society of Edinburgh, 1831.

selves has been questioned even within a few years, and defended by none more effectually than by Dr. Daubeny*.

From the beginning to the end of his scientific career, volcanic phenomena occupied the attention of Dr. Daubeny; and he strove by frequent journeys through Italy, Sicily, France and Germany, Hungary and Transylvania, to extend his knowledge of that interesting subject. In 1823–25, he had by this means prepared the basis of his great work on volcanoes, which appeared in 1826, and contained careful descriptions of all the regions known to be visited by igneous eruptions, and a consistent hypothesis of the cause of the thermic disturbance, in accordance with the view first proposed by Gay-Lussac and Davy. Water admitted to the uncombined bases of the earths and alkalies existing below the oxidized crust of the globe, was shown to be an efficient cause of local high temperature, and a real antecedent to the earthquake movements, the flowing lava, and the expelled gas and steam. In later years† Dr. Daubeny freely accepted, as at least very probable, a high interior temperature of the earth; but he did not allow that the admission of water to a heated interior oxidized mass would account for the chemical effects which accompany and follow an eruption. On this point there are still data to be gathered and inferences to be examined.

Four years previously to the publication of the ‘Description of Volcanoes,’ Dr. Daubeny was appointed to succeed Dr. Kidd as Aldrichian Professor of Chemistry, and took up his abode in, or rather below, the time-honoured Museum founded by Ashmole. In these rather gloomy apartments nearly all the scientific teaching of Oxford had been accomplished since the days of Robert Plot; in them were still collected, as late as 1855, by gas-light and furnace-fires, the most zealous students of Practical Chemistry; but now they are filled with Greek sculpture, and Chemistry has flitted to the magnificent laboratories of the University Museum, directed by Sir Benjamin Brodie.

In 1834 he was appointed Professor of Botany, and migrated to the “Physic Garden,” as it was called, which had been founded in the early part of the reign of Charles I.

Under his diligent and generous management, with liberal aid from the University, Dr. Daubeny lived to see the old Garden entirely arranged, enriched with extensive houses, extended in area, and made both attractive and beautiful.

In the pleasant residence at the Botanic Garden, Dr. Daubeny passed the remainder of his life—the third of a century. Here, incessantly active, he instituted many experiments on vegetation under different conditions of soil, on the effects of light on plants, and of plants on light, on the distribution of potash and phosphates in leaves and fruits, on

* Quarterly Journal of Science, 1866.

† “Memoir on the Thermal Waters of Bath,” British Association Reports for 1864.

the conservability of seeds, on the ozonic element of the atmosphere, and on the effect of varied proportions of carbonic acid on plants analogous to those of the coal-measures*. These last-mentioned experiments are among the very few which can be referred to as throwing light on the curious question whether the amazing abundance of vegetable life in the carboniferous ages of the world may not have been specially favoured by the presence, in the palæozoic atmosphere, of a larger proportion of carbonic acid gas than is found at present.

A favourite subject of research with Dr. Daubeney, naturally springing from his volcanic explorations, was the chemical history of mineral waters. The presence of iodine and bromine in some of these formed the subject of a paper in the Philosophical Transactions for 1830; and a Report to the British Association in 1836 included a general survey of mineral and thermal waters. This subject was not neglected in his 'North-American Tour' (1837-38), which contains a great number of interesting observations on the character of the country which he traversed, as well as the educational institutions, where he was heartily welcomed.

Dr. Daubeney was a great traveller, almost an annual visitor to the continent, usually, at least in his later years, accompanied by some scientific or literary friend, some member of his family, or some young Oxonian of cultivated taste, to whom the sight of Auvergne and the Tyrol in the company of such a guide was a gift of priceless value.

In one of his journeys to Spain in 1843, for the purpose of studying the geological relations and agricultural value of the great phosphatic deposit of Estremadura, he was accompanied by Captain Widdringtoe, R.N. It was a journey prompted by benevolence and attended by hardship. No doubt, in some future day, railways will carry heavy loads of this valuable substance to enrich the agriculture of Spain†. In another year he might be found in Norway, or musing in the Garden at Geneva, where he was always welcomed by the great botanist whose friendship he gained in early life, and to whose memory he has devoted a careful critical essay, which was read to the Ashmolean Society in 1842‡.

It was at Geneva that he "began to estimate at their true weight the pretensions of Botany to be regarded as a science, and to comprehend the principle on which it might be inculcated as constituting an essential part of a liberal education." Here he first pursued his botanical studies under the guidance of Decandolle in 1830, and thus qualified himself for the Professorship to which, as already observed, he was appointed in 1834.

Chemistry, however, was the thread which bound together all the researches of Dr. Daubeney; not that he was personally a dexterous

* Miscellaneous Memoirs and Essays, 1867. British Association Reports, 1837-57.

† Memoir read to the Geological Society in 1844.

‡ Daubeney's Miscellanies, vol. ii. "On the Life and Writings of A. P. Decandolle."

manipulator of chemical instruments, though a diligent practical analyst. He was rich in chemical knowledge, profound and varied in his acquired views of chemical relations, always prompt and sagacious in fixing upon the main argument and the right plan for following up successful experiment or retrieving occasional failure. In 1831 appeared his 'Sketch of the Atomic Theory,' a work which well sustained the reputation of the author as a master of language and a conscientious teacher of science.

So soon as the arrangements were made for the location of Chemistry in its new abode Dr. Daubeny took the occasion of resigning the Chair of Chemistry, and used all his influence to increase the efficiency of the office and secure the services of the present eminent Professor.

In his position as a teacher of Botany, he took pleasure in drawing attention to the historical aspects of his subject, and specially, as a part of his duty, treated of Rural Economy both in its literary and its practical bearings. Hence arose the "Lectures on Roman Husbandry" (1857), written in a style very creditable to the classical training of his early years, and containing a full account of the most important passages of Latin authors bearing on crops and culture, the treatment of domestic animals, and horticulture. To this is added an interesting Catalogue of the Plants noticed by Dioscorides, arranged in the modern natural orders. This was followed, after a few years, by a valuable Essay on the Trees and Shrubs of the Ancients, and a Catalogue of Trees and Shrubs indigenous in Greece and Italy (1865).

To facilitate his researches in Experimental Botany, Dr. Daubeny had obtained possession of a piece of land lying some half a mile or so from Oxford; but of late years symptoms of ill-health interfered both with his enjoyment of the recreation of his little farm, and the experiments for which it was destined.

During a few late winters Dr. Daubeny found it desirable to exchange his residence in Oxford for the milder climate of Torquay. Here his activity of mind was equally manifested by public lectures on the temperature and other atmospheric conditions of that salubrious resort, and by experiments on ozone and the usual meteorological elements, in comparison with another series in Oxford. By this connexion with Devonshire he was induced to join the Association in that county for the Advancement of Science, Literature, and Art; and one of his latest public addresses was delivered to that body, as President, in 1865.

In his whole career Dr. Daubeny was full of that practical public spirit which delights in cooperation, and feeds upon the hope of benefiting humanity by associations of men. When the British Association came into being at York, in 1831, Daubeny alone stood for the Universities of England.

In 1856 he was its President, at Cheltenham, in his native county,

amidst numerous friends, who caused a medal to be struck in his honour—the only occurrence of this kind in the annals of the Association.

The same earnest spirit was manifested in all his academic life. No project of change, no scheme of improvement in University Examinations, no modification in the system of his own college, ever found him indifferent, prejudiced, or unprepared. On almost every such question his opinion was formed with rare impartiality, and expressed with as rare intrepidity. Firm and gentle, prudent and generous, cheerful and sympathetic, pursuing no private ends, calm amid jarring creeds and contending parties—the personal influence of such a man on his contemporaries for half a century of active and thoughtful life fully matched the effect of his published works. His latest labour was to gather his ‘Miscellaneous Essays’ into two very interesting volumes, and then, after patiently enduring severe illness for a few weeks, he sank to that rest which, often in his thoughts, had ever been expected, with the calmness of the philosopher and the hopefulness of the Christian. He died at five minutes past twelve A.M., December 13, 1867, in his 73rd year.

His remains were laid in a vault adjoining the walls of Magdalen College Chapel, in accordance with his own expressed wish “that he might not be separated in death from a society with which he had been connected for the greater part of his life, and to which he was so deeply indebted, not only for the kind countenance and support ever afforded him, but also for supplying him with the means of indulging in a career of life at once so congenial to his taste and the best calculated to render him a useful member of the community.”

In the preceding brief notices no mention has been made of Dr. Daubeny’s short career as a medical man, for which he had prepared himself by professional study in Edinburgh and London. In Oxford he justified his title of M.D. and his Fellowship with the College of Physicians by attaching himself to the Radcliffe Infirmary. In this capacity, however, he did not long remain; nor did he continue his medical practice, though during all his life the progress of medical science was much at his heart, as may be seen in the Harveian Oration which he delivered before the College of Physicians in 1845. In that elegant address he speaks of himself as “... quem, a medicinæ castris tanquam profugum, Physicarum Scientiarum amor, aut Otii Literati dulcedo, ad aliam vitæ normam jam tot per annos transtulit, ut ne inter commilitones vestros recenseri merear.”

In these words we have the key to the valuable life which was passed so busily and so gracefully among his academic brethren, and to the works of scientific and literary interest which are all that now remain to us of Charles Daubeny. What he has said of these works is perhaps the truest and most modest comment that will ever be made on them and on the circumstances under which they were produced. For they are “some of the fruits of a

life chiefly spent in tranquil intellectual occupation, under the fostering wing of one of those great semimonastic establishments which are peculiar to this country; and however slight their intrinsic value, considered as contributions to the stock of human knowledge, may be, they will serve at least to show, by their number and variety, what might be accomplished by persons gifted with greater energy and more profound attainments, through the aid of foundations in which an exemption from domestic cares, and a liberal provision for all the reasonable wants of a celibate life, afford such facilities for the indulgence of either literary or scientific tastes."

Under the influence of the traditions of former scientific culture in Oxford, and

"Not mindless of those mighty times"

when the leading spirits of remote antiquity committed to posterity the priceless records of early philosophy, was Charles Daubeny conducted to the School of Chemistry, and the School of Geology. In them, but especially in the former, he imbibed sound and various knowledge. From them he passed at once to researches and publications which have contributed as much as those of any physicist of this century to sustain the credit of the University and guide the progress of useful knowledge. And the influence of these publications was in no slight measure due to the pure classical taste, and the sure employment of appropriate language, which were the gift of the foundations of William of Wykeham and William of Waynflete.

The same accuracy appeared in the frequent addresses which he was called on to make on social or public occasions. He affected no grace or oratory;

"His words succinct, yet full, without a fault,
He said no more than just the thing he ought;"

but the calm and reasonable views which he might be trusted to present on all subjects of scientific interest or administrative reform, never failed to have their due influence even over the agitations of controversy—from which he never shrank if his sense of justice and love of truth called for vindication. Any one accustomed to a considerable degree of intimacy with Dr. Daubeny would be able to declare that he never met with any man more entirely truthful and just-minded. You might absolutely rely upon him, in regard of deeds, thoughts, and motives. To convince his judgment was to enlist his sympathy and secure his active help; to be censured with overmuch strictness was a passport to such protection as he could honestly give. In defence of a friend whose Essay was unpopular, in opposition to a course of University mutation which he did not think was reform, in advocating what he believed to be desirable changes, his arms were ever ready; nor did he throw a pointless dart.

With reference to the influence of Dr. Daubeny in scientific discussions,

one may venture to say that it would have been greater had his early studies been more turned in the direction of mathematics, especially as applied to physical research. In the beginning of his career, indeed, Chemistry was only acquiring numerical exactness, and Geology was quite unprovided with mechanical laws of earth-movement. But no one knew better than Dr. Daubeny that right geometrical conceptions are always necessary to a student of science, and laws of proportion indispensable elements of sound philosophy.

The published writings of Dr. Daubeny are very numerous. Besides what have appeared as independent works, the list of his Memoirs in Transactions and Journals up to 1863, as given in the Royal Society's "Catalogue of Scientific Papers," amounts to seventy-too. Many of these, scattered through various periodicals and not conveniently accessible, were collected and arranged by their author in two volumes of Miscellanies. In this collection appeared twelve Experimental Essays, ten Geological Memoirs, eight Essays on Scientific Subjects, and twelve on Literary Subjects. Besides these were several papers of interest, some published separately, which, having been composed after the first edition of the 'Description of Volcanoes,' were employed in the preparation of the second edition, or noticed in supplements to that work.

By these arrangements Dr. Daubeny has rendered it unnecessary, for those who desire to know his views on the various subjects which occupied his mind, to refer to such publications as the Edinburgh Philosophical Journal, Edinburgh New Philosophical Journal, or Journal of the Geological Society, or even to the Linnean Transactions, Royal Society's Transactions, or Reports of the British Association, except from a desire to learn his first thoughts from his first words. The following is a list of the works which contain the principal results of Dr. Daubeny's scientific and literary labours:—

1. Description of Active and Extinct Volcanoes. 8vo, London, 1826. Second Edition, 1848. Several Supplements.
2. Tabular View of Volcanic Phenomena. Folio, thick, 1828.
3. Notes of a Tour in North America (privately printed). 8vo, 1838.
4. Introduction to the Atomic Theory. 8vo, 1852.
5. Lectures on Roman Husbandry. 8vo, 1857.
6. Lectures on Climate. 8vo, 1863.
7. Trees and Shrubs of the Ancients. 8vo, 1865.
8. Miscellanies on Scientific and Literary Subjects. 2 vols. 8vo, 1867.

JULIUS PLÜCKER, Foreign Member of the Royal Society, was born on the 16th of July 1801, at Elberfeld. After studying in the Gymnasium of Düsseldorf, and in the Universities of Bonn, Berlin, and Heidelberg, he passed some years in Paris. In 1825 he became a Privatdocent of Mathematics in Bonn, and in October 1828 was appointed Professor extraordinarius in that University. In 1833 he went to Berlin in the same capacity, and lectured also in the Friedrich-Wilhelm's Gymnasium. In 1834 he obtained the Professorship of Mathematics in the University of Halle, and in 1836 he was appointed Professor of Mathematics in the University of Bonn. The treatises and memoirs on Analytical Geometry written by him during the twenty years that followed his return from Paris secured for him a place among the first mathematicians of his time. He now entered upon a new career; for the superintendence of the Physical Museum having been entrusted to his care, he turned his attention to experimental research, and was appointed to the Professorship of Physics in 1847. A series of brilliant discoveries soon placed him among the foremost labourers in this department of science. These researches occupied him till 1856.

In repeating some of Faraday's experiments, he was led to the discovery of magnecrystallic action,—that is, that a crystallized body behaves differently in the magnetic field according to the orientation of certain directions in the crystal. These researches occupied him till 1856, when he turned his attention to the action of powerful magnets on the luminous electric discharge in glass tubes containing highly rarefied gas. In a wide tube the light of such a gas is too faint to permit a satisfactory observation of its spectrum; he found, however, that by employing tubes which were capillary in one part, brilliant light and definite spectra were obtained in the narrow part. These spectra were found to be characteristic of the several gases and to indicate their chemical nature, though the gases might be present in such minute quantity as utterly to elude chemical research.

In continuing these researches he next made the remarkable discovery of the two totally different spectra of each of the elementary substances, nitrogen, sulphur, selenium, hydrogen, iodine, lead, manganese, and copper, according as it is submitted to the instantaneous discharge of a Leyden jar charged by an induction coil, or rendered incandescent by the simple discharge of the coil, or else, in some cases, by ordinary flames. The two spectra were found to exhibit a difference in character, and are not merely different in the number and position of the lines which they show. This difference he attributed, with the greatest probability, to a difference in the temperature of the gas when the two are respectively produced. These results were made known in a memoir by himself conjointly with Dr. S. W. Hittorf, printed in the Philosophical Transactions for 1865. About this

time he resumed his geometrical investigations, but only lived to see the publication of the first part of the treatise upon which he was engaged.

He took an active part in the management of the University, having been twice Rector, frequently Dean of the Faculty of Philosophy, for many years Member of the Academic Senate and the Examination Commission. He was a Member of the Academies of Munich, Haarlem, Rotterdam, Lund, and Upsala, of the Société royale de Liège, of the Société des Sciences Naturelles de Cherbourg, of the Société Philomathique of Paris, Honorary Member of the Cambridge Philosophical Society, Corresponding Member of the Institute, of the Academies of Vienna, Göttingen, and the Physikalische Verein of Frankfort; his election as Foreign Member of the Royal Society was in 1855. The Copley Medal for the year 1866 was awarded to him for his researches in Analytical Geometry, Magnetism, and Spectral Analysis.

His separate works are :—

Analyseos applicatio ad geometriam altiore et mechanicam (Bonnæ, 1824).

Analytisch-geometrische Entwicklungen (Essen, 1831).

System der analytischen Geometrie (Berlin, 1835).

Theorie der algebraischen Curven (Bonn, 1839).

System der Geometrie des Raumes in neuer analytischer Behandlungsweise (Düsseldorf, 1846, second edition, 1852).

Enumeratio novorum phenomenorum in doctrina de magnetismo inventorum (Bonnæ, 1849).

De crystallorum et gazorum conditione magnetica (Bonnæ, 1850).

Neue Geometrie des Raumes, gegründet auf die Betrachtung der geraden Linie als Raumelement (Leipzig, 1868, Erste Abtheilung).

He also edited a work by his former pupil, Professor August Beer, entitled “*Einleitung in die Electrostatic, die Lehre vom Magnetismus und die Electrodynamik*,” left in manuscript by the latter at his death.

His papers in the ‘*Transactions*’ of the Royal Society are :—

On the Magnetic Induction of Crystals, March 26, 1857.

On the Spectra of Ignited Gases and Vapours, with especial regard to the different Spectra of the same elementary gaseous substance, conjointly with Dr. S. W. Hittorf, February 23, 1864.

On a New Geometry of Space, December 22, 1864.

Fundamental Views regarding Mechanics, May 29, 1866.

He is also the author of numerous papers on analysis, geometry, electricity, magnetism, physical optics, and spectral analysis, in Crelle’s ‘*Journal*,’ Gergonne’s ‘*Annalen*,’ Liouville’s ‘*Journal*,’ Poggendorff’s ‘*Annalen*,’ the Abbé Moigno’s ‘*Les Mondes*,’ the ‘*Philosophical Magazine*,’ the ‘*Annali di Matematica*.’

He died at Bonn on the 22nd of May, 1868.

JEAN BERNARD LÉON FOUCAULT, Foreign Member of the Royal Society, was born in Paris on the 18th of September 1819. He began the study of medicine, but soon gave the preference to physics and the sciences of observation. At the age of twenty he employed himself in improving the processes of photography. For three years he assisted M. Donné in preparing the illustrations of his lectures on microscopic anatomy, and was associated with M. Fizeau in conducting a variety of original researches. They investigated the comparative intensities of the light of the sun, of the voltaic arc between carbon poles, and of lime heated before the oxyhydrogen blowpipe. They read memoirs on the interference of calorific rays, on the interference of two rays of light in the case of a large difference in the lengths of their routes, and on the chromatic polarization of light. In December of 1849 Foucault described an electromagnetic regulator of the electric light. Conjointly with Regnault he was the author of a paper on binocular vision. He contributed besides several memoirs on colour, on voltaic and frictional electricity, and on the employment of the conical pendulum as a time-keeper.

M. Arago had suggested the employment of Wheatstone's revolving mirror, in a manner resembling its use in measuring the propagation of the electric current in a wire, to decide whether the velocity of light within a refractive medium is greater or less than its velocity in air. The former result implies the truth of the emission theory, the latter that of the undulatory theory. The experiment, as devised by M. Arago, was nearly (perhaps quite) impracticable, inasmuch as it depended upon the observation of an image of momentary duration formed in an unknown part of the field of view. By the happy introduction of a concave mirror having its centre in the axis of the revolving mirror, a fixed image was obtained; and the experiment thus rendered possible proved that the velocity of light is greater in air than in water. This experiment was made in 1850, not long after M. Fizeau had approximately determined the velocity of light in air by measuring the time it occupied in travelling from the place of the observer to a station 8633 metres distant, and back again. Foucault also suggested the means of measuring the velocity of propagation of radiant heat.

In February 1851 he communicated to the Academy the results of his observations on the rotation of the plane of oscillation of a freely suspended pendulum in the direction east-south-west, and thus supplied an ocular demonstration of the diurnal motion of the earth. By the construction of the gyroscope, in September 1852, he gave a second demonstration of the same phenomenon. For these discoveries the Copley Medal for the year 1855 was awarded to him. About this time he was appointed Physical Assistant to the Imperial Observatory. In September of the same year he exhibited a new instance of the conversion of work into heat. A copper

disk being made to revolve rapidly in its own plane, on bringing a horse-shoe magnet into such a position that the disk revolved with its rim between the poles of the magnet, the moving force required to maintain the velocity of rotation increased, and the temperature of the disk was raised.

On the 16th of February 1857 he described a reflecting telescope, having a speculum formed of glass coated with chemically reduced silver and afterwards polished, of 10 centims. aperture and 50 centims. focal length, without being aware that a telescope on the same principle and nearly of the same dimensions had been described by Steinheil in the *Allgemeine Zeitung* of the 24th of March 1856. In the following year Foucault succeeded in giving the speculum the form of a spheroid or of a paraboloid of revolution, and described a new process for finding out the configuration of optical surfaces. A reflector of this description, having an aperture of 40 centims. and 2·5 metres focal length, was mounted in the Imperial Observatory of Paris in June 1859. Another of these reflectors, having an aperture of 78 centims. and a focal length of 4·5 metres, was constructed for the Observatory in 1862. The polarizer known as his was invented in 1857.

The project of determining the absolute velocity of light in air with the aid of Wheatstone's revolving mirror, conceived in 1850, was carried out in 1862. The value Foucault obtained for it was 298,000 kilometres in a second of time, instead of 308,000 kilometres, the previously received value. Combining the newly found velocity with the constant of aberration, 20·445, the sun's equatorial parallax is found to be 8''·86, the value deduced by Mr. Stone in his recent discussion of the transit of Venus in 1769 being 8''·91, and the value adopted in the 'Nautical Almanac' for 1870 being 8''·95. In this year Foucault was elected a Member of the Bureau des Longitudes.

In the years 1863, 1864, 1865 he appears to have been occupied with the task of investigating the conditions of isochronism of Watt's governor, and modifying its construction so as to render the time of revolution invariable. These improved governors are applied to the transit-recorders constructed for the use of the Indian Survey. In January 1865 he was elected a Member of the Mechanical Section of the Institute. In 1866 he invented a new and improved regulator for the electric light, and a telescope for viewing the sun, in which the light is rendered endurable to the eye by coating the outer surface of the object-glass with a film of chemically reduced silver so thin as to be transparent. This process was applied with complete success to a refractor having an aperture of 25 centims.

In July 1867 he was attacked by paralysis, and died on the 11th of February, 1868. The date of his election as Foreign Member of the Royal Society is June 9, 1864.

ANTOINE FRANÇOIS JEAN CLAUDET was born at Lyons in 1797. He received a good commercial and classical education in his own country, and at the age of 21 he entered the office of his uncle, M. Vital Roux, an eminent banker, who a few years after placed him at the glass-works of Choisy-le-Roi, as director, in conjunction with M. G. Bontemps, the well-known glass-manufacturer. Eventually M. Claudet came to London to introduce the productions of Choisy. In 1833 he invented the machine now generally used for cutting cylindrical glass. For this invention he received the medal of the Society of Arts in 1853. But all this while he was a student of science training and waiting for the object to which his true life was to be devoted. The path was opened to him by the discovery of M. Daguerre.

In January 1839 that discovery was first *announced* to the world, and specimens of the results were exhibited, the *modus operandi* being still preserved secret. The French Government at once entertained the project of rewarding the discoverer, and in the following June assigned to M. Daguerre a pension of 6000 francs annually, and to M. Niépce, jun., a pension of 4000 francs annually, that the new art might be presented *a gift to the world*.

In the month of August 1839 the new discovery was *published* to the world. It was received with enthusiasm, and rapidly adopted as a means of delineation, portraiture being its most early and extensive application. England alone failed to partake freely of this "gift to the world," M. Daguerre having entered into negotiations which secured a patent in this country whilst the question of his claims was under the attention of the French Government. M. Claudet became the possessor of a part of this patent, and commenced in 1840 the practice of portraiture in the Adelaide Gallery, where his studio remained for many years. There, as a zealous worker, he devoted himself to the improvement and development of photography, perfecting known processes and inventing new ones. His earliest contribution to the art was a mode of obtaining vastly increased sensitiveness by using chloride of iodine instead of iodine alone. His paper on this subject was read before the Royal Society in June 1841; and, by a curious coincidence, it followed Mr. Fox Talbot's description of his own photographic process, the calotype. From this period till his death his contributions to photographic literature were copious and interesting, the idiomatic excellence and elegance of his English being remarkable.

In 1847, discussing the properties of solar radiation modified by coloured glass media, he made a bold attempt to lay the foundation of a more complete theory of the photographic phenomena, and he was rewarded by the publication of his paper in the Philosophical Transactions, and by his subsequent election (in 1853) as a Fellow of the Royal Society. At this time the collodion process had supplanted the method of Daguerre; and Claudet was one of the first to appreciate and adopt it.

The marvellous phenomenon of objects in relief was now brought before him in the stereoscope, and seemed to him a greater charm than the ex-

quisite detail of the Daguerreotype. He assisted Sir Charles Wheatstone in the early application of the stereoscope to photography; and in his admirable treatise on the stereoscope he gives the history of the art and the theory of the principles of binocular vision. His great aim was the elevation of photography by rendering her work scientifically true; and the Reports of the British Association during a period of twenty years bear ample testimony to the ingenuity and originality of his inventions. His dynactinometer, his photographometer, his focimeter, his stereomonscope, his system of unity of measure for focusing enlargements, his system of photosculpture, and other results of his experimental researches are familiar to most photographers.

In the later years of his life he became convinced that one of the greatest deficiencies of photography, in the representation of solid figures, is the incapability of obtaining an equally well-defined image of all the various parts situated on different planes. Hence it became his object to remove from photographic portraiture the mechanical harshness which marked and marred the plane situated in the exact focus of the lens, and so to produce, as in the best works of art, a uniformly soft and harmonious treatment. His success in the first instance was partial, inasmuch as the adopted motion of the posterior lens only of the optical combination slightly altered the size of the superimposed images, and thus introduced a theoretical, though hardly visible, amount of blurring. Dr. Sommer, M. Voigtlander's stepson, supplied a series of formulæ showing that, although for all practical purposes in photography the movement of one lens attained the object in view, yet the simultaneous motion of the two lenses, receding from or approaching a fixed point between them, was the only legitimate mode of reconciling practice with theory, and of securing in every plane an exact uniformity of image. To fulfil this condition was a difficult problem, the solution of which was most perplexing. But, says Claudet, with a determination which commands success, "I did not like that it should be said my plan was not entirely in accordance with the mathematical laws of optics, and I set to work to find a mechanical means by which I could avail myself of the calculations of Dr. Sommer. I have found such means; and it proves that the differential movement can be effected, not only as readily, but with a greater command and steadiness than by moving only one lens." His ingenious automatic arrangement is described in his last paper read before the Royal Society, in 1867, and published in the Proceedings, entitled "Optics of Photography: on a Self-acting Focus-Equalizer, or the means of producing the Differential Movement of the two Lenses of a Photographic Optical Combination, which is capable, during the exposure, of bringing consecutively all the Planes of a Solid Figure into Focus, without altering the size of the various images superposed."

After this, and in the same year, he had an interesting correspondence with his veteran collaborateur Sir David Brewster, who held that the most perfect photographic instrument is a single lens of least dispersion, and

least aberration, and least thickness. Claudet realized these views in his portraiture with a small topaz lens, which reached with equal distinctness every plane of the figure. He then communicated the nature and result of his experiments to the British Association at Dundee; and his work was done. His last illness, in December 1867, was of very brief duration. He suddenly passed away from us, in the 70th year of his age, while his mental powers retained the vigour and freshness of youth; and by his death photography lost a father, and very many photographers a friend.

The scientific life of Claudet is given at length in a "Memoir" published in the 'Scientific Review,' and reprinted for distribution at the Meeting of the British Association at Norwich in August 1868. In an Appendix there is a list of forty papers communicated from 1841 to 1867 to the Royal and other Philosophical Societies and to photographic and philosophical publications in England and France. Here also we have a striking portrait of this zealous photographer, obtained with his Focus-Equalizer, and printed from the only negative preserved when his "Temple to Photography" in Regent Street was destroyed by fire, "a few weeks after its chief priest had quitted it for ever."

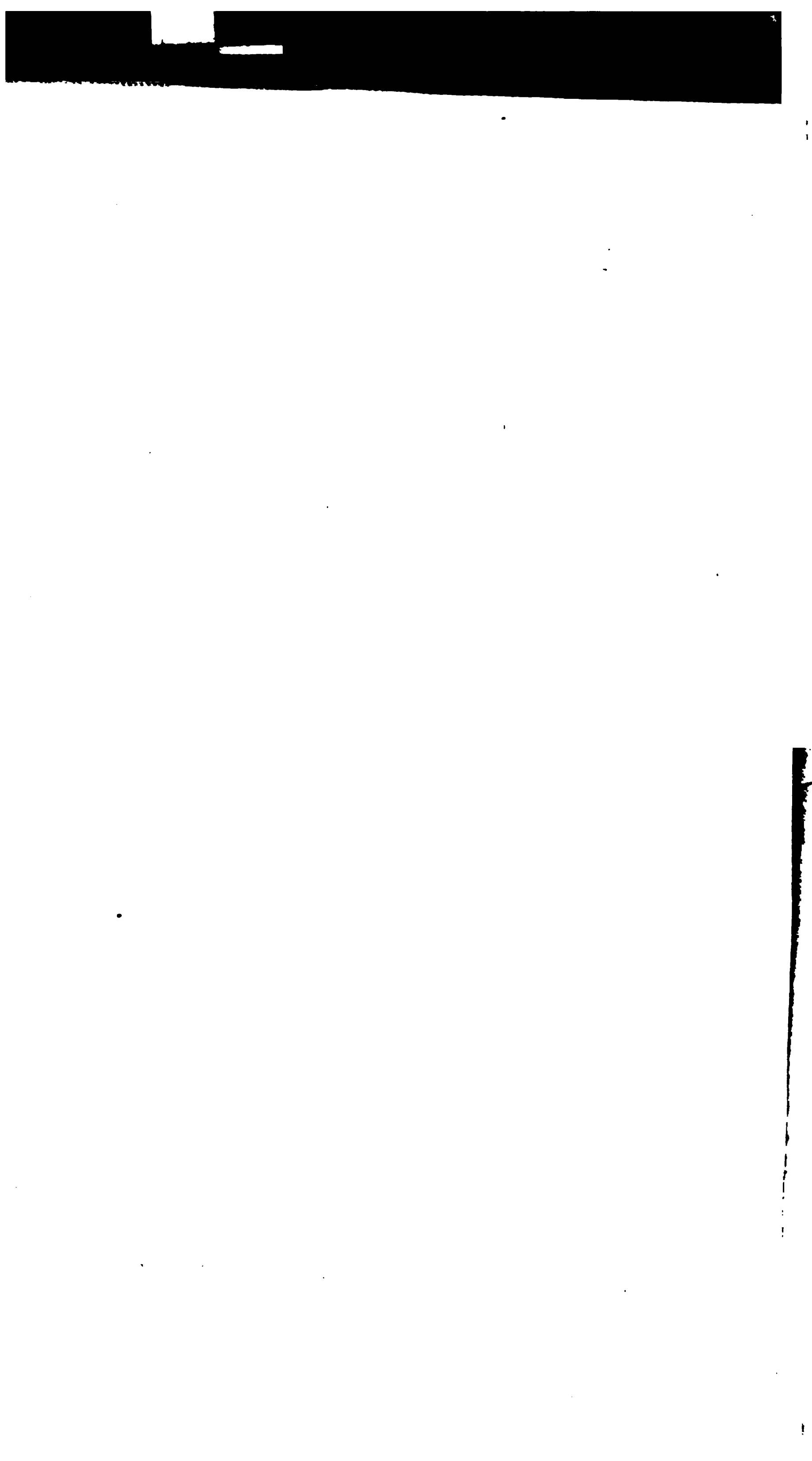
In recognition of his merits M. Claudet received awards of eleven medals, including the Council Medal of the Universal Exhibition, 1851, besides that, being on juries, on other great occasions he was excluded from the awards. He was elected a Fellow of the Royal Society in 1853, and in 1865 he was made a Chevalier of the Legion of Honour.—J. B. R.

CHARLES JAMES BEVERLY, F.R.S., F.L.S., was born in August 1788, at Fort Augustus in the Highlands, where his father's regiment was then quartered. He entered the Navy in 1810 as Assistant Surgeon, and was employed in that capacity during four years on the Baltic and Mediterranean stations, but chiefly the latter, in H. M. SS. 'Pyramus,' 'Resistance,' and 'Caledonia,' during which period he was frequently sent in boats on cutting-out expeditions, and was present at the capture of Porto d'Anzo in 1813. He was then placed on Lord Exmouth's list for promotion, but, falling into bad health, was sent to England in charge of sick and wounded from the fleet.

On his recovery he was appointed to the 'Tiber' as Assistant Surgeon, and served in that ship till 1818, when, upon a strong recommendation, he was selected by the Admiralty to be Assistant Surgeon in the 'Isabella,' then about to proceed to the polar regions under the command of Sir John Ross. In 1819 and 1820 he served in Sir Edward Parry's first expedition, and passed the winter at Melville Island, discovered in that well-known voyage. On his return he was promoted to the rank of Full Surgeon, having seen more than ten years' service in sea-going ships as Assistant Surgeon, and being highly commended for his skill and care in his attendance on the sick. He subsequently suffered from an affection of his eyes, and immediately on his recovery was nominated most unexpectedly

to the Flagship on the Barbadoes Station as Supernumerary Surgeon. The risk of changing from an arctic to a tropical climate while in weak health forced him to decline the appointment, and he was removed from the list of surgeons. He served in 1827 as a volunteer under Sir Edward Parry in the capacity of Surgeon and naturalist in the long and perilous ice-journey on the Spitzbergen seas. He was elected a Fellow of the Royal Society in May 1831.

After retiring from the Navy, Mr. Beverly entered into private practice in London. He died on the 16th of September, 1868, a short time after attaining the age of 80.



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